WINTER/SPRING 1983

NUMBER 7

Los Alamos Science

LOS ALAMOS NATIONAL LABORATORY

" . . . When you come right down to it the reason that we did this job is because it was an organic necessity. If you are a scientist you cannot stop such a thing . . . . You believe that it is good to find out how the world works . . . [and] to turn over to mankind at large the greatest possible power to control the world and to deal with it according to its lights and its values.

" . . . I think it is true to say that atomic weapons are a peril which affect everyone in the world, and in that sense a completely common problem . . . . I think that in order to handle this common problem there must be a complete sense of community responsibility.

" . . . The one point I want to hammer home is what an enormous change in spirit is involved. There are things which we hold very dear, and I think rightly hold very dear; I would say that the word democracy perhaps stood for some of them as well as any other word. There are many parts of the world in which there is no democracy . . . . And when I speak of a new spirit in international affairs I mean that even to these deepest of things which we cherish, and for which Americans have been willing to die—and certainly most of us would be willing to die—even in these deepest things, we realize that there is something more profound than that; namely the common bond with other men everywhere . . . ."

J. Robert Oppenheimer
speech to the Association of Los Alamos Scientists
Los Alamos
November 2, 1945

Excerpts from a speech to the Association of Los Alamos Scientists in Los Alamos, New Mexico, on November 2, 1945. Reprinted with permission from an original document in the Papers of the Federation of American Scientists, Box 21, Folder 4, Department of Special Collections, University of Chicago Library.

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THE EVOLUTION OF THE LABORATORY

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Address mail to
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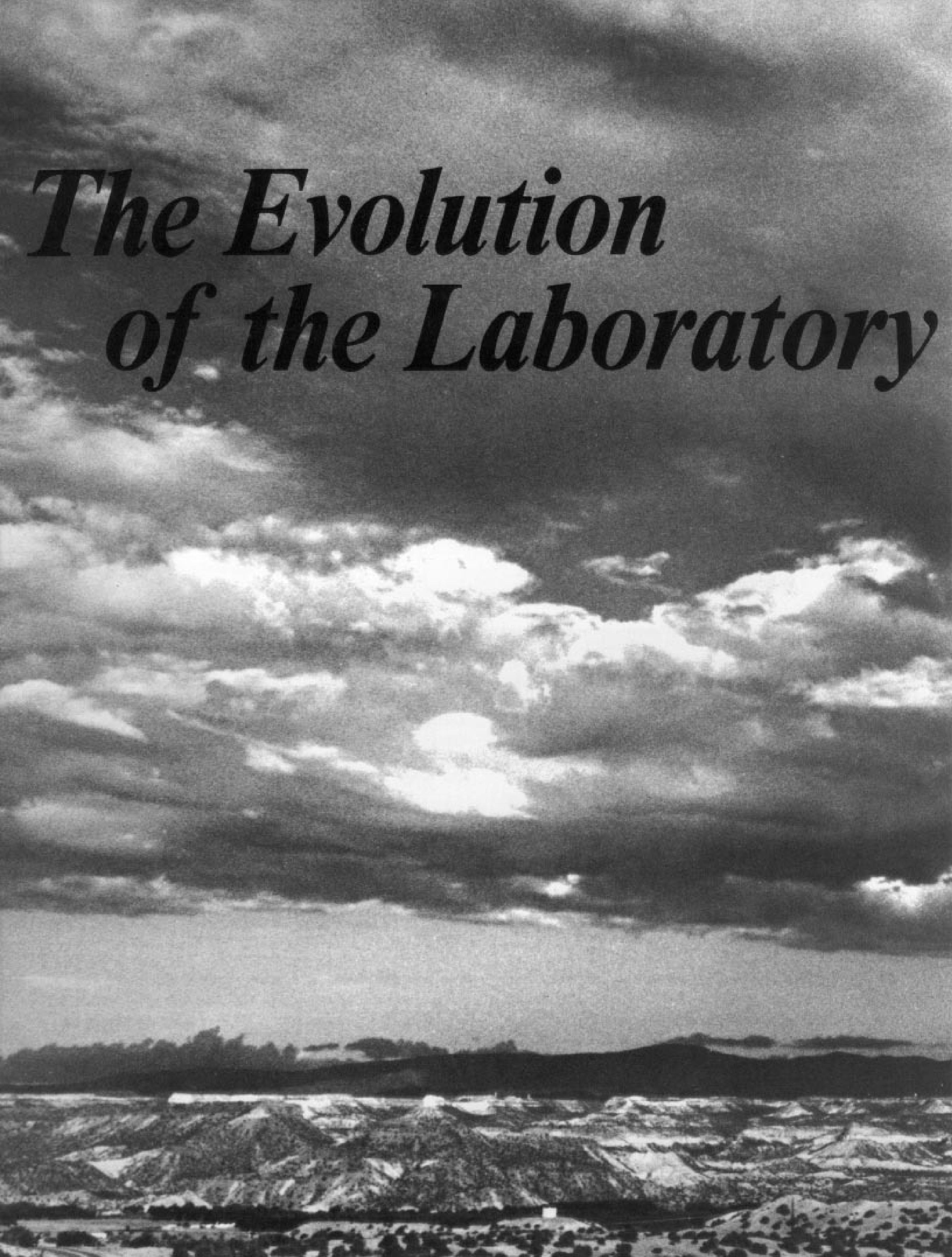
The Evolution of the Laboratory



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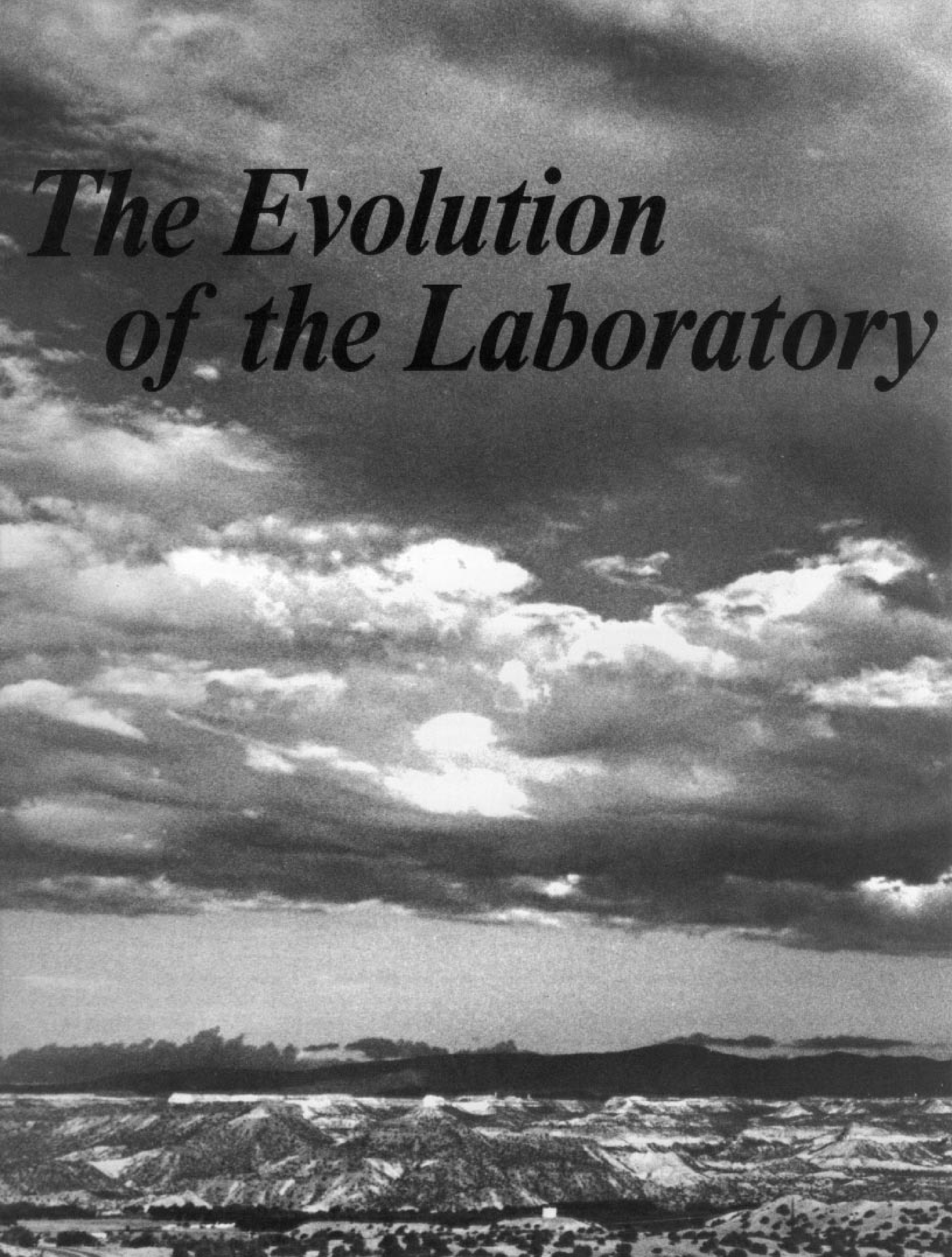


THE OPPENHEIMER YEARS

1943-1945

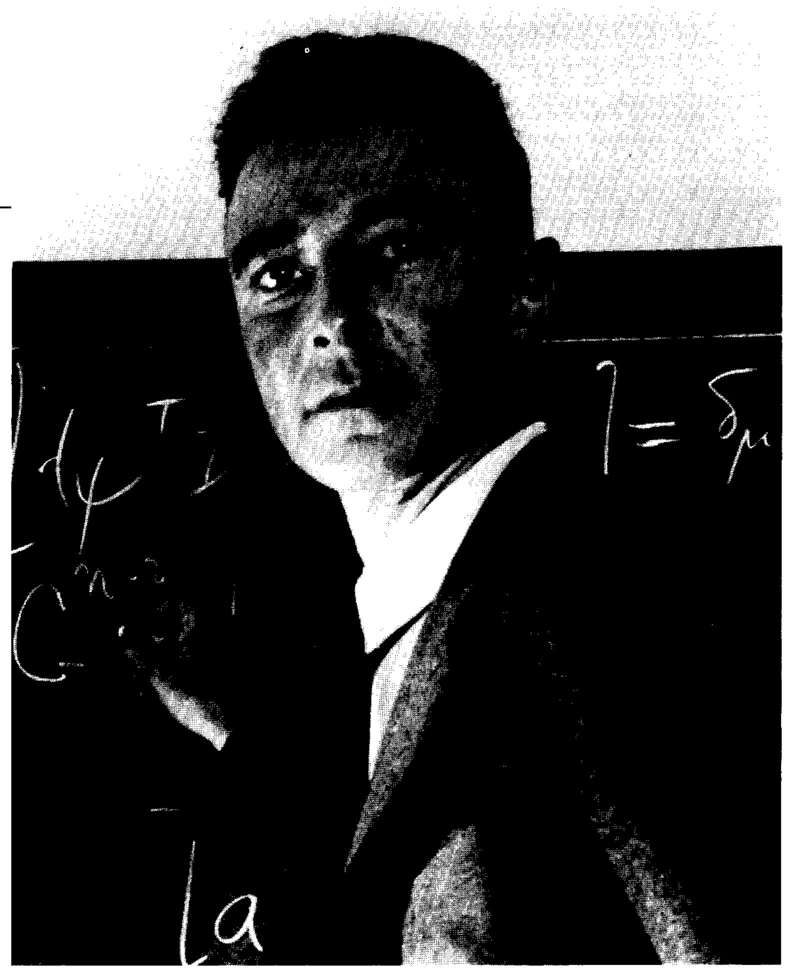


Photo courtesy of the J. Robert Oppenheimer Memorial Committee

" . . . I think surely if I were asked to do a job I could do really well and that it needed doing, I'd not refuse. "

Robert

Berkeley, 1941

Reasons for project

The first step toward a more concerted program of bomb development was the appointment, in June 1942. of J. Robert Oppenheimer from the University of California as Director of the work. By October of 1942, it had been decided that the magnitude of the difficulties involved made necessary the formation of a new project. Even the initial work of providing nuclear specifications for the bomb was seriously hampered by the lack of an organization united in one locality: it was clear that without such an organization the ordnance work would be impossible.

David Hawkins, "Manhattan District History: Project Y," Los Alamos Laboratory report LAMS-2532 (1946), Chapter L

“What is wrong with us?”

September 21, 1942

These lines are primarily addressed to those with whom I have shared for years the knowledge that it is within our power to construct atomic bombs. What the existence of these bombs will mean we all know. It will bring disaster upon the world if the Germans are ready before we are. It may bring disaster upon the world even if we anticipate them and win the war, but lose the peace that will follow. .,

We may take the stand that the responsibility for the success of this work has been delegated by the President to Dr. Bush. It has been delegated by Dr. Bush to Dr. Conant. Dr. Conant delegates this responsibility (accompanied by only part of the necessary authority) to Compton. Compton delegates to each of us some particular task, and we can lead a very pleasant life while we do our duty. We live in a pleasant part of a pleasant city [Chicago] in the pleasant company of each other, and have in Dr. Compton the most pleasant “boss” [at the Metallurgical Laboratory] we could wish to have. There is every reason why we should be happy, and since there is a war on, we are even willing to work overtime.

Alternatively, we may take the stand that those who have originated the work on this terrible weapon and those who have materially contributed to its development have, before God and the World, the duty to see to it that it should be ready to be used at the proper time and in the proper way.

I believe that each of us has now to decide where he feels that his responsibility lies.

L. Szilard

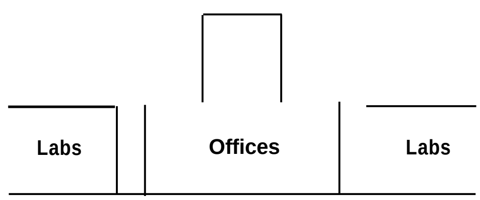
Logistics

Metallurgical Laboratory
October 12, 1942

Dr. J. R. Oppenheimer
Le Conte Hall
University of California, Berkeley

Dear Oppy:

I enclose two copies of the material submitted to Stone and Webster [Boston architects for initial planning of facilities at Project Y] on Saturday. The plot plan submitted was essentially like the sketch I sent you except that two schemes for the office building were turned in, Scheme A looks like this . .



and Scheme B looks like this . . .

Jackson (University architect) will prepare the more detailed study plan here so that we can keep in close touch with him. . . . Do you see any harm in letting some of our group know about these plans? . . .

Has anyone considered thorium for our purposes?

Sincerely yours,

John H. Manley

Los Alamos, N. Mex. SPELA November 25, 1942

SUBJECT: Acquisition of land for Demolition Range, Los Alamos, New Mexico. The Commanding General, Services of supply.

1. There is a military necessity for the acquisition of land indicated under subject above and described more in detail in paragraph 2 below:

2. Description of land and other pertinent data are as follows:

a. BRIEF DESCRIPTION OF THE LAND: The area is located near Santa Fe and within Sandoval County, New Mexico, as shown in blue on the enclosed map.

b. PROPOSED USE: The land is required for the establishment of a Demolition Range.

c. ACREAGE INVOLVED: approximately 54,000 acres. .

d. IMPROVEMENTS: One established boys school containing expensively constructed improvements and personality, altogether having a value of $246,600. , . .

e. ESTIMATED COST: $440,000 . . . .

Thomas M. Robins,
Major General
Assistant Chief of Engineers

February 8, 1943

Mr. R. M. Underhill
Secretary to the Regents
University of California, Campus

Dear Mr. Underhill:

At your suggestion I am writing to ask for permission to waive in certain cases the University rules which forbid the employment of a man and his wife in the same department of the University. The reason for this request is that in the work on our new project we shall be in an isolated community where it will be difficult to procure the services of secretaries, stenographers, technicians, librarians, etc. Furthermore, it will be a great help . . . from the point of view of, reinforcing the morale of our people to allow those women who are qualified and experienced to work. . . . In addition, there are a few cases where a man and his wife are both trained physicists, and it would be a great waste for us if we had to exclude one or the other. . . .

Very sincerely yours,

Robert Oppenheimer

Robert Oppenheimer

February 19, 1943

Professor Robert Oppenheimer
Radiation Laboratory, Campus

Dear Professor Oppenheimer:

Mr. Underhill has referred to me your letter of February 8. . . .

I am quite willing to relax this rule in isolated communities. .

Yours sincerely,
Robert G. Sproul

Within the meaning of the Espionage Act, the contents of this document are not to be discussed....You may discuss them with your wife if she accepts these limitations in all strictness. .

MEMORANDUM OF THE LOS ALAMOS PROJECT

We know you will want to have as clear a picture as possible, before coming to Los Alamos. of the many aspects of life here. . . It is set in the pines at 7300 feet in very tine country. .

The country is a mixture of mountain country such as you have met in other parts of the Rockies, and the adobe-housed, picturesque, southwest desert that you have seen in Western Movies. . . .

Rent for furnished. equipped single rooms including utilities is $13.00 a month. Room service is $2.00 extra a month.

Rents for unfurnished apartments of all sizes are based on salaries and not on space occupied and are as follows:

Less than $2600	$17.00 a month
$2600-3100	23.00 " "
3100-3400	29.00 " "
3400-3800	34.00 " "
3800-4400	42.00 " "
4400-5200	50.00 " "
5200-6000	59.00 " "
Over 6000	67.00 " "

Persons now under OSRD contract will be paid the same amount without subsistence allowance.

Persons not now holding an academic position but who were in academic work will be paid according to the following schedule:

BS	$200
MS or BS plus 1 yr. education or experience	220
MS plus 1 yr. or BS plus 2 yrs.	240
MS plus 2 yrs. or BS plus 3 yrs.	260
PhD or MS plus 3 yrs. or BS plus 4 yrs.	280
PhD plus 1 yr.	305
PhD plus 2 yrs.	330
PhD plus 3 yrs.	355
PhD plus 4 yrs.	380
PhD plus maximum (Maximum of this scale)	400

Under a recent ruling of the War Manpower Commission, it is necessary to classify employees according to their duties and to freeze the wage range of each class of employees. The range for our technicians is $185.50 to $300.00 per month.

At Los Alamos

NOTES ON MEETING
March 6. 1943

Steering Committee: There was some discussion of the frequency of meetings of the whole planning committee. Dr. Oppenheimer said about once a month. Dr. Condon felt it should meet one night a week. Dr. Serber questioned the need of a steering committee. Dr. Oppenheimer felt that a planning committee of seventeen people could not act. He said. "We have one great problem of secrecy. I take it very seriously, If we muff it, we will get clamped down on so completely that a lot of us will leave, and the rest will work under conditions that they won't like at all. . I have asked Groves that a man from G-2 be assigned to us.'" . .

April Conference: Dr. Oppenheimer asked for opinions on the question of inviting to the conference men who were not definitely committed to Los Alamos. It was agreed that Fermi should come. Dr. Oppenheimer said that Rabi was not willing to join the project. but that he had said. "YOU can have half of my time free of charge in anything useful I can do.'" Dr. Oppenheimer said he would also like Feynman and all the theorists. . I and] that he did not want either Groves or Conant present: . . it was agreed that the meeting was scientific and completely independent of the administrative work. . . .

The Conference, 15-24 April 1943

OUTLINE OF PRESENT KNOWLEDGE
[J. Robert] Oppenheimer

Materials and Schedules: . . . The isotope 25 [^{235}U] will support a chain reaction because neutrons of all energies can cause fission in it and because there are no known competing processes. . . It has been shown that there is no appreciable fraction of neutrons delayed by more than 10–5 sec. It (25) is being produced in two ways.

Lawrence's group [Berkeley] is separating the isotope 25 by mass spectrographic means. It is planned to have 500 tanks of two each installed by January 1, 1944. [t is expected that each arc will give 100 milliamps of 28 [^{238}U] and 3 milliamps of enriched beam.

Urey's group is separating 25 by a diffusion process [Columbia University]. . .

The element 49 [^{239}Pu] is produced from 28 by the absorption of neutrons. The material is to be produced on a large scale by the Chicago pile. 300 gms per day is hoped for by Jan. 1945.

Isotope 23 [^{233}U] can be produced by putting thorium around a pile. The yield is small, 5% of 49, for a carbon pile. The yield would be 20% for a deuterium pile.

Energy Release: The destructive effect of the gadget is due to radiative effects and the shock wave generated by the explosion. . The shock wave effect seems to extend over the biggest area and would be, therefore, most important. The area devastated by the shock wave is proportional to the 2/3 power of the energy release and may be simply calculated by comparing the energy release with that of TNT. If the reaction would go to completion, then 50 kg of 25 would be equivalent to 10 tons of TNT. Actually it is very difficult to

There was . . . a weekly Colloquium which all staff members were privileged to attend . . . [and which] was less a means of providing information than of maintaining the sense of common effort and responsibility . . . [This] policy adopted concerning communication represented a considerable departure from the customs normally surrounding the protection of military secrets. Hawkins, "Project Y," Chapter III.

obtain a large percentage of the potential energy release.

Detonation: The second major difficulty facing us is connected with the question of detonation. . It is important that no neutron should start a premature chain reaction. . . Possible sources of neutrons are 1) Cosmic ray neutrons . . . and 2) Spontaneous fission neutrons. . .

EXPERIMENT RESULTS AND DESCRIPTION OF AVAILABLE EQUIPMENT
John Manley

Experimental Nuclear Research Facilities: . . . We shall have a cyclotron, obtained from Harvard, which should give us about 50 µa of 10 MeV deuterons. . . .

Two pressure Van de Graaffs have been obtained from Wisconsin. . . .

Illinois has loaned us a Cockcroft-Walton outfit which when used as a D-D [deuteron-deuteron] source, delivers 300 µa of 0.3 MeV deuterons producing some 10^8 n/see.

Neutrons may also be produced from chain reactions. Fermi's pile operates conveniently at 100 watts, at which power it gives 10]3 n/see or about 5 x 1O^{5}n per sec per cm*. These include both fast and thermal neutrons. . . .

The natural source situation is not completely clear. but we are obtaining from Chicago the following sources: 200 mc pressed Ra-Be mixed source, yielding 2 x 10^6 n/see; 500 mc RdTh for a photo source which should yield about 5 x 10^6 n/see of .9 MeV with Be; 2000 mc pressed Ra-B mixed source, yielding about 5 x 10^6 n/see. . .

THE CHAIN-REACTING PILE
[Enrico] Fermi

The first chain reacting pile was built in the fall of 1942. It contained 6 tons of metal, 40 tons of oxide, and 400 tons of graphite. The shape was a sphere of 26' diameter with the best materials in the center. . . . This first chain reaction was obtained on December 2, 1942. . . .

The present chain reacting pile is designed for convenient performance of experiments. Its dimensions are 20' x 22' x 18' and it has a removable 33" section in the center. It is shielded to a factor 10^4- 10^5 by a 5' concrete wall. On top, a 6' graphite column for a source of thermal neutrons projects through the shield.

The pile has two types of uses. First it is a relatively intense and very stable source of neutrons. The intensity can be controlled within 0.1%. . . .

The other main use of the pile is to measure changes in the critical position of the control rod due to insertion of various materials in the pile. This is especially useful for rapid determination of absorption cross sections.

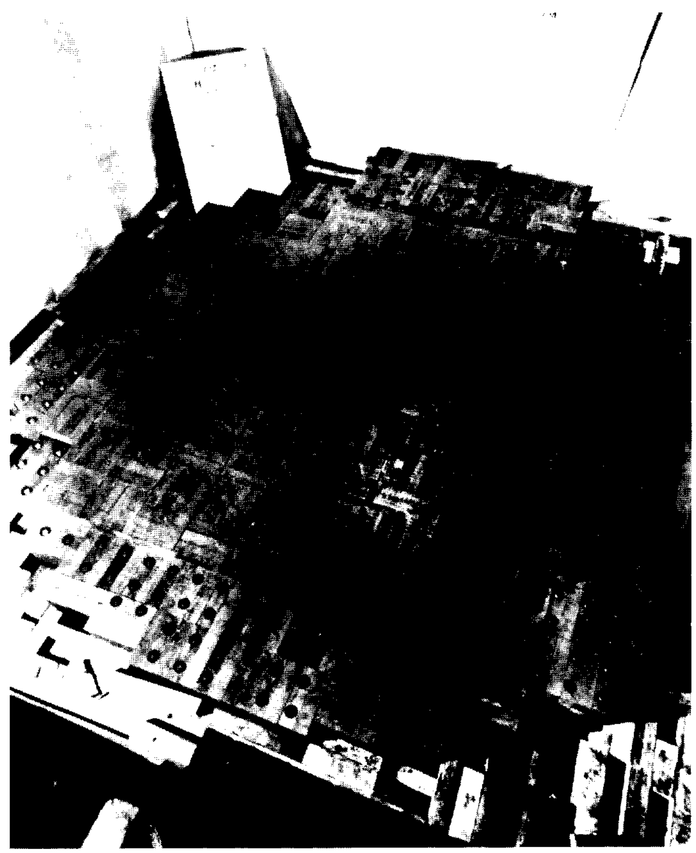
EXPECTED DAMAGE OF THE GADGET
[Hans] Bethe

Comparison with TNT: The most striking difference between the gadget and a TNT charge is in the temperatures generated. The latter yields temperatures of a few thousand degrees whereas the former pushes the temperature as high as [tens of millions of degrees]. . . .

The actual damage depends much on the objective. Houses begin to be smashed under shocks of 1/10 to 1/5 of an atmosphere. For objects such as steel supported buildings and machinery, greater pressures arc required and the duration of the shock is very important. If the duration of the pressure pulse is smaller than the natural vibration period of the structure, the integral of the pressure over the duration T of the impulse is significant for the damage. If the pulse lasts for several vibration periods. the peak pressure is the important quantity. . . .

Other Damage: The neutrons emitted from the gadget will diffuse through the air over a distance of 1 to 2 km, nearly independent of the energy release. Over this region, their intensity will be sufficient to kill a person,

The effect of the radioactive fission products depends entirely on the distance to which they are carried by the wind. If 1 kg of fission products is distributed uniformly over an area of about 100 square miles, the radioactivity during the first day will represent a lethal dose (=500 R units): after a few days, only about 10 R units per day are emitted, If the material is more widely distributed by the wind, the effects of the radioactivity will be relatively minor.



The chain-reacting pile at Chicago.

Day-to-day operations

July 29, 1943.

Dr. J. R. Oppenheimer
P. O. Box 1663
Santa Fe. New Mexico

Dear Dr. Oppenheimer:

. . . It is requested that:

(a) You refrain from flying in airplanes of any description; the time saved is not worth the risk.

(b) You refrain from driving an automobile for any appreciable distance (above a few miles) and from being without suitable protection on any lonely road, such as the road from Los Alamos to Santa Fe. . . .

(c) . . In driving about town a guard of some kind should be used, particularly during hours of darkness. The cost of such guard is a proper charge against the United States.

I realized that these precautions may be personally burdensome. . .

Sincerely,

L. R. Groves
Brigadier General, C. E.

LIAISON WITH X [Oak Ridge]

Dear General Groves,

I enclose the list of questions you requested. The list is not exhausted—I am. You surely know that one cannot think of or ferret out all the pertinent questions. . .

1 am not able to understand your feeling that whoever tried to act as liaison for this project would be in any sense competing with you. I should certainly not want to have any part in such a duty if this feeling exists. . . .

As to the nature of the questions, I have endeavored to ask only those which have a direct and immediate bearing on the program here. Two examples will serve to illustrate:

1. We cannot properly assign a given small quantity of 49 among the numerous experimental uses without knowing when and how much will arrive later.

2. We cannot specify the amount of polonium required for a certain application if 49 production could compete unless the details of both polonium and 49 production are known, so that relative production costs (time, chiefly) can be weighed against physical advantages and disadvantages. . . .

I hope that this execution of the task you assigned to me meets with your approval. . . . You cannot have been unaware that I left our conference on this subject with little conviction or enthusiasm for this task, except in so far as you considered it as a preliminary to what we regard as a necessary liaison.

Very truly yours,
J. H. Manley

September 18, 1943

Dr. R. G. Sproul, President
University of California, Berkeley

Dear President Sproul:

At the time when the special project in New Mexico was opened, my salary was set by the University. . at $10,000 a year. . . .

In peacetime I was, both at the University of California and at California Institute of Technology, a professor of physics and not a director of anything. Thus my present salary exceeds by a little over $200 a month that which I would get if we applied our usual formula to my peacetime salaries. I think that neither the University nor I would want to regard work done for the Government of the United States in time of war as the occasion for any essential increase in income. and I am therefore suggesting that in the future my salary might be reduced in accordance with the procedure which we in general follow. . . .

Very sincerely yours,
J. R. Oppenheimer

September 30, 1943

Note to President Sproul:

As I told you yesterday in Los Angeles, I do not see any particular reason why the salary of Dr. J. R. Oppenheimer should be reduced. . . .

Robert M. Underhill
[Secretary of the Regents]

War Department
P. O. Box 2610
Washington, D.C.
20 June 1944

Dr. J. R. Oppenheimer
P. O. Box 1663
Santa Fe, New Mexico

My dear Dr. Oppenheimer:

This refers to your proposal to develop the one kilowatt water boiler for use as a strong source of neutrons for experiments at Y, as proposed orally to me last week. . . .

Our main and actually our sole interest at this time lies in procuring, at the earliest date possible, the necessary but small number of the final gadgets, properly designed and fabricated. . . .

From the teletype Fermi and Bacher appear to feel that the water boiler project will make such a contribution to the desired end. If you. . . feel the same way, then we should go ahead with the proposed project. . . .

Sincerely,
L. R. Groves
Major General, C.E.

"We are free to start things, free to go about them, but then the rock of what the world is really like limits and shapes this freedom."

J. Robert Oppenheimer

August 1944 reorganization

During the first six months of the Laboratory, the gun method of assembly was the focus of administrative and technical activities in the ordnance program. By February 1944 . . . sufficiently accurate calculations had been made so that, for the U^{235} gun, Group T-2 specified the actual bore. During the period to August 1944 the main focus of activity was the plutonium. gun. In the summer of 1944 . . . when the first Clinton plutonium made by chain reactor arrived-much more heavily irradiated than the previous samples made by cyclotron bombardment—the existence of Pu^{240} was verified, as was the fear that it might be a strong spontaneous fissioner. Neutron background in the plutonium which would be produced at full power was punched up into the region where, to prevent predetonation, assembly velocities would have to be much greater than those possible with the plutonium gun. The implosion was the only hope, and from current evidence a not very good one. It was decided to attack the problems of the implosion and with every means available. "to throw the book at it." Administratively, the program was taken out of the Ordnance Division and divided between two new divisions. One of these was to be concerned primarily with the investigation of implosion dynamics. the other primarily with the development of adequate HE [high explosives] components.

Hawkins, Project Y. Chapter IV,

August 14, 1944

R. F. Bacher
J. R. Oppenheimer

Organization of Gadget Division

I am sending you a directive on the functions of the Gadget Division and of its relations to other parts of the laboratory. . .

1. To develop methods and to apply them for the determination of the hydrodynamics of implosion. . . .

2. To conduct semi-integral and integral studies of the materials to be used in implosion gadgets from the point of view of their multiplication properties.

3. To be immediately responsible for the design specifications of the tamper [neutron reflector], active material. source, etc., to be used in implosion gadgets. . . .

4. To collaborate wherever possible in providing instrumentation for studying the problems of the Explosives Division.

. keep Captain Parsons promptly and fully informed. . . .

It is clearly appreciated by me that in undertaking at this late date the grave responsibilities of the direction of the Gadget Division you are in no way assuring me that the program for which you will be responsible can be successful within the short time limits set by our directives and by the war.

J. R. Oppenheimer

August 14, 1944

G. B. Kistiakowsky
J. R. Oppenheimer

Organization of Explosives Division

. .. I would like to formulate as follows the functions of the Explosives Division of which you are assuming the direction.

1. To investigate promising explosives, methods of initiation, boosting, detonation, etc. for implosion.

2. To develop methods for improving the quality of castings.

3. To develop lens systems and methods for fabricating and testing them.

4. To develop a suitable engineering design for the assembly. . .

5. To cooperate closely with the Gadget Division in providing the necessary charges for their investigations.

. . . keep Captain Parsons promptly and fully informed. . . . Feel free to present me with any problems in whose solution I could prove useful.

J. R. Oppenheimer

September 15, 1944

Captain W. S. Parsons, USN

Subject: Organization

Your thoughtful and considered memorandum on the subject of organization has focused attention on points which need to be clarified. On the whole my reaction to what you say is sympathetic.

1. I have always understood your position here as including responsibility and authority for the determination of the actual components of the weapon subject to the fact that these components must attempt to meet certain specifications imposed by physical requirements which can be defined only by physical and mathematical research. It has not been my intention to take the direct responsibility for this determination myself; I have neither the qualifications for, nor the intention of, doing so in the future. .

2. The kind of authority which you appear to request from me is something that I cannot delegate to you because I do not possess it. I do not in fact, whatever protocol may suggest, have the authority to make decisions which are not understood and approved by the qualified scientists of the laboratory who must execute them, . [and] I should not consider making a decision which was not supported by responsible and competent men in the laboratory. Therefore any authority which I might ask you to assume in connection with the conduct of your part of the work would have to be similarly qualified. . . .

Nothing that I can put in writing can eliminate this necessity. I will support decisions reached by you . . . as long as these decisions are reached after competent technical discussion and after the opinions of all vitally concerned have been given appropriate weight. I am not arguing that the laboratory should be so constituted, It is in fact so constituted, . .

J. R. Oppenheimer

W. S. Parsons

October 6.1944

Major General L. R. Groves
P. O. Box 2610
Washington, D. C.

Dear General Groves:

I am glad to transmit the enclosed report of Captain Parsons, with the general intent and spirit of which I am in full sympathy. There are a few points on which my evaluation differs somewhat from that expressed in the report and it seems appropriate to mention them at this time.

I believe that Captain Parsons somewhat misjudges the temper of the responsible members of the laboratory. It is true that there are a few people here whose interests are exclusively "scientific" in the sense that they will abandon any problem that appears to be soluble. I believe that these men are now in appropriate positions in the organization. . . .

Sincerely yours,
J. R. Oppenheimer

Leslie R Groves

"In every investigation, in every extension of knowledge, we're involved in action. And in every action we're involved in choice. And in every choice we're involved in a kind of loss, the loss of what we didn't do. We find this in the simplest situations. . . . Meaning is always obtained at the cost of leaving things out. . . . Impractical terms this means, of course, that our knowledge is always finite and never all encompassing. . . . This makes of ours an open world, a world without end."

J. Robert Oppenheimer

Final work

As the implosion program developed and the time schedule tightened, . . . functions were taken over by various interdivisional committees and conferences. Among the most important of these were the Intermediate Scheduling Conference under Captain Parsons, the Technical and Scheduling Conference, and the "Cowpuncher" Committee, . . . organized to "ride herd on" the implosion program. Both of the last named committees were under the chairmanship of S. K. Allison, former Director of the Metallurgical Laboratory, who arrived at Los Alamos in November 1944. In this shift from the single Technical Board to the more flexible structure of specialized committees, the Director had the advice not only of these committees, but also of certain senior consultants, notably Niels Bohr, I. I. Rabi, and C. C. Lauritsen, who served in the capacity of elder statesmen to the Laboratory. . . .

Early in March 1945 two new organizations were created with the status of divisions—the Trinity Project and the Alberta Project—one to be responsible for the test firing of an implosion bomb at Trinity, and the other to be responsible for integrating and directing all activities concerned with the combat delivery of both types of bombs.

Hawkins, Project Y, Chapter IX.

MEMORANDUM ON TEST OF IMPLOSION GADGET February 16, 1944

1. The implosion gadget must be tested in a range where the energy release is comparable with that contemplated for final use. . . . This testis required because of the incompleteness of our knowledge. Thus the reaction will proceed at a temperature unobtainable in the laboratory, which corresponds to energies at which nuclear properties are, and will probably remain, rather imperfectly known. Further, pressures under which the gadget will operate are likewise unobtainable in the laboratory and the information which we may obtain on the spacio-temporal distribution of the pressures will in all probability be not only imperfectly known to us, but somewhat erratic from case to case.

Various attempts have been made to propose an experimental situation which would enable a test of the kind mentioned above to be carried out under conditions so controlled that the energy release was small. . . . All present proposals seem to me unsatisfactory, at least in the sense that they cannot replace more realistic tests. The proposals which have been made are the following:

a. That the amount of active material used be so limited that the nuclear reaction proceeds over a matter of some 30 ± 15 [neutron] generations to give a readily detectable radio-activity or neutron burst, but no appreciable energy liberation.

b. That the reaction be limited by the thermal stability and increased time scale of excess hydrogenation.

c. That the reaction be limited with normal or excess hydrogenation by the addition of appropriate resonance absorbers which will quench the reaction at temperatures of the order of tens of volts.

As for the first of these proposals, . . . we do not now have, and probably will never have, information precise enough to predict an appropriate mass with any degree of probability. . . . This would involve, among other things, knowing the radius of the compressed core to within 5 per cent. Furthermore, it is doubtful whether one could approach this limited explosion by gradual stages with any certainty and without very numerous subcritical trials since there is no a priori assurance, and some a priori doubt, that the implosions will be reproducible to the extent required.

As for the second and third proposals, which have been advocated with eloquence by Dr. Teller, it appears at the present time extremely doubtful whether a sufficiently complete knowledge of the hydrodynamics and nuclear physics involved will be available to make these tests either completely safe or essentially significant. We should like to leave open at the present time the possibility that either these experiments or others not yet proposed may, some months from now, be capable of essentially unambiguous interpretation. . . .

4. . . . It is my decision that we should plan . . . an implosion . . . so designed that the energy release be comparable with that of the final gadget, but possibly smaller by as much as a factor of 10; . . . that no definite decision against more controlled experiments be made at the present time. . .; and that in the light of the above considerations, all methods which hold promise of giving reliable information about the hydrodynamics and nuclear physics of the implosion be pursued with greatest urgency. . . . It would appear to be very much less difficult to predict and interpret the dimensions and construction of a gadget releasing some thousands of tons of TNT equivalent in nuclear energy than to make the corresponding predictions for nuclear explosions whose energy release, though finite, is negligible.

J. R. O.

March 10, 1944

Brig. Gen. L. R. Groves
P. O. BOX 2610
Washington, D. C.

Dear General Groves:

. . . [In regard to] a containing sphere [Jumbo] for proof tiring, there were a number of points made which I should like to have down in the record. . . .

. . . It was not known to us whether it could be made in the form of a single sphere or would have to be built up from plates. Excluding the extra weight introduced by manholes and reinforcements, the weight of the sphere was given by us as 80 tons provided steel could be obtained of yield strength 60,000 psi or better. You expressed the conviction that individual castings in excess of a hundred tons would introduce very serious transportation problems which should be avoided if possible. . . .

We shall attempt to have a container fabricated and completely assembled by September so that it may play as useful a part as possible in the later stages of implosion development.

[J. Robert Oppenheimer]

December 22, 1944

K. T. Bainbridge
J. R. Oppenheimer

[Gadget Testing Using Water for Recovery and Control]

After the meeting Tuesday I had some further opportunity to discuss with General Groves and Dr. Conant the matter of water recovery at Trinity. I think the factors affecting this are well known to you, namely that we do not at the present time plan a test implosion with 25 and that water recovery with 49 looks like a most difficult and hazardous undertaking.

. . . Under these circumstances it seemed to all of us that no further plans should be made for water recovery at Trinity. . . .

J. R. Oppenheimer

May 18, 1945

Capt. W. S. Parsons
K. Bainbridge

Thank you very much for your tine cooperation in obtaining information concerning helicopters and blimps [for collecting air samples] in the TR [Trinity] program.

The rockets have worked out so well. . .we will proceed with the use of rockets only, and no further inquiries on blimps or helicopters will be required.

K. Bainbridge

April 17, 1945

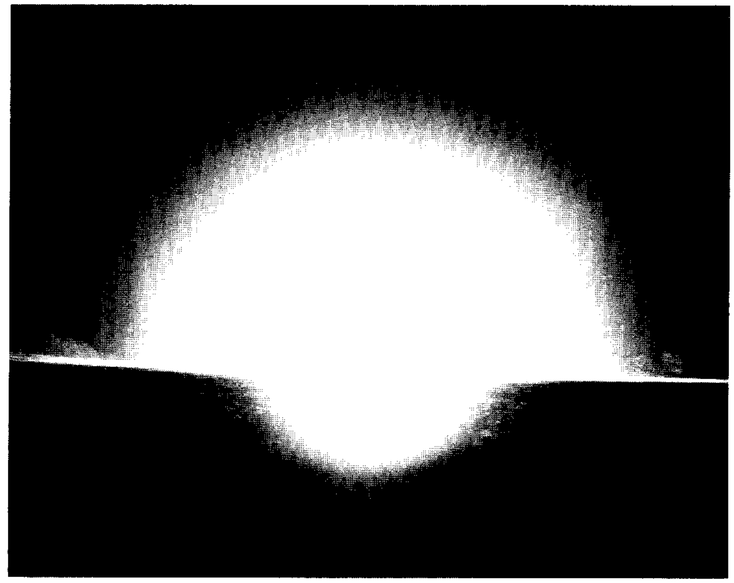
Mr. K. T. Bainbridge
H. L. Anderson

Plutonium Spiking for 100 T Shot

Please consider the advisability of adding 10 grams of plutonium to the active solution of the 100 T shot. Sugarman would like to try a plutonium as well as a fission product extraction from the dirt recovered after this shot.

H. L. Anderson

[The firing of 100 tons of TNT was used as a rehearsal test of blast effects, The stack of HE was provided with tubes containing 1000 curies of fission products derived from a Hanford slug to simulate at a low level of activity the radioactive products expected from the nuclear explosion. (Hawkins. Chapter X)].



The 100 T shot.

Dec. 15, 1944

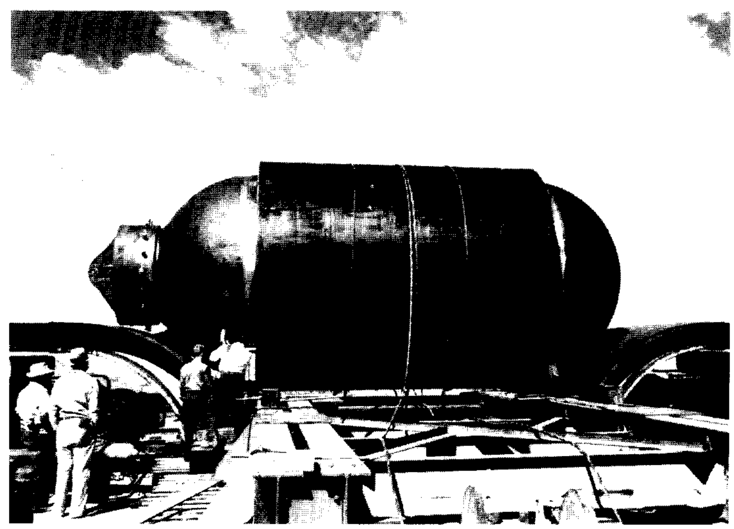
Mr. Carlson and Mr. Mack
Mr. Penney

The Heat of Combustion of Jumbo

. . . The energy needed to vaporize one gram of iron is between 300 and 400 calories. Taking the mass of Jumbo to be 220 tons, and the heat of detonation of HE to be 1000 calories per gram, it is seen that Jumbo cannot be vaporized if the energy released by the gadget is less than about 100 tons HE equivalent. . . .

If Jumbo is completely vaporized, there is a strong probability that the iron vapor will burn rapidly. and the energy thereby released will be right up in the front of the blast wave. The energy of combustion of one gram of solid iron at room temperature is about 1950 calories. Hence the HE equivalent of Jumbo is about 400 tons.

W. G. Penney



Jumbo was designed to withstand the explosion of HE and permit recovery of active material should the Trinity shot fail. It was not used.

July 11, 1945

Comdr. N. E. Bradbury
K. Bainbridge

Jumbo

Jumbo is a silent partner in all of our plans and is not dead yet, .

K. Bainbridge

K. Bainbridge

TR Hot Run

9 July 1945

Personnel Concerned
Comdr. N. E. Bradbury

TR [Trinity] Hot Run

The firm dates for the TR Hot Run are as follows [in part]:

Monday, 9 July, 0830 Schaffer Shake Test charge given eight-hour road test. Remove polar cap and dummy plug and inspect top of charge only after three hours riding.

Thursday, 12 July, 0830 Use two groups—one at V Site [shops] to assemble TR charge. . . .

Friday, 13 July, 0001 TR charge starts on its way to TR. G-2 escort cars fore and aft. G. B. Kistiakowsky to ride in fore car.

Friday, 13 July, 1300 Assembly at TR

- With jib hoist, remove polar cap and dummy plug. Special polar cap and funnel put in place. Gadget now belongs to tamper people (at about 1400 on Friday). Prior to their taking over, a fifteen minute period will be available for generally interested personnel to inspect the situation. After this time, only G engineers and two representatives from the assembly team will be present in the tent.
- Place in hypodermic needle *in right place.* (Note: check this carefully.)
- At this point another 15-minute period will be available for inspection . .
- Insert HE—this to be done as slowly as the G Engineers wish. Have on hand extra paper if charges are slightly small. Also grease and hypodermic needle grease gun. Be sure glass tape and/or shim stock shoe horn is on hand.
- Another inspection period of 15 minutes will be available.
- Leave tent in place till morning.

Saturday, 14 July, 0800 Lift to tower top

- Remove tent with main hoist.
- Lift sphere to tower top.

Saturday, 14 July, 0900 Operations aloft

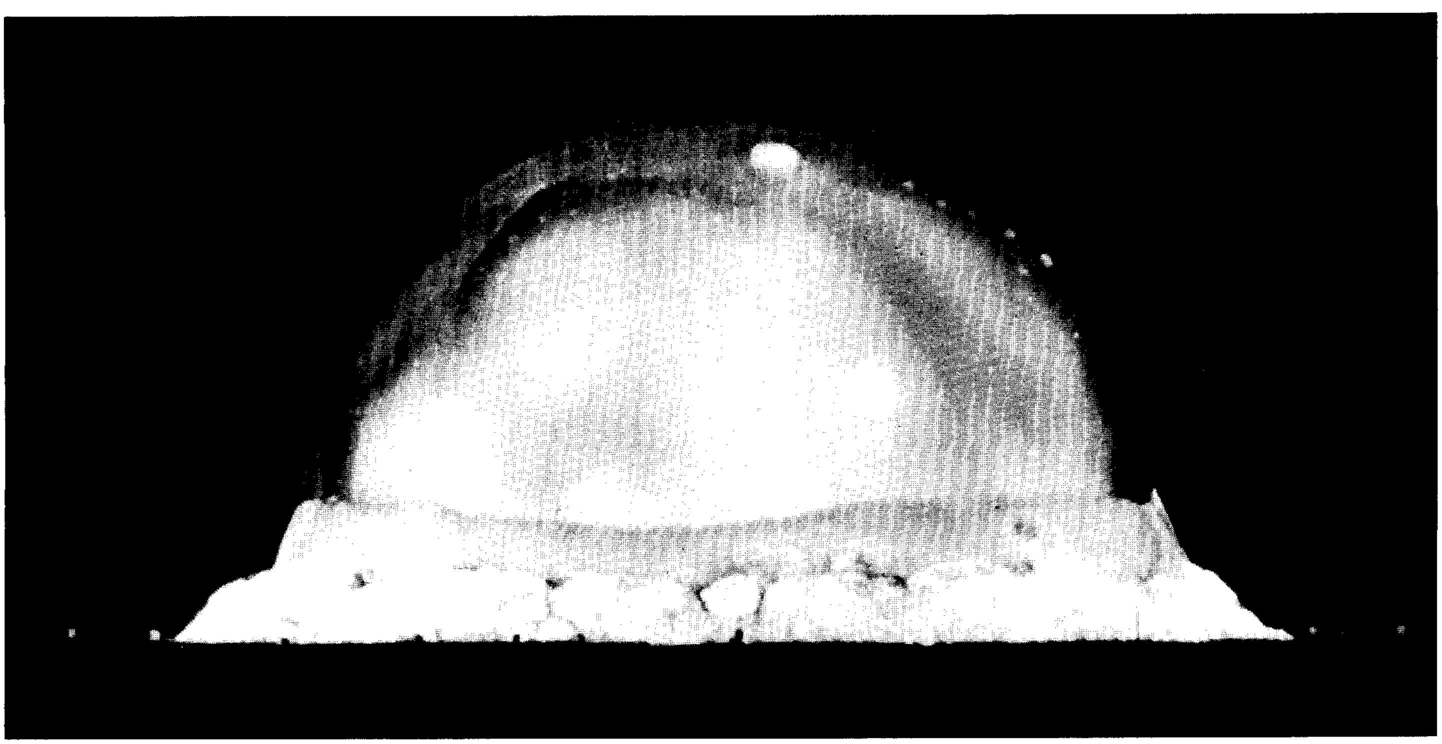
- Wiring of X unit proceeds . . . .
- Detonators are staked to co-ax . . . .
- X unit and informer unit safed—verified by Bradbury or Kistiakowsky....
- Note that once detonators are on sphere, no live electrical connection can be brought to X unit, informer unit, or anywhere else on sphere. Hence all testing must be done before sphere is lifted to tower. After that it is too late.

Saturday, 14 July, 1700 Gadget complete

Sunday, 15 July, all day. Look for rabbit's feet and four leafed clovers. Should we have the Chaplain down there? Period for inspection available from 0900-1000

Monday, 16 July, 0400 BANG!

N. E. Bradbury



"History of Project A"

[1945]

The history of Project A [Alberta] is essentially the history of the combat use of the ATOMIC BOMB and of the preparation and planning to make this use possible. . . . Project A as such was not established until March of 1945. However, . . . the first major activities . . . concerned with the delivery program began in June of 1943. . . . The only United States aircraft in which such a bomb [Pu^{239} gun assembly] could be . . . internally carried was the B-29. . . .

In the fall of 1943 . . . two external [bomb] shapes and weights were selected. . . . For security reasons these were called by the Air Forces representatives the "Thin Man" and "Fat Man" respectively—the Air Forces officers tried to make their phone conversations sound as if they were modifying a plane to carry Roosevelt (the Thin Man) and Churchill (The Fat Man). . . .

Tests with the modified aircraft and full scale dummy bombs were begun at Muroc on 3 March 1944. The negative results . . . thoroughly justified the holding of preliminary tests at such an early date. The fuses proved to be unreliable and the Fat Man . . . proved to wobble badly with its axis departing 20° from the line of flight. Although the B-29 release mechanism worked satisfactorily for the Fat Man, it failed completely for the Thin Man. . . .

Between the end of the first tests and June 1944 . . . it became apparent that Pu^{239} could not be used in a gun due to neutrons of Pu^{240} almost certainly causing a predetonation. . . .For U^{235} the gun velocity could be reduced . . . and the length of its bomb correspondingly. . . . This model finally acquired the appropriate name of Little Boy. . . .

Tests at Muroc were resumed in June of 1944, . . . The Fat Man models with their tails modified ., . still had an undampable wobble. As a desperate last resort Ramsey suggested a drop be made with internal 45° baffle plates welded into the inside of the shroud. . . . To everyone's surprise this modification was successful . . . the ballistic coefficient being improved rather than decreased as anticipated. . . .

The first tests [with a combat unit] began at Wendover [code name, "Kingman"] in October 1944. . . . tests which continued intermittently, then monthly, and finally almost continuously up to August of 1945. . . .

The chief design activities during this period were . . . the exact design of the tamper sphere, incorporation of. . . a trap door assembly. . . . etc.

The unfortunate failure of the Raytheon Company to meet its delivery schedule on X-Units (electrical detonators) added markedly to the difficulty of the test program. . . . It was not until the end of July that sufficient X-units had been tested to confirm their safety with HE: the first HE filled Fat Man with an X-unit was tested at Wendover 4 August, . . .[another] at Tinian [the overseas base] 8 August, and the first complete Fat Man with active material was dropped on Nagasaki 9 August.

On 26 July the U^{235} projectile for the Little Boy was delivered by the cruiser Indianapolis. The U^{235} target insert arrived in three separate parts in three otherwise empty Air Transport Command C-54's during the evenings of 28 to 29 July. . . . Although the active unit was completely ready in plenty of time for a 2 August delivery, the weather was not. Finally on the morning of 5 August we received word that the weather should be good on 6 August. ., . The progress of the mission is best described in the log which Capt. Parsons kept during the flight.

N. F. Ramsey

N. F. Ramsey

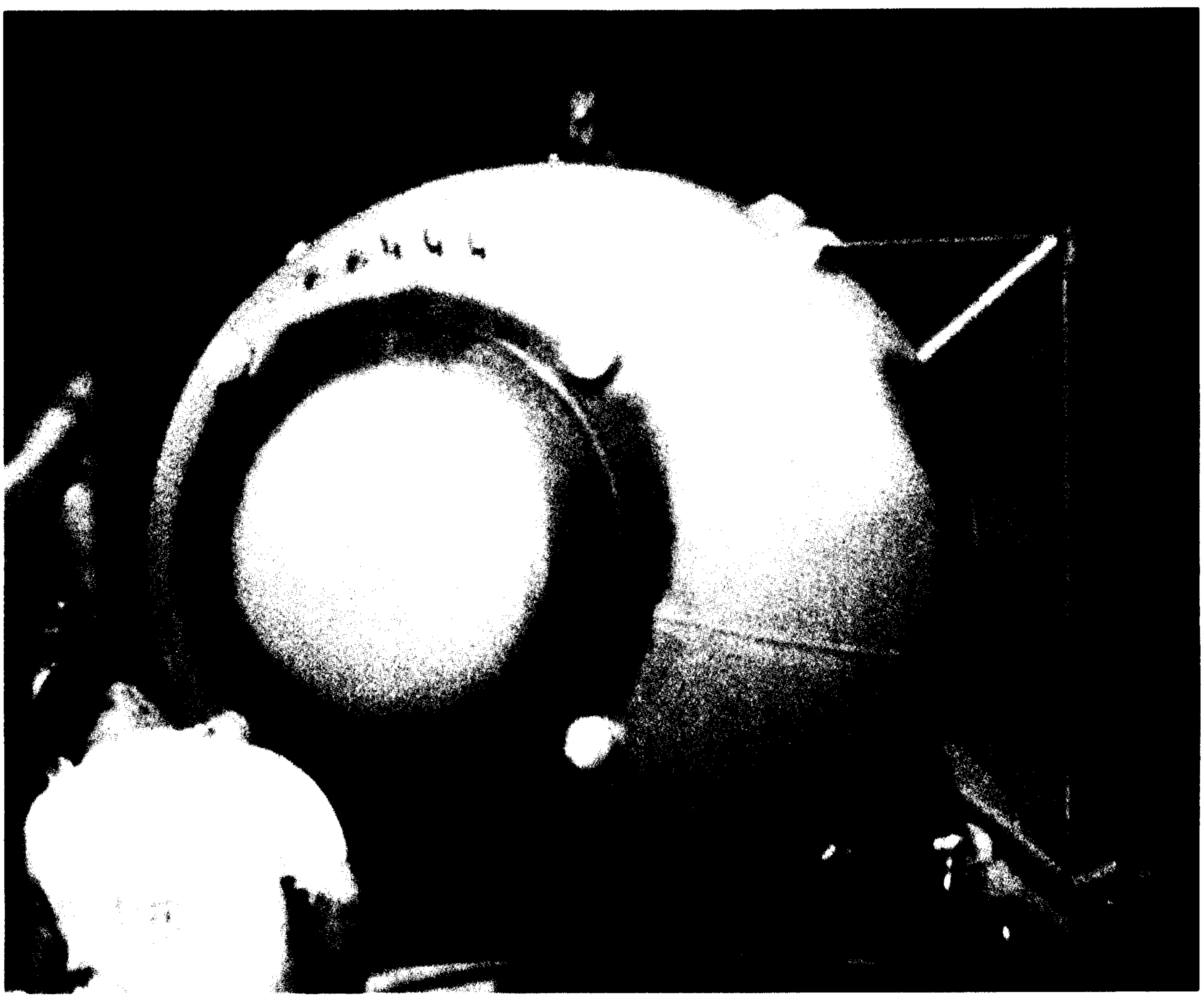
With the exception of three italicized quotations, the material for "The Oppenheimer Years" was drawn directly from the archives and from the report library of Los Alamos National Laboratory.

". . . I think surely . . ." is a line from a letter written by Robert Oppenheimer to William A. Fowler shortly after the latter left California Institute of Technology to serve as assistant director of research for the National Defense Research Committee. Reprinted by permission from Robert Oppenheimer: Letters and Recollections, ***edited by Alice Kimball Smith and Charles Weiner (Cambridge: Harvard University Press, 1980), p. 215.***

The quotations "We are free to start things . . ." and "In every investigation . . ." are excerpts from a talk given by Robert Oppenheimer at the University of Colorado, June 6, 1961. The talk was published under the title "Reflections on Science and Culture" in the Colorado Quarterly, ***Vol. 10, No. 2, 101-111 (Autumn 1961). Reprinted by permission.***

6 August 1945

0245	Take off
0300	Started final loading of gun
0315	Finished loading
0605	Headed for Empire from Iwo
0730	Red plugs in (these plugs armed the bomb so it would detonate if released)
0741	Started climb
	Weather report received that weather over primary and tertiary targets was good but not over secondary target.
0838	Leveled off at 32,700 feet
0847	All Archies (electric fuses) tested to be O.K.
0904	Course west
0909	Target (Hirsohima) in sight
0915	Dropped bomb (Originally scheduled time was 0915)
	Flash followed by two slaps on plane. Huge cloud
1000	Still in sight of cloud which must be over 40,000 feet high
1003	Fighter reported
1041	Lost sight of cloud 363 miles from Hiroshima with the aircraft being 26,000 ft. high.



It is with appreciation and gratefulness
that I accept from you this scroll
for the Los Alamos Laboratory, and for the men and women
whose work and whose hearts have made it.
It is our hope that in years to come we may look at the scroll
and all that it signifies, with pride.

Today that pride must be tempered by a profound concern.
If atomic bombs are to be added as new weapons
to the arsenals of a warring world,
or to the arsenals of the nations preparing for war,
then the time will come when mankind will curse
the names of Los Alamos and Hiroshima.

The people of this world must unite or they will perish.
This war that has ravaged so much of the earth, has written these words.
The atomic bomb has spelled them out for all men to understand.
Other men have spoken them in other times,
and of other wars, of other weapons.
They have not prevailed.
There are some misled by a false sense of human history,
who hold that they will not prevail today.
It is not for us to believe that.
By our minds we are committed, committed to a world united,
before the common peril, in law and in humanity.

J. Robert Oppenheimer
Acceptance Speech, Army-Navy “Excellence” Award
November 16, 1945

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 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.

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.

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. Operation 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 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.

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.

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 gentleman 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 involved 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 tit 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 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.

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.

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.

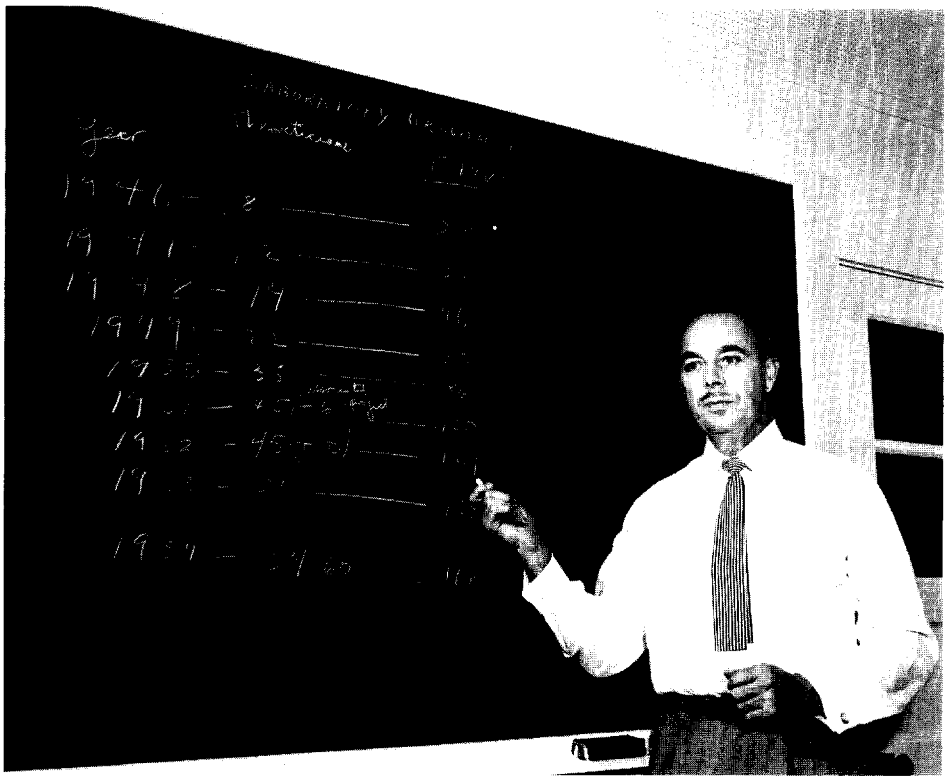
The Russian test caused a number of people, most of them not at Los Alamos, to feel that the nation was now in peril and must make a strong and tremendously impressive response to the terrible misdeed of the Russians. Teller, Lawrence, Alvarez, Lewis Strauss, Senator MacMahon, and Air Force Secretary 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.

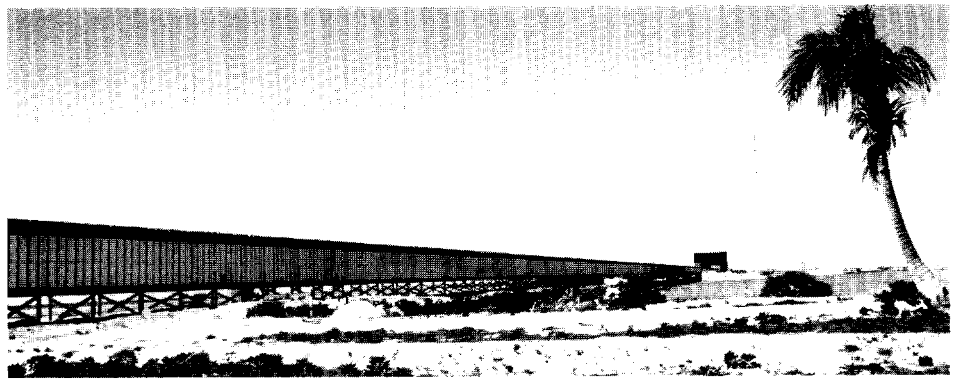
To return to the technical story, on the theoretical side we tried to calculate how thermonuclear reactions might possibly proceed. taking into account this effect or that effect that had been ignored before. There were also gaps in what was known about the neutron and thermonuclear cross sections, and, 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.

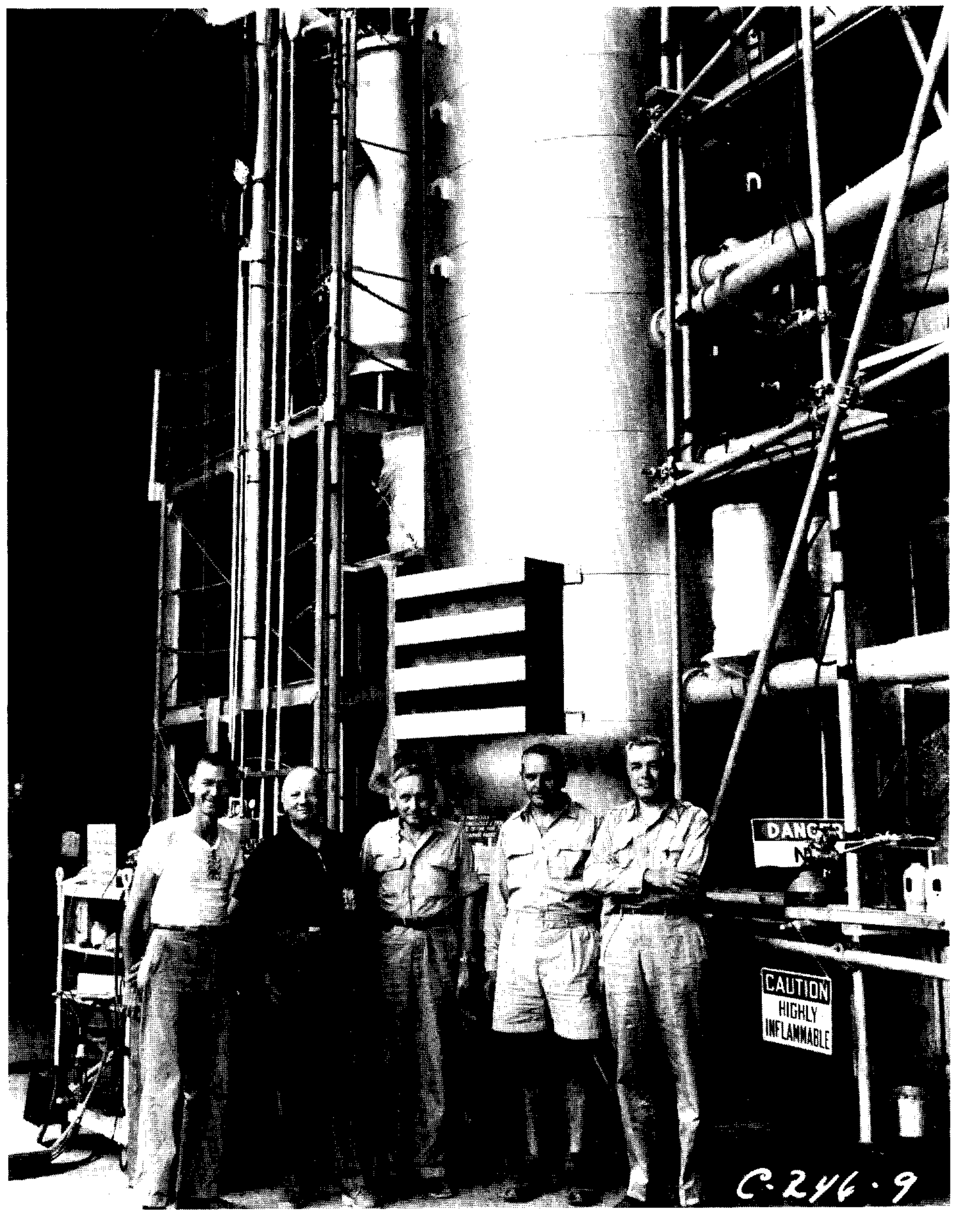
Between January 1950 until the end of

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 tit into missiles.

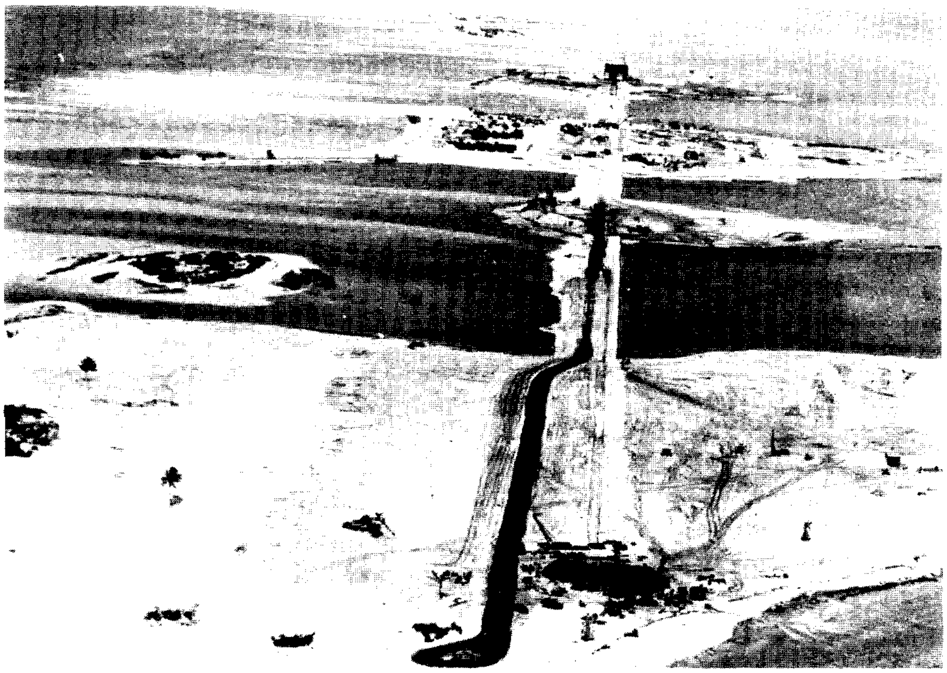
COWAN: During this period following the Russian test, we were also involved in an accelerated program for testing small fission devices. which, by the way, was done at the Nevada Test Site in 1951.

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 halfway 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 U-NIVAC 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.

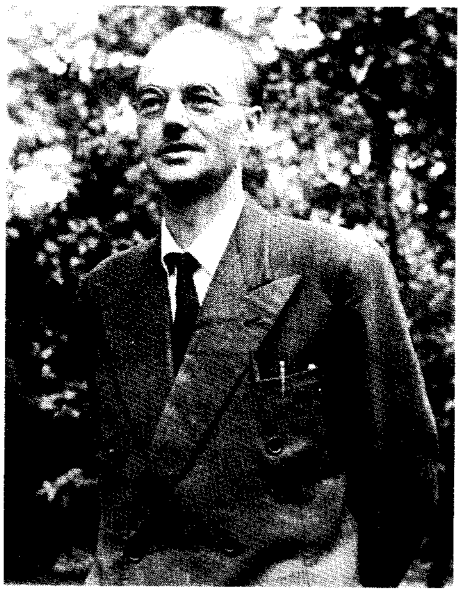
MARK: So a stream of salesmen from all the computing manufacturers began to beat a track to the door.

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 bad 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.

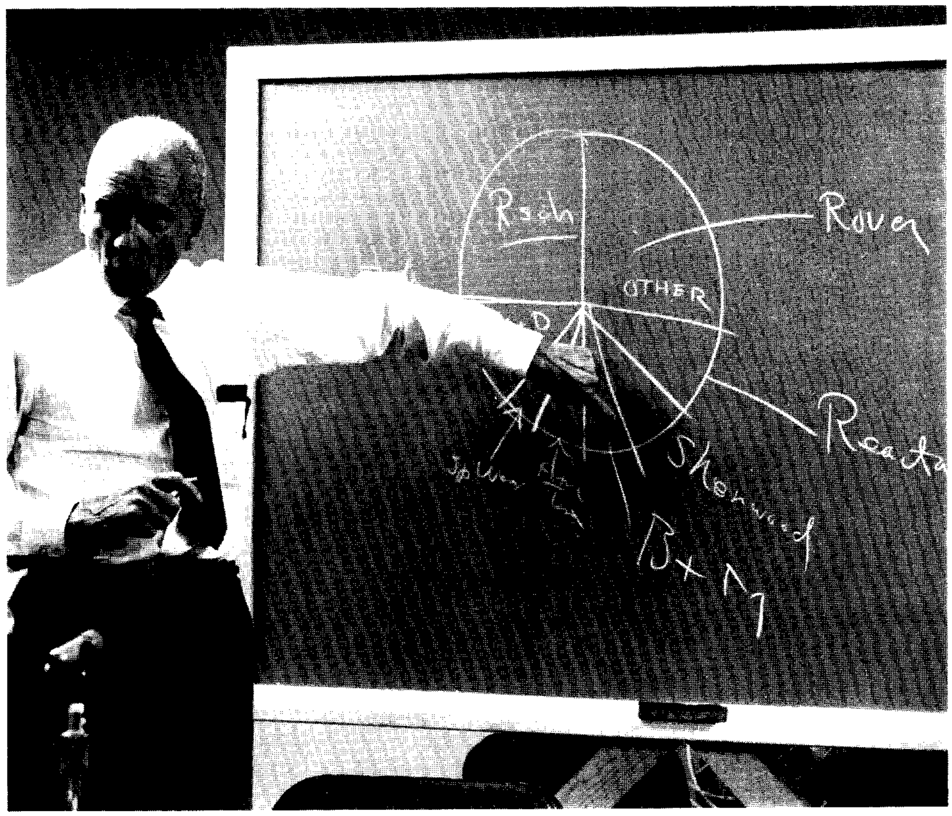
COWAN: But Carson, the Russians paid enormous attention to the significance of our thermonuclear event. The Kurchatov biography says that he was in effect given a blank check. He didn't get it to develop the fission weapon, but after Mike went off he had the resources of Mother Russia at his disposal. 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 1%3.

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 '5l–-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 sideshow such as McCarthy was so fond of—not that they came off much better.

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

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

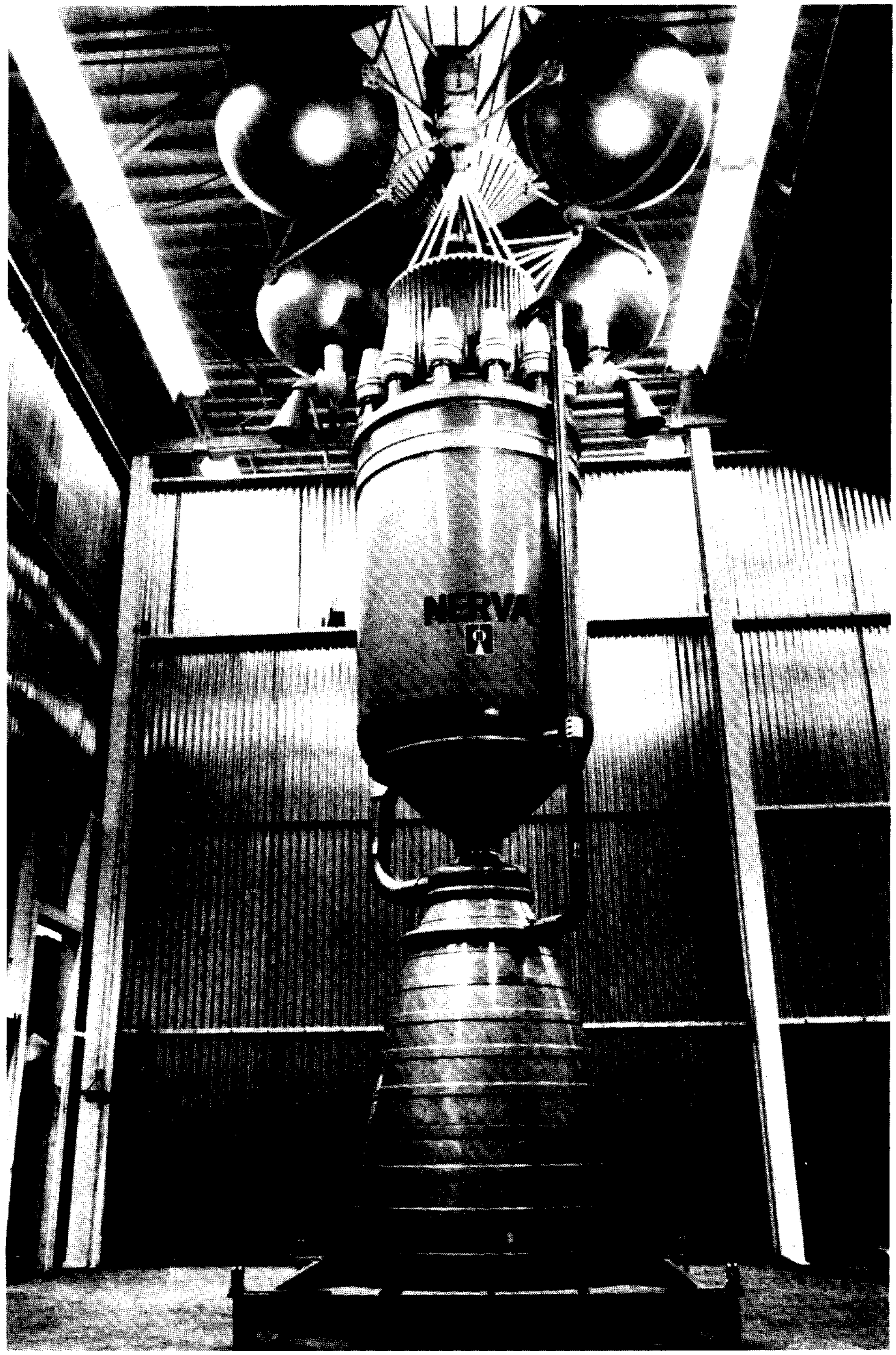
Atomic Energy Commission. These were all inspired by the reaction to the Oppenheimer hearing. The comments were pretty caustic and highly critical, particularly of the guilt-by-association aspect. Lewis Strauss visited at that time, and an indignant group of scientists went to see him at the height of 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 1 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.

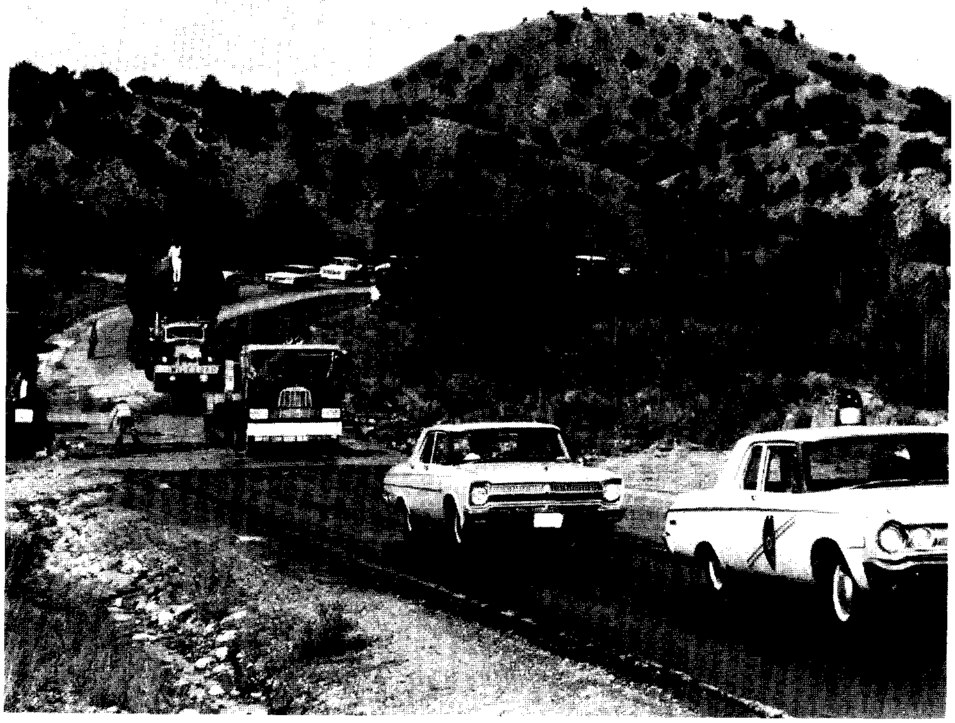
To summarize, health physics was a separate program. Although it was necessary in connection with weapons it really went into a much broader field.

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.

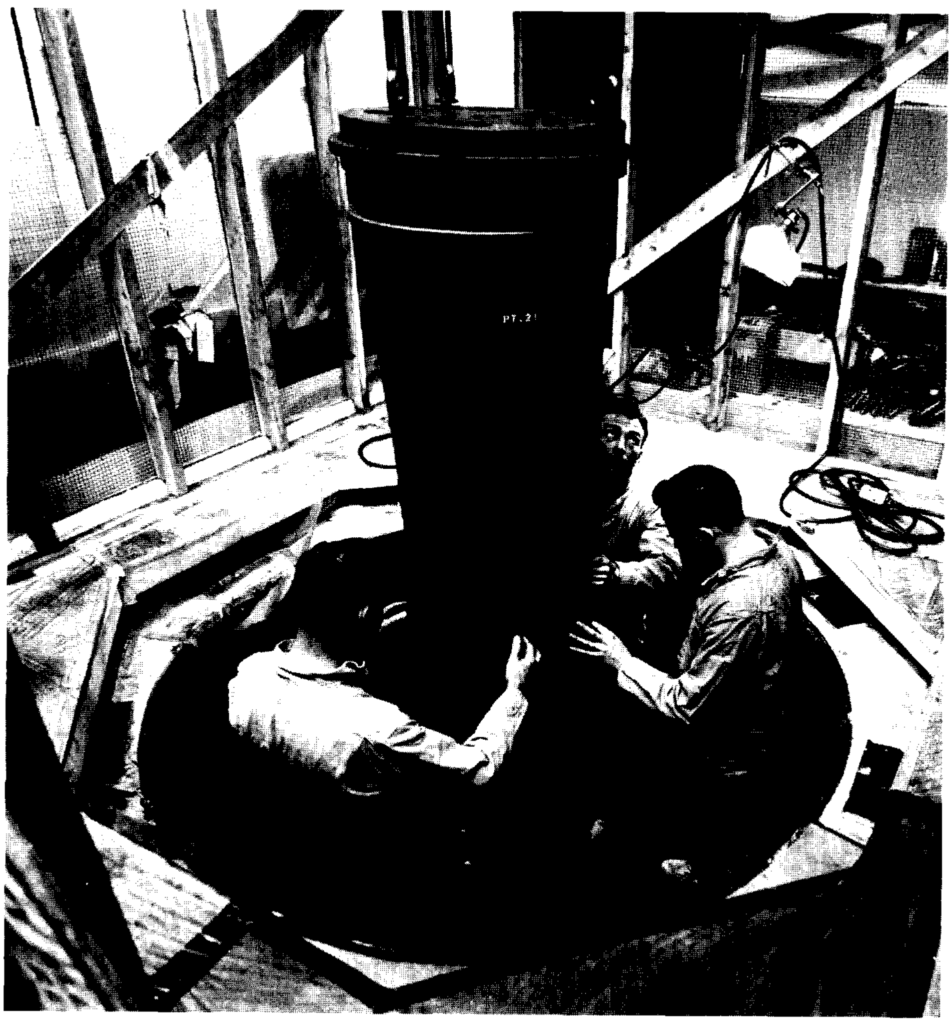
COWAN: An example, not of a program but of the scientific spin-offs, was in radiochemistry. Radiochemists had the freedom to investigate the debris from the Mike explosion, and the result was the discovery of two new elements, einsteinium and fermium, and of all the heavy isotopes of plutonium including 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 19.58 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.

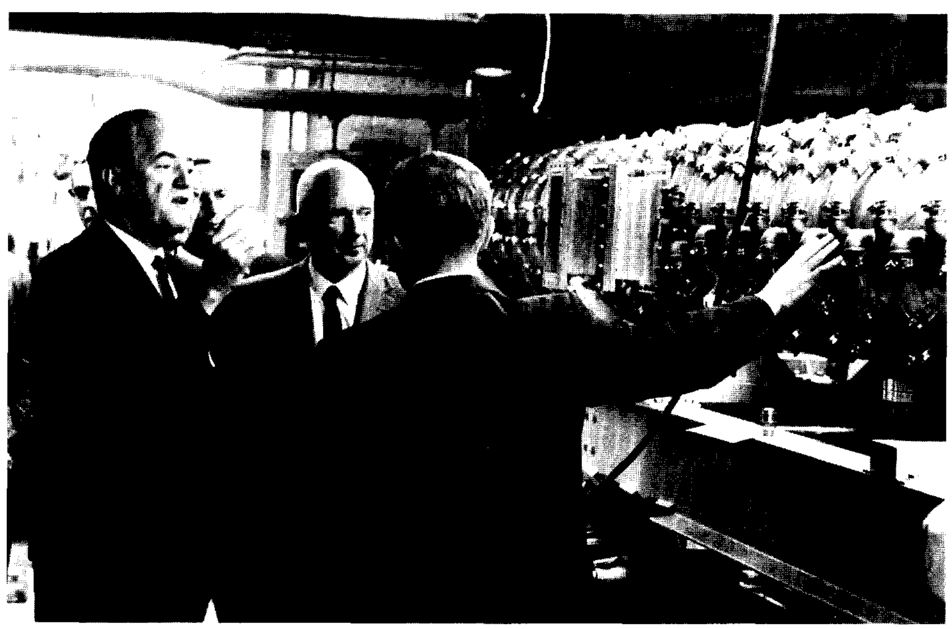
COWAN: We should point out another significant change in the weapons program that occurred after 1959. The emphasis changed from qualitative new concepts in weapons design to systems engineering because the delivery system had changed from airplanes to transcontinental missiles. There came to be an increasing emphasis on the engineering aspects of weapons, their weights, the way they were configured, the way they could lit 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 tit 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 1%2. 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.

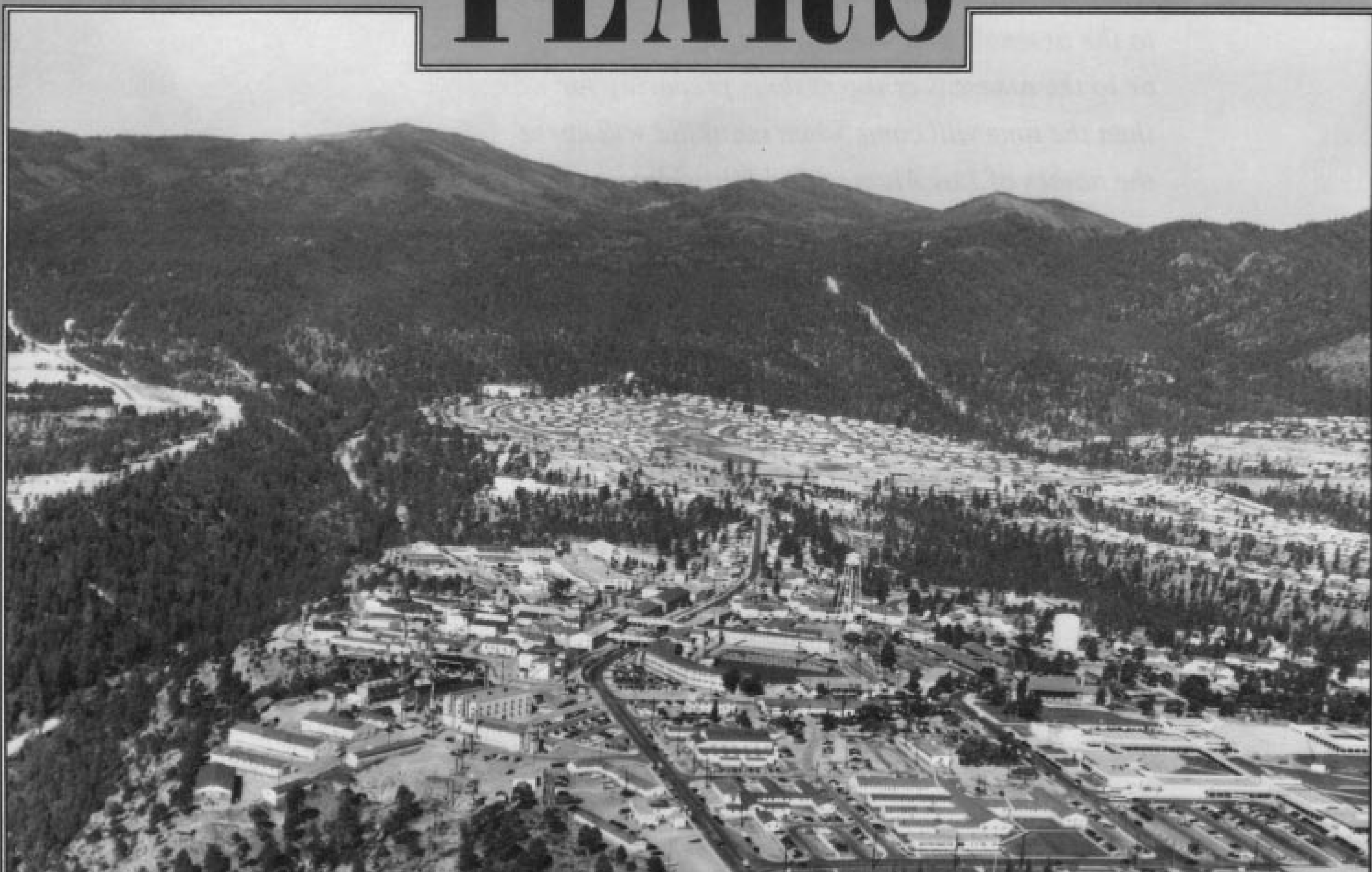
BAKER: There is a great deal to be learned with this deal on the patents. And if DOE lawyers weren't so plentiful, we could go faster with it. But the thing I still don't see is how we are going to completely overcome the problem of proprietary information. A couple of us approached the carbon companies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

A comment from Bradbury in 1980...

This country does not always know how to run its long-range programs, The basic problem is this: major programs today, the nuclear reactor, breeder reactors, controlled thermonuclear fusion programs, and the like, take years and years and years. I'm speaking of decades. But the professional lifetime of some manager in Washington, if he's lucky, is possibly five years. And so what turns out to be one man's meat may be another man's poison in some types of programs. And no man is ever held to account for his errors. When mistakes are made and discovered in the reactor business, the chances are good that the individual who made them is long gone. What is one going to do about it? Programs last so long, by nature, that the man who starts the reactor research doesn't live to finish it. It used to be a sort of standing joke that in our nuclear rocket work we felt similar to the people who built the cathedrals in Europe: they were started by the grandparents and finished by the grandchildren. The last thing that I managed to accomplish before I retired was to get Washington's approval to build a very large, half-mile-long accelerator for the production of some nuclear particles, pions, and a so-called meson factory, which is now running and doing useful research. And you say, what's that for? It's not for bombs, it's not for energy, it's just plain good physics, and the argument for doing plain. good nuclear physics has to be what it always was. You've got to look under every stone and see what might be there. If you hadn't looked under certain stones about neutrons versus uranium in 1938-39, you'd never have found fission. I don't think that this accelerator is very likely to do more than produce good physics, good understanding of sub-nuclear physics, sub-nuclear particles, medical-use discoveries to deal with malignancies because of certain characteristic ways mesons react with tissue. You simply cannot let the country leave stones unturned. There may not be anything there, but suppose there is. You'd better find it.

From "Los Alamos—The First 25 Years" by Norris Bradbury in *Reminiscences of Los Alamos 1943-1945,* Lawrence Badash, Joseph O. Hirschfelder, and Herbert P. Broida, Eds., (D. Reidel Publishing Company. Dordrecht, Holland. 1980), pp. 174-175.

THE BRADBURY YEARS



1945–1970

In late 1945 a small group of courageous and loyal scientists and technicians undertook to continue the post-war operation of the Los Alamos Scientific Laboratory. These men believed that atomic weapons development had barely begun, that other countries would develop such weapons, and that the safety and security of the United States—if not of the world—depended upon the technical lead of this country. These men had the courage to stay at Los Alamos in the face of an uncertain future. . . .

These men did not make demands nor require promises. These men stayed and built the greatest weapons laboratory this country has ever known. These men stayed and developed the greatest array of powerful and flexible atomic weapons of any country in the world—developed them faster, developed them where they were urgently needed and requested by the Armed Forces—developed them to fit the productive resources of the newly established Atomic Energy Commission. They stayed and built a laboratory that developed every successful thermonuclear weapon that exists today. Others left, but these men stayed and worked, and many others came to join them.

What these men accomplished cannot be told in detail, for these facts are classified TOP SECRET. These men do not talk. They believe in deeds, not words. But these deeds earned for the Los Alamos Scientific Laboratory the only Presidential Citation ever awarded to any laboratory for its extraordinary success in the development of both fission and fusion weapons, and its contribution to the collective security of the Nation and the free world. What these men accomplished was this: They built a laboratory from 1200 employees in 1946 to 3000 employees in 1954. They brought back many of the senior wartime staff members as consultants, frequently for months at a time. They worked and thought and had ideas, In the fission weapons field, they advanced development from the few primitive wartime weapons to weapons enormously more powerful; to weapons enormously cheaper; to weapons so enormously more efficient that only a small fraction of the bomb load, and a small fraction of the number of planes, and a small fraction of the cost in fissionable material were required. They multiplied the atomic capability of this country in so many ways that not even billions of dollars spent in active material production would have been equivalent.

Nor was the Laboratory idle in the thermonuclear field. The wartime efforts of a small group of men in the Laboratory were summarized in the 1946 conference. Later in that year, the basic idea for one of the present patterns of thermonuclear weapons arose, although no way to exploit it effectively could then be seen. An elaborate program of basic research, both theoretical and experimental, was undertaken in order to provide both the necessary fundamental data for the basic calculations as to whether the "super" bomb would work at all, even if it could be ignited.

Thermonuclear work never stopped. Basic nuclear data was obtained, TOP SECRET theoretical studies on thermonuclear processes were carried out, the great electronic brain, the Maniac, was being built with such calculations in mind, and simultaneously the necessary practical studies of materials and potential engineering problems were conducted. All this is in the official record of the Laboratory's work during the period from 1946 to 1951. Thermonuclear work grew as the Laboratory grew. By 1949 the design and understanding of fission bombs had proceeded far enough to permit studies of their application to thermonuclear systems to be undertaken. Even before the Russian Bomb was fired, the Laboratory was working on the detailed design of an experiment employing thermonuclear principles which would answer some (but far from all) of the basic questions regarding thermonuclear systems. Still later events suggested the addition to the Greenhouse program of even a more elaborate experimental approach. In March 1950 the Laboratory went, on its own volition, on a 6 day week for almost 3 years to speed its developments while it was further expanding its scientific staff.

Had the Laboratory attempted to exploit the thermonuclear field to the exclusion of the fission field in 1946, what would have happened? Hypothetical history can only be an educated guess, but the guess in this case is almost certain. The fission weapons stockpile would have been but a fraction of its present size. The essential fission techniques required for practical thermonuclear weapons would not have been developed. Discouragement would have nagged at those who worked in a field without the means for practical accomplishment, and the program—and the Laboratory—might have died.

Rather than delaying the actual accomplishment of thermonuclear weapons, the Los Alamos Scientific Laboratory has, by its insistence on doing necessary things first, demonstrably provided the fertile soil in which the first feasible ideas could rapidly grow, and demonstrably did develop such weapons, and probably, but not demonstrably, did so years ahead of any other course which could have been pursued with the facilities and people available. Technically, the development of fusion weapons is so inextricably allied with and dependent on the development of fission weapons, that great success in the former had to follow success in the latter. . . .

At every stage from 1946 to the present time, the fission and fusion programs—both in basic research and in practical application—were pursued with the maximum appropriate emphasis, with care, with precision, and with success. What "might have been" is idle speculation. What would have happened to World War II if the Manhattan District had started work in 1939?

The imputation of disloyalty to that now large group of scientists and technicians who are fundamentally responsible for every nuclear weapon, fission and fusion, that the United States has in its stockpile, who are responsible for the atomic weapons leadership that this country presently enjoys, and who are dedicated to the continuance of this leadership, is a tragic, if not malevolent, thing. The motives behind these accusations of Los Alamos are unclear; their bases are faulty and irresponsible information necessarily obtained from those who do not and cannot know the classified facts; and their effect on the Laboratory would be wholly disheartening were it not for our knowledge that the facts warrant the full confidence of the Nation in our accomplishments over many years.

Norris Bradbury, September 24, 1954

Press statement made to Santa Fe's The New Mexican *in response to advance press on* The Hydrogen Bomb: The Men, The Menace, The Mechanism, *a book by Shepley and Blair.*

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 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 applacations. He is head of the Laboratory's Meson Physics Facility and was in large part responsible for its existence.

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.

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. Operation 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 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.

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.

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 gentleman 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 involved 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 tit 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 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.

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.

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.

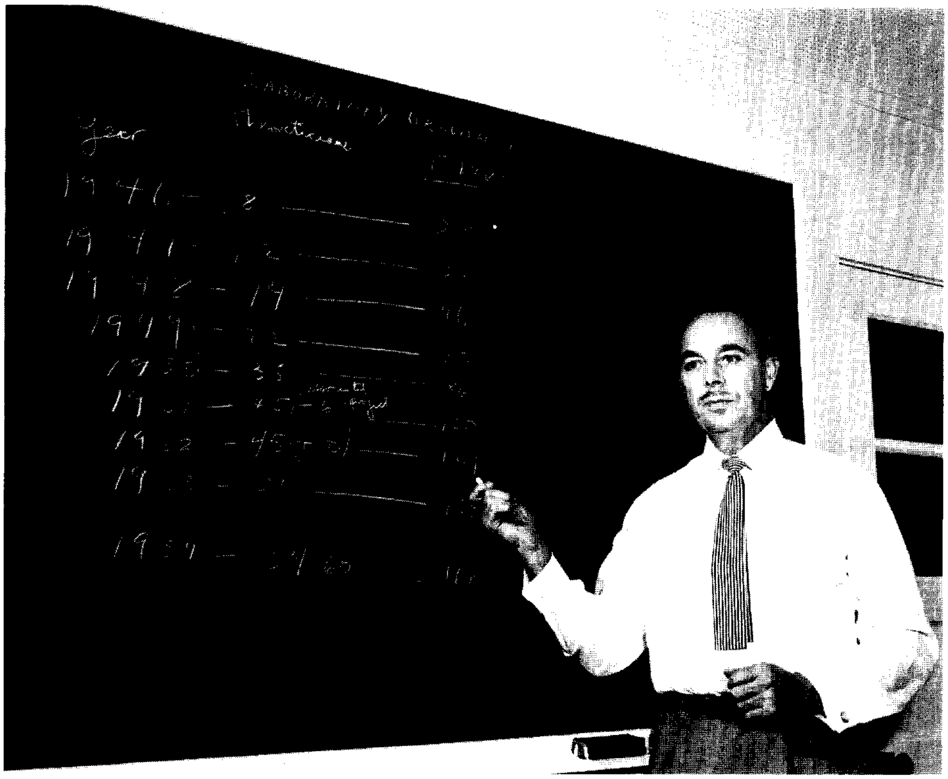
The Russian test caused a number of people, most of them not at Los Alamos, to feel that the nation was now in peril and must make a strong and tremendously impressive response to the terrible misdeed of the Russians. Teller, Lawrence, Alvarez, Lewis Strauss, Senator MacMahon, and Air Force Secretary 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.

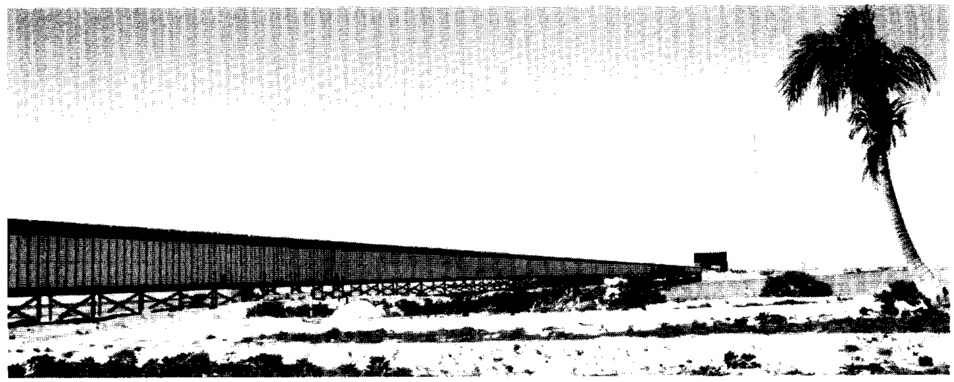
To return to the technical story, on the theoretical side we tried to calculate how thermonuclear reactions might possibly proceed. taking into account this effect or that effect that had been ignored before. There were also gaps in what was known about the neutron and thermonuclear cross sections, and, 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.

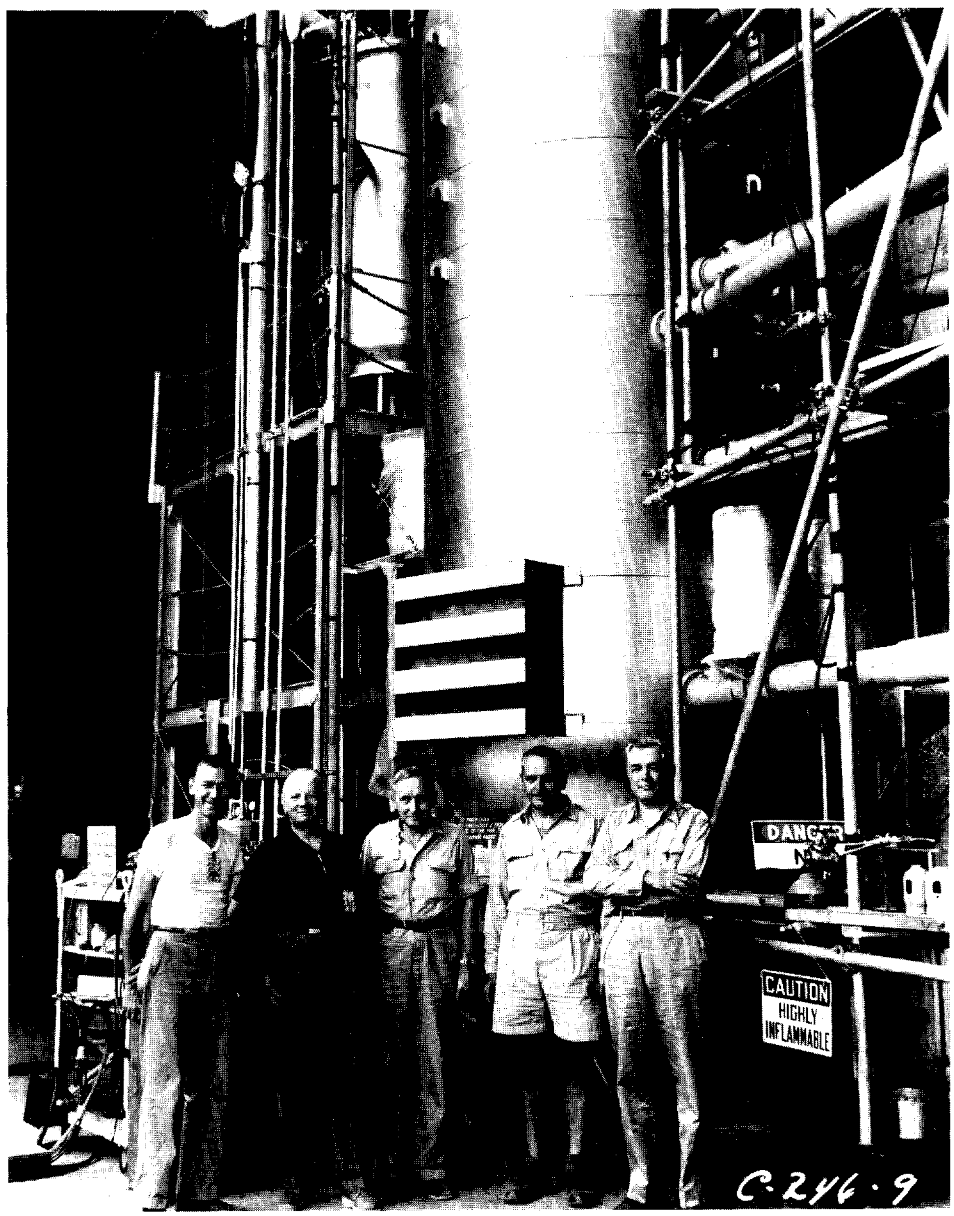
Between January 1950 until the end of

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 tit into missiles.

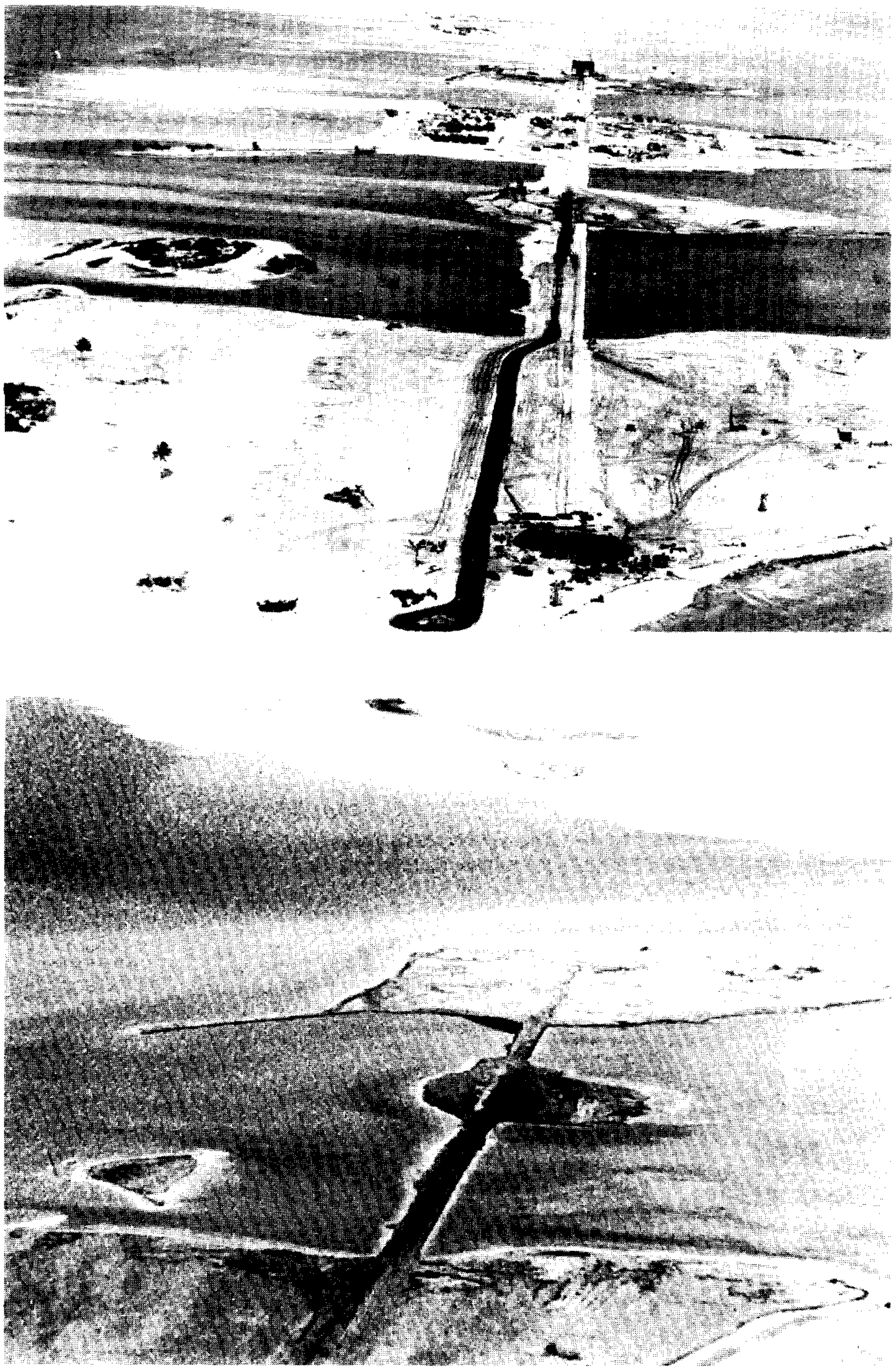
COWAN: During this period following the Russian test, we were also involved in an accelerated program for testing small fission devices. which, by the way, was done at the Nevada Test Site in 1951.

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 halfway 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.

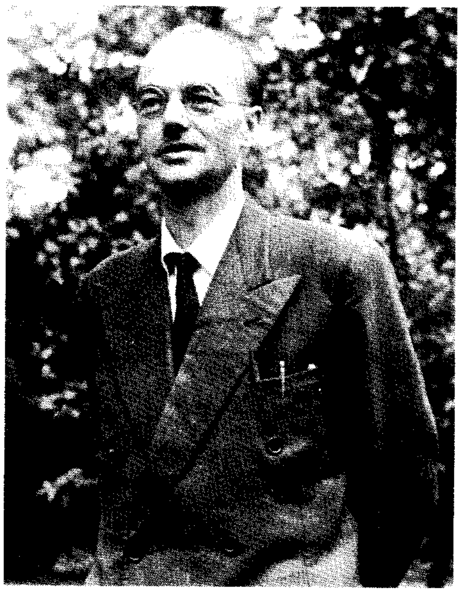
MARK: So a stream of salesmen from all the computing manufacturers began to beat a track to the door.

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 bad 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.

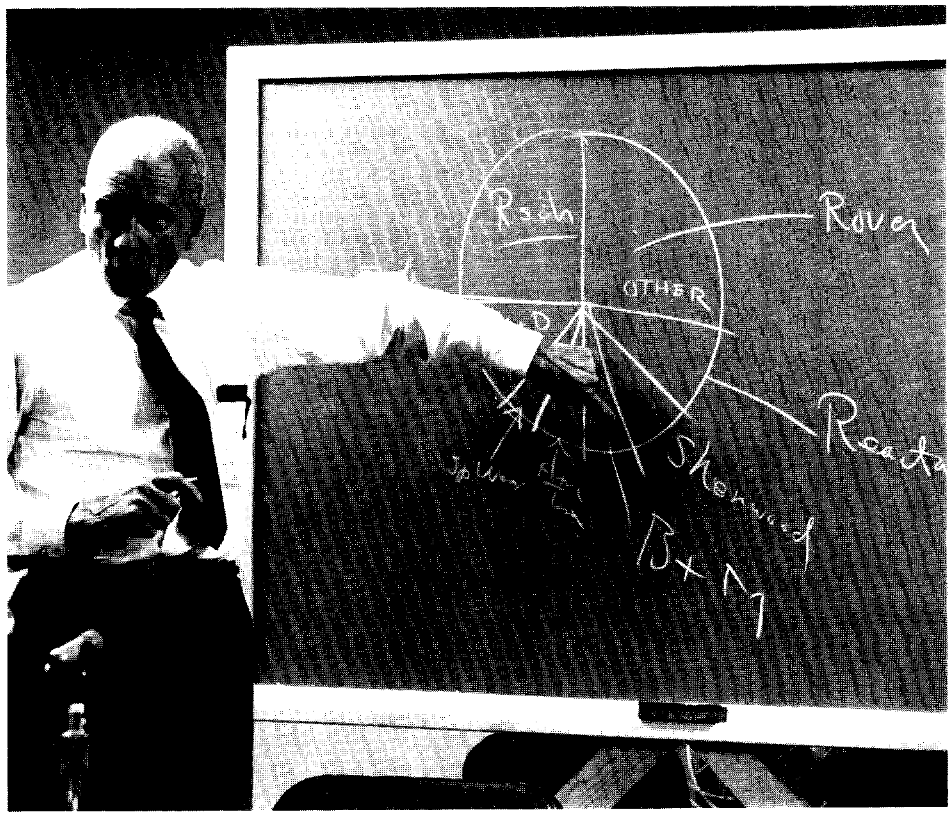
COWAN: But Carson, the Russians paid enormous attention to the significance of our thermonuclear event. The Kurchatov biography says that he was in effect given a blank check. He didn't get it to develop the fission weapon, but after Mike went off he had the resources of Mother Russia at his disposal. 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 1%3.

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 '5l--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 sideshow such as McCarthy was so fond of—not that they came off much better.

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

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?*

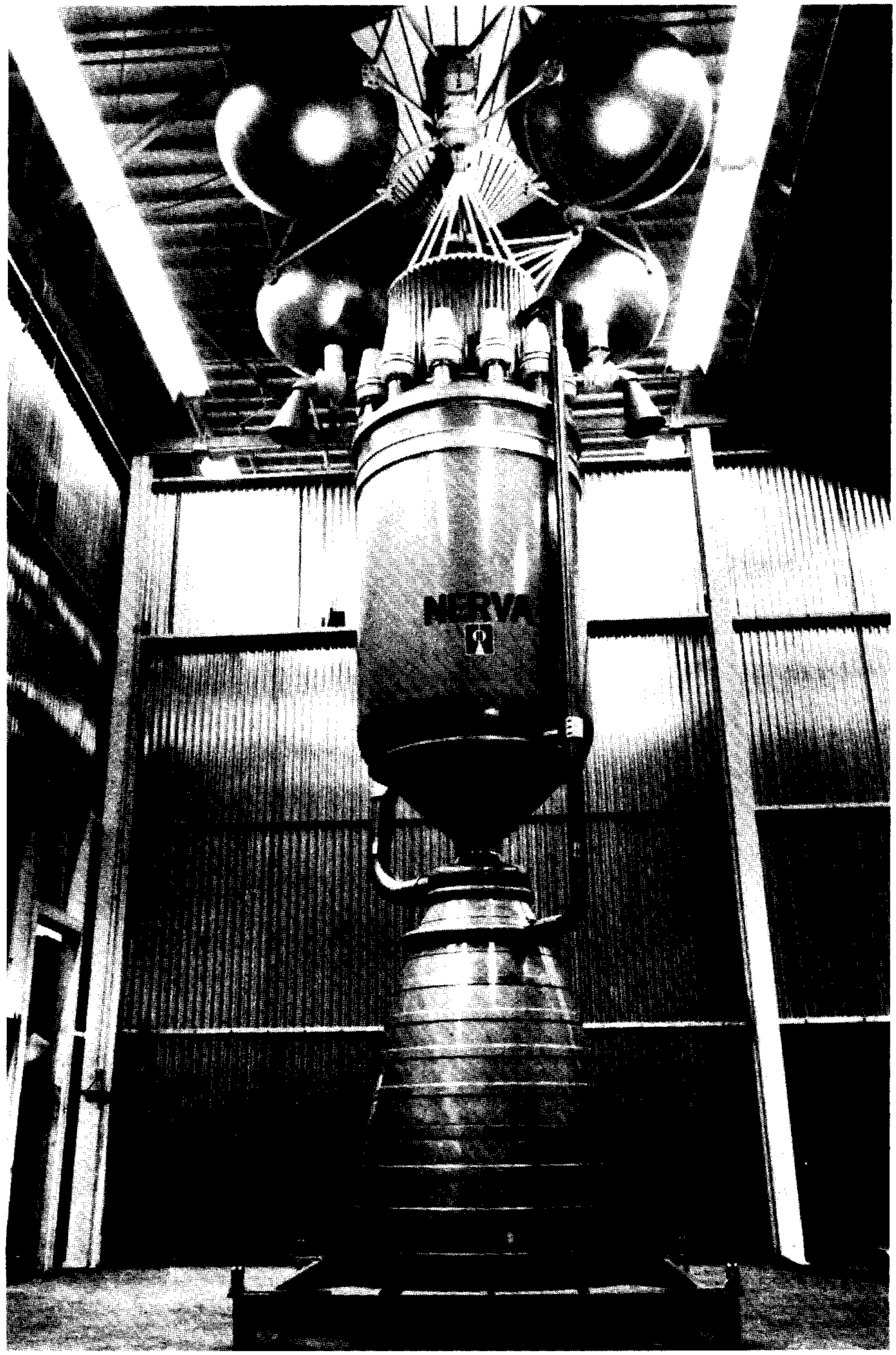
MARK: The fast reactor Clementine was approved in late '45 to investigate plutonium as a possible reactor fuel. It had never been used in a reactor. and the only place in the country, or for that matter in the world, that was prepared to handle plutonium was Los Alamos. Also, it was known then that a 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 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



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.

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 1 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.

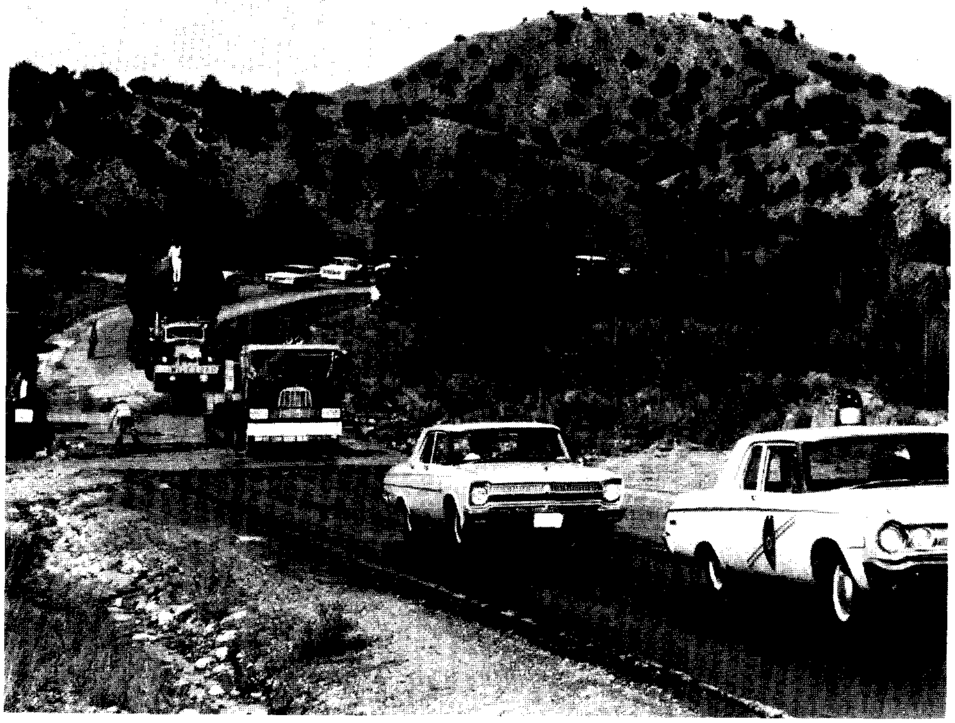
To summarize, health physics was a separate program. Although it was necessary in connection with weapons it really went into a much broader field.

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.

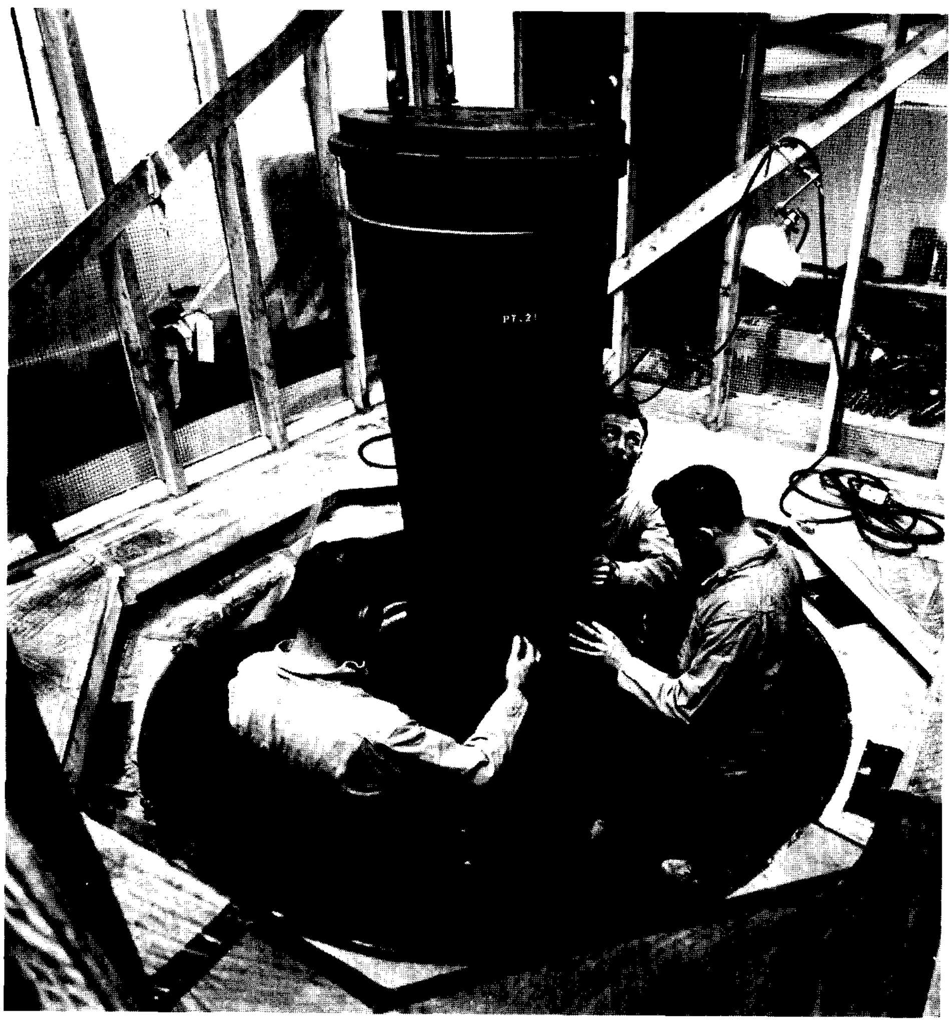
COWAN: An example, not of a program but of the scientific spin-offs, was in radiochemistry. Radiochemists had the freedom to investigate the debris from the Mike explosion, and the result was the discovery of two new elements, einsteinium and fermium, and of all the heavy isotopes of plutonium including 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 19.58 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.

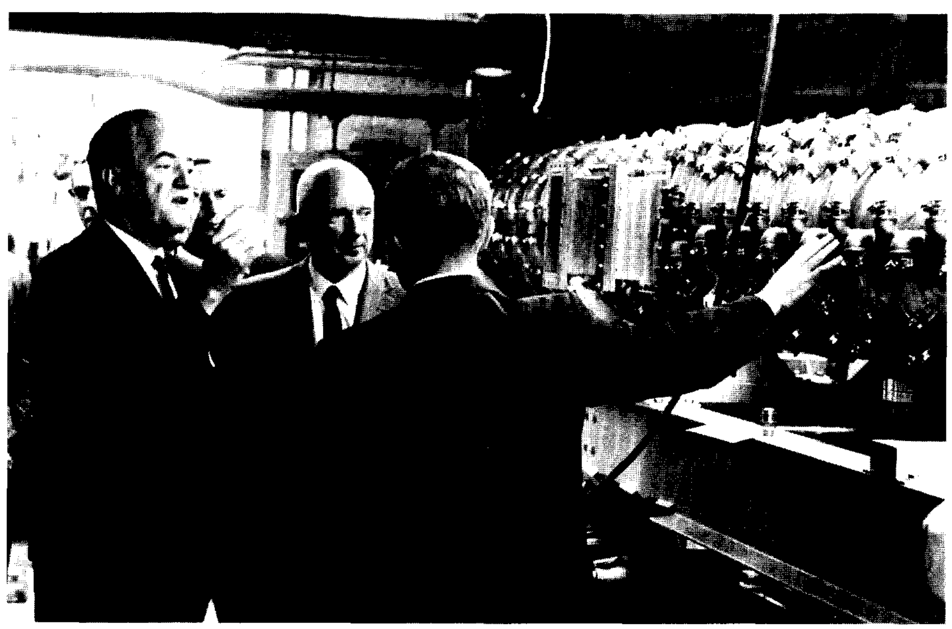
COWAN: We should point out another significant change in the weapons program that occurred after 1959. The emphasis changed from qualitative new concepts in weapons design to systems engineering because the delivery system had changed from airplanes to transcontinental missiles. There came to be an increasing emphasis on the engineering aspects of weapons, their weights, the way they were configured, the way they could lit 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 tit 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 1%2. 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 companies 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." ■

A comment from Bradbury in 1980...

This country does not always know how to run its long-range programs, The basic problem is this: major programs today, the nuclear reactor, breeder reactors, controlled thermonuclear fusion programs, and the like, take years and years and years. I'm speaking of decades. But the professional lifetime of some manager in Washington, if he's lucky, is possibly five years. And so what turns out to be one man's meat may be another man's poison in some types of programs. And no man is ever held to account for his errors. When mistakes are made and discovered in the reactor business, the chances are good that the individual who made them is long gone. What is one going to do about it? Programs last so long, by nature, that the man who starts the reactor research doesn't live to finish it. It used to be a sort of standing joke that in our nuclear rocket work we felt similar to the people who built the cathedrals in Europe: they were started by the grandparents and finished by the grandchildren. The last thing that I managed to accomplish before I retired was to get Washington's approval to build a very large, half-mile-long accelerator for the production of some nuclear particles, pions, and a so-called meson factory, which is now running and doing useful research. And you say, what's that for? It's not for bombs, it's not for energy, it's just plain good physics, and the argument for doing plain. good nuclear physics has to be what it always was. You've got to look under every stone and see what might be there. If you hadn't looked under certain stones about neutrons versus uranium in 1938-39, you'd never have found fission. I don't think that this accelerator is very likely to do more than produce good physics, good understanding of sub-nuclear physics, sub-nuclear particles, medical-use discoveries to deal with malignancies because of certain characteristic ways mesons react with tissue. You simply cannot let the country leave stones unturned. There may not be anything there, but suppose there is. You'd better find it.

From "Los Alamos—The First 25 Years" by Norris Bradbury in *Reminiscences of Los Alamos 1943-1945,* Lawrence Badash, Joseph O. Hirschfelder, and Herbert P. Broida, Eds., (D. Reidel Publishing Company. Dordrecht, Holland. 1980), pp. 174-175.

Magnetic Fusion

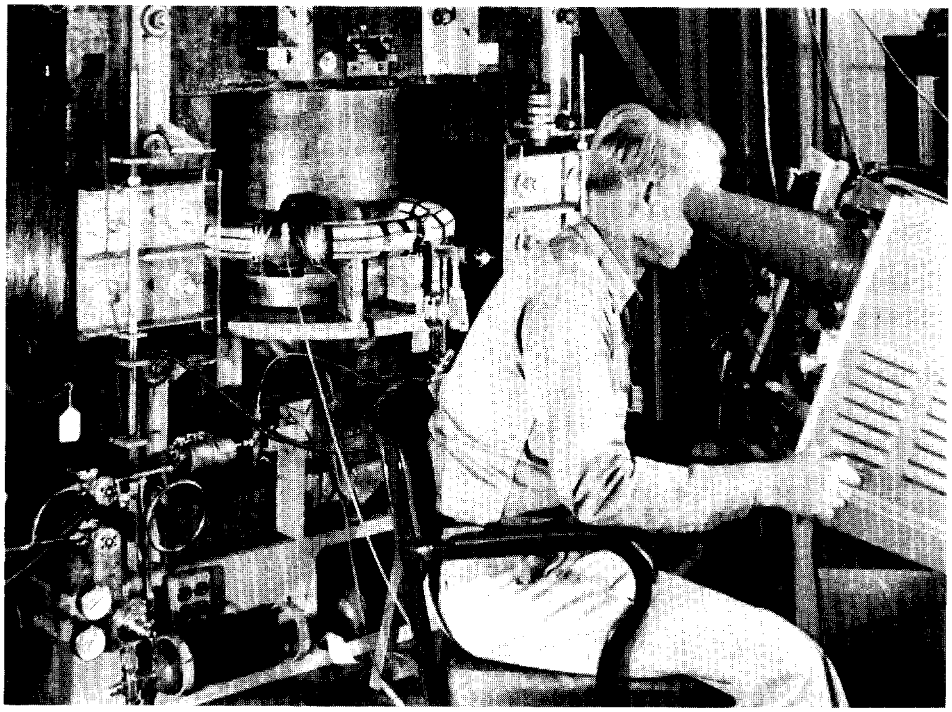
by James A. Phillips

During the war years while the Laboratory was thinking about ways to use nuclear energy to create violent explosions. Ulam, Fermi. Teller, Tuck, and others were also talking about using fusion of the light elements for the controlled release of energy and the production of useful power.

It had been understood since the '30s that the source of energy in the sun and other stars is thermonuclear fusion occurring in the very hot plasmas that make up the stars' centers, The thermal energy of the nuclei in these plasmas is so high that positively charged nuclei can penetrate the Coulomb barrier and approach so closely that fusion can occur,

To duplicate this process in the laboratory requires creating a plasma, heating it to thermonuclear temperatures, and confining it long enough for fusion reactions to take place. By 1946 the Los Alamos group concluded that the plasma would have to be heated to about 100 million degrees Celsius—ten times hotter than the sun's center and many orders of magnitude higher than any temperature yet achieved on Earth. Since a plasma that hot would quickly vaporize the vacuum container in which the plasma is created, some means for preventing the plasma's contact with the container walls was required. A "magnetic bottle," that is, a magnetic field of appropriate strength and geometry, was a possibility. A cylin-

Pipe-smoking Jim Tuck, John Osher foreground), and John Marshall (right) with "Picket fence, " one of the early magnetic fusion experiments.



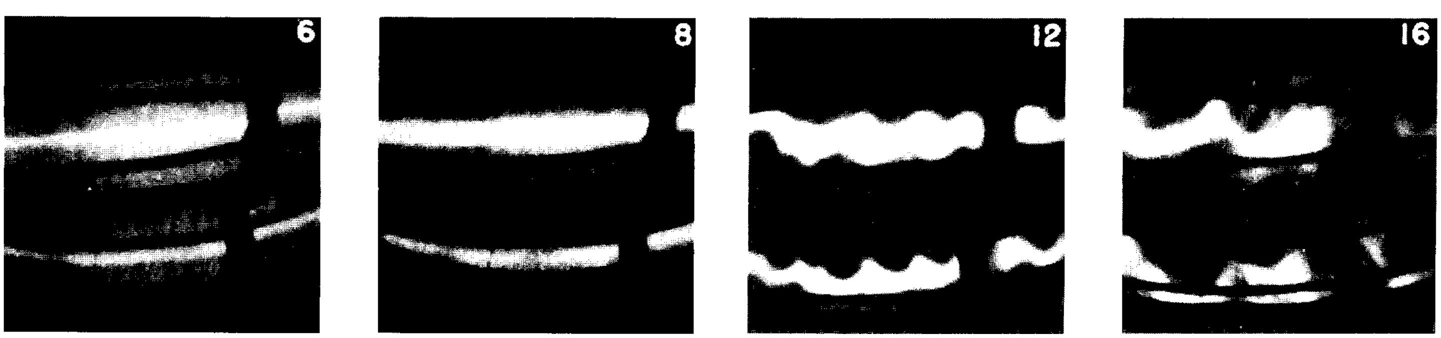
The Perhapsatron, which was built in 1952-53, was the first Z-pinch device at Los Alamos. The toroidal discharge tube surrounds the central core of an iron transformer.

drical magnetic bottle could be produced, but the plasma particles would quickly be lost out the ends. On the other hand a toroidal, or doughnut-shaped, bottle would eliminate end losses, but, as Fermi pointed out, particles in a simple toroidal magnetic field will rapidly drift outward and strike the walls.

Calculations of the energy released by thermonuclear reactions versus the energy lost through radiative and other processes were also done in those early days. The conclusion was that in terms of energy balance a power reactor based on nuclear fusion was not impossible.

In 1950 Jim Tuck returned to Los Alamos (after a sojourn in his native England and at the University of Chicago) and began working on magnetic confinement with a "Z-pinch." In this scheme an electric field applied along the axis of a discharge tube drives an electric current whose self-magnetic field pinches the current channel toward the axis of the tube. It was thought that the pinching process would produce the high plasma densities and temperatures necessary for fusion. Tuck knew from the work of British scientists that building up the current rather rapidly to create high temperatures caused instabilities in the pinch. He suggested that the instabilities might be minimized by applying a small electric field across the length of the discharge tube and increasing the current slowly. In addition he wanted to try this slow Z-pinch in a toroidal discharge tube.

In late 1951 Tuck took his ideas for the slow pinch to Bradbury, who gave him $50,000 to see what he could do. By early 1952 Tuck and his group had scrounged some parts of an old betatron, gotten the shops to make a toroidal quartz tube, hooked up a bank of capacitors, and built the first Perhapsatron. (At the same time this group was making definitive cross-section measurements on the fusion of deuterium and tritium for the hydrogen bomb project.) By 1953 it was clear that this device produced a pinch, but that instabilities



Z-pinch instabilities observed in the first Perhapsatron experiments in 1953. The plasma, initially uniformly concentrated along the Z-axis, begins to break up and strikes the walls of the discharge tube in a few millionths of a second.

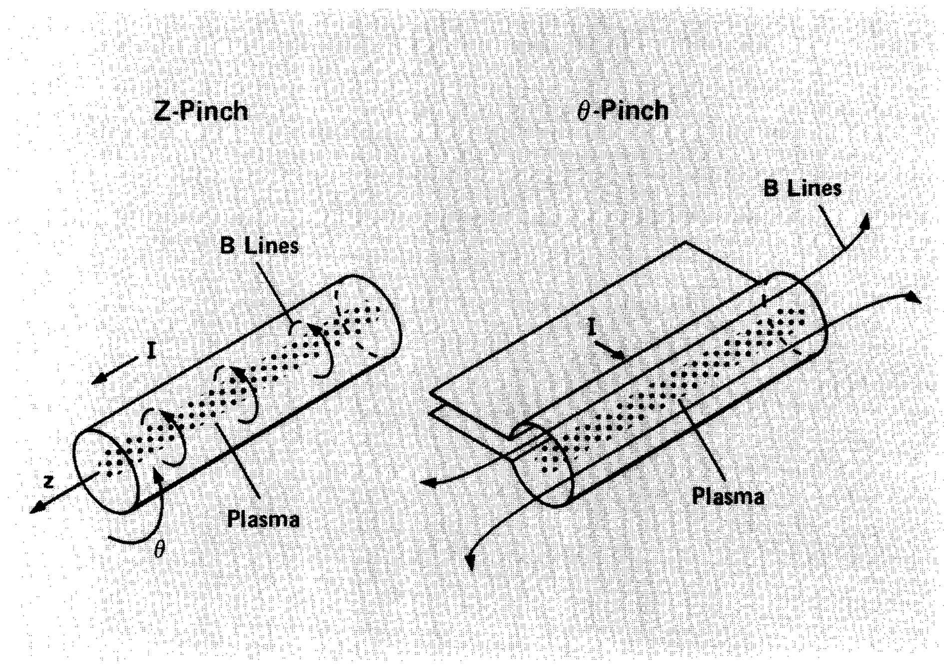
quickly dispersed the plasma.

The fast Z-pinch was the next idea to be tried. In 1954 Garwin and Rosenbluth, then at Los Alamos. suggested a theory indicating that a very strong electric field could form a pinch so fast that the heating of the plasma by its inward motion would initiate thermonuclear burn before the plasma had a chance to disperse. This theory led to experiments on microsecond time scales with a cylindrical tube. Again instabilities destroyed the plasma.

The stability of the Z-pinch could be improved by adding a longitudinal magnetic field, and a series of experiments were done over the next few years with Perhapsatrons incorporating such a field. Neutrons from the fusion of two deuterons were detected, but it was quickly shown that this fusion was caused not by heating but by acceleration of some of the plasma particles. Thermonuclear temperatures had not been reached.

The first experiment in which *thermonuclear* fusion was achieved in any laboratory was done in 1958 with the Scylla I machine. This experiment was based on the "@pinch," a pinch produced by a very short. intense pulse of current in a coil outside the discharge tube. The measured energy distributions of the neutrons, protons, and tritons from the Scylla I experiments gave definitive evidence that the plasma reached a temperature of about 15 million degrees Celsius and that the neutrons were the result of thermonuclear fusion.

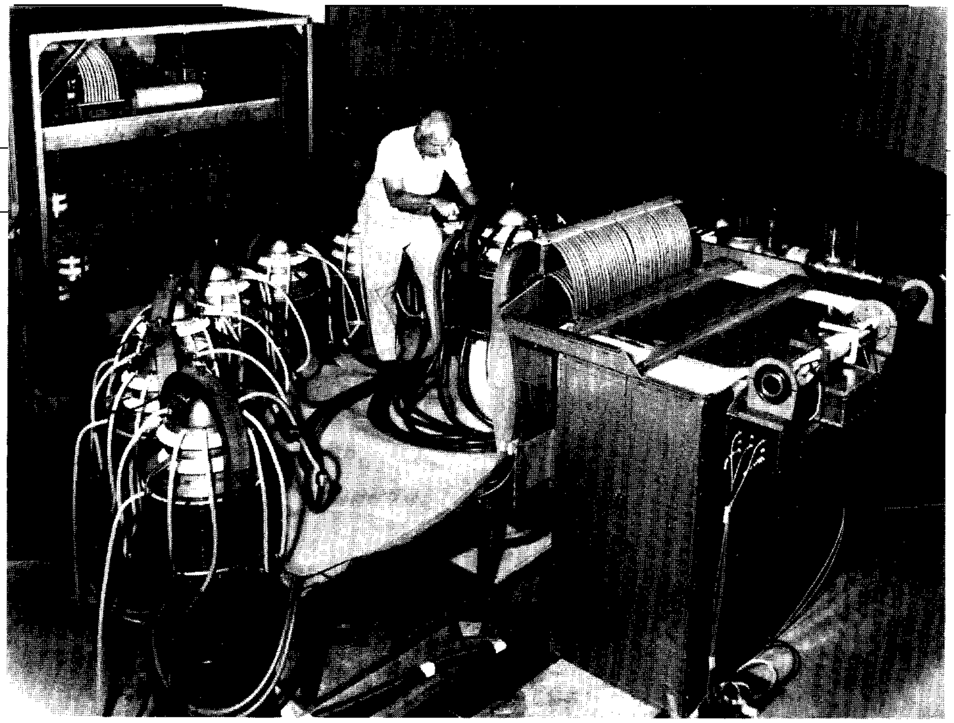
Attempts were made over the next decade to scale up the 6-pinch experiment in order to improve the confinement times. In 1964 plasma temperatures of approximately 40 million degrees Celsius and a few billion deuteron-deuteron fusion reactions per discharge were achieved with the Scylla IV device, but the plasma confinement times were less than 10 millionths of a second, The largest 6-pinch machine was Scyllac, a toroidal machine completed in 1974. Experiments with Scyllac demonstrated the behavior of high-density pinches in toroidal geometry. However, during this time the national fusion research program began to examine the technologies required for fusion reactors. The fast risetime, high voltage, and fast feedback systems required for a Scyllac-type O-pinch machine did not project to an attractive reactor. Work on Scyllac was discontinued in 1977.



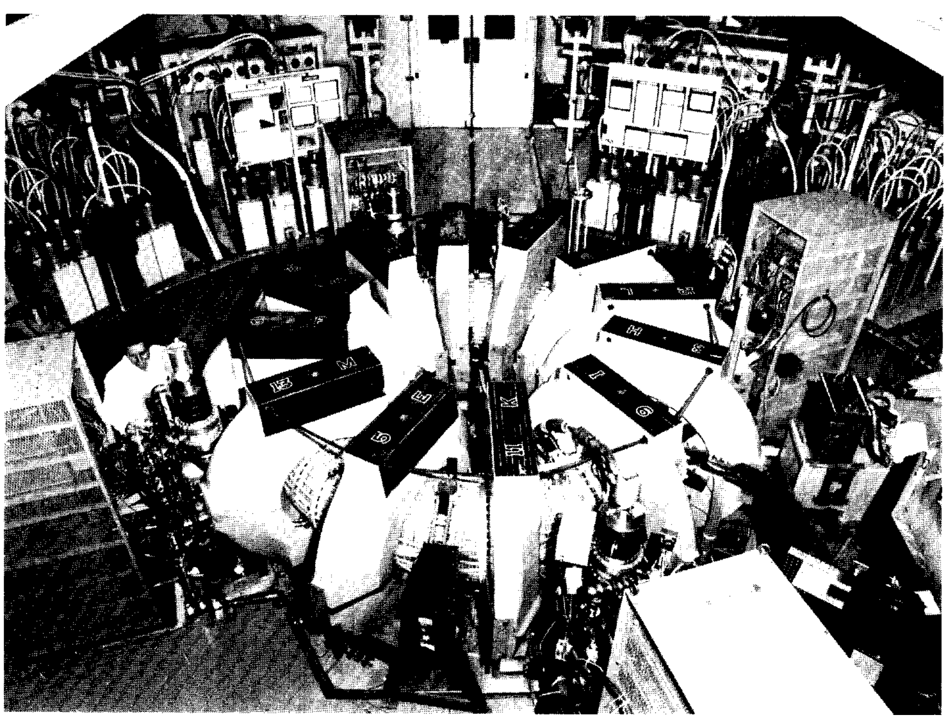
In the Z-pinch an electric field applied along the axis of a discharge tube produces a current I whose self-magnetic field* B *pinches the current channel toward the axis. In the O-pinch the magnetic field is created by a current flowing in the 0 direction through a coil outside the discharge tube. In both cases the pinching process produces high plasma temperatures and densities.

In the meantime work on the Z-pinch, which had been abandoned in 1961, was revived in 1967. Advances in experiment and theory have led to further improvements of the toroidal Z-pinch approach, the most significant of which is the reversed-field pinch. It is similar to the early stabilized Z-pinches of the Perhapsatron days, but addition of a reversed toroidal magnetic field further increases plasma stability. Present-day experiments are carried out with better vacuum pumps and with metal, rather than glass, container walls. These improvements reduce contamination of the plasma by impurities and thereby reduce radiative energy losses and cooling of the plasma. Also, computer simulations have helped provide better magnetic field configurations for the reversed-field pinch.

The reversed-field pinch is one of the alternative approaches to controlled fusion being studied in the United States. It offers distinct advantages for a fusion reactor. Since its magnetic field configuration allows a greater plasma pressure to be confined for a given magnetic field pressure, it offers the possibility of more energy output per unit plasma volume. Other possibilities it offers are ohmic heating to ignition and a reduction in reactor complexity. Overall, the reversed-field pinch offers a new option in magnetic fusion: a compact, high-power-density reac-



Scylla I, the 0-pinch device that in 1958 produced the first thermonuclear fusion in any laboratory. The high-voltage capacitors are on the left and the discharge tube is on the right. This machine and the Z-pinch Perhapsatron S-4 were displayed in Geneva in 1958 at the Second International United Nations Conference on Peaceful Uses of Atomic Energy.



ZT-40M, the latest Los Alamos reversed-field pinch experiment. Toroidal and poloidal current windings are wound on the outer surface of the torus. The large objects surrounding the torus at several positions are the iron transformer cores.

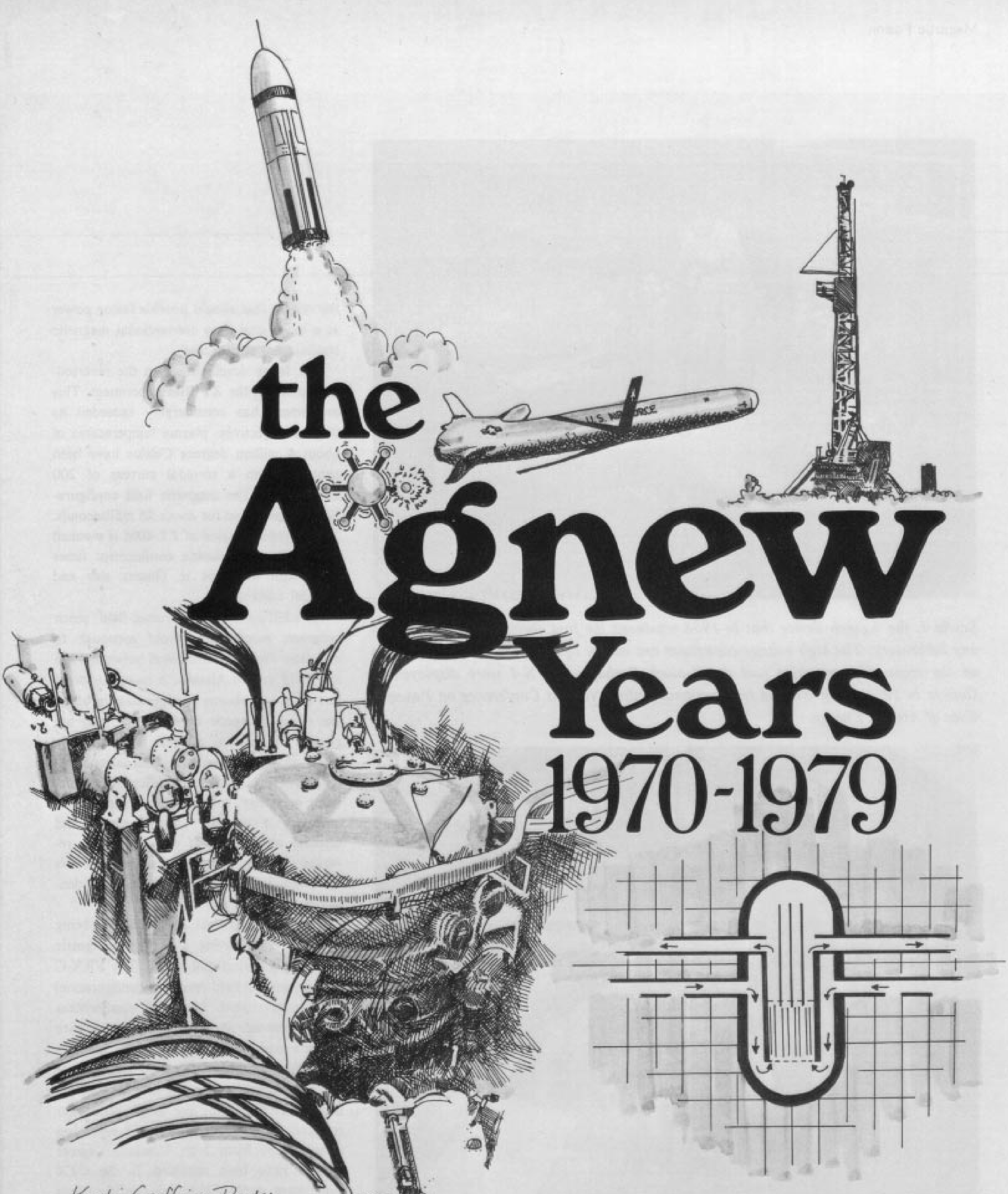
tor system that should provide fusion power at a lower cost than conventional magnetic confinement approaches.

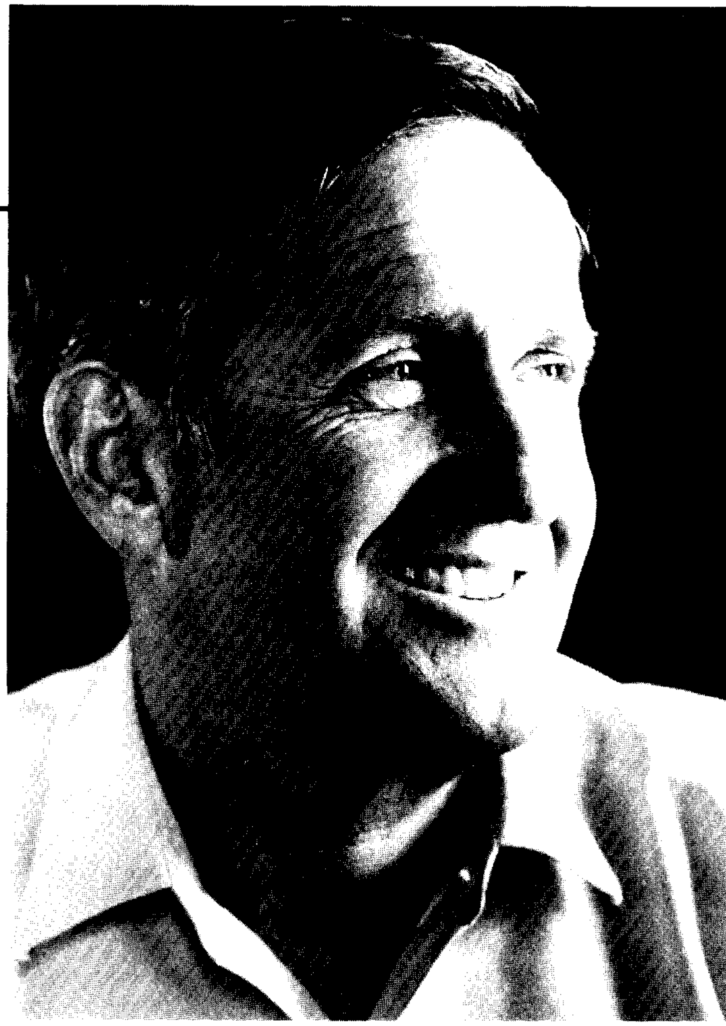
Our latest development on the reversed-field pinch is the ZT-40M experiment. This experiment has considerably exceeded its original objectives: plasma temperatures of about 4 million degrees Celsius have been achieved with a toroidal current of 200 kiloamperes. The magnetic field configuration is maintained for about 25 milliseconds. An upgraded version of ZT-40M is planned to explore how plasma confinement times scale with increases in plasma size and toroidal current.

In addition to the reversed-field pinch program, a compact toroid approach to magnetic fusion was initiated between 1976 and 1979 at Los Alamos. A compact toroid has a toroidal plasma configuration in which the major magnetic confinement fields are created by internal currents in the plasma rather than by currents in external conductors. This arrangement simplifies the confinement geometry and thereby eliminates the need for a toroidal vacuum vessel and toroidal magnetic field coils. Like the reversed-field pinch, the compact toroid offers the possibility of compact, high-power-density reactor systems.

Two compact toroid approaches are being pursued, each having a different magnetic field configuration and shape. The FRX-C experiment (a field-reversed configuration) has demonstrated favorable confinement scaling with size and has achieved impressive Lawson parameters (density times confinement time) of about 4 x 10^{11} seconds per cubic centimeter compared with the approximately 10^{14} seconds per cubic centimeter required for fusion energy break-even. Ion temperatures from I to 7 million degrees Celsius have been achieved. In the CTX experiment stable spheromak configurations lasting a record time of 2 milliseconds have been produced, We are currently studying methods for changing from pulsed to steady-state spheromak operation. ■

the Agnew Years 1970-1979





Vintage Agnew

June 15, 1966

talk to Army group

. . . It does seem to me that with all the basic information we don't know, with all the problems in which we are involved, with all the deficiencies that exist in the world, that a scientist should in some degree. ., stick his head out of his office or his laboratory, whether he is a first year lab assistant or last year's Nobel laureate, and ask himself . . . Is the problem I'm working on one of those whose solution might directly help my colleagues or my fellow countrymen right now or in the future? If the scientist doesn't know, it is probably because in his narrow pursuit of his particular field he actually doesn't know what is going on around him. He may not have taken the time to even find out, or worse, he doesn't want to. This attitude worries me very much.

July 8, 1970

talk entitled Tactical Nuclear Operations

In the last 20 years we and other nations have been engaged in numerous arguments which resulted in physical combat. The political and military approach to these confrontations has been to rely on conventional weapons systems. Although we pretend to have a tactical nuclear capability, we have no doctrine for carrying out tactical nuclear warfare, nor do we seem interested in developing a tactical nuclear capability. Yet, if properly structured, it could conceivably deter these lesser wars—or at least make our forces more effective if they are challenged . . . .

Let me take as an example a particular military target in North Vietnam: the Thanh Hoa Bridge. This bridge is about 540 feet long. For military reasons we decided it had to be destroyed. . . .

We flew 657 strike sorties. In addition we employed approximately 300 supporting sorties. We dropped [2.5] million pounds of bombs, we lost 9 aircraft. In addition three optically guided Walleyes were launched at the bridge. Each of the Walleyes actually hit the bridge but the 750 pound warheads were insufficient to seriously damage it. We never were able to collapse a single span. Present rumors state that the bridge doesn't exist but is simply painted on the water. . . .

Had [the] Walleyes carried a [subkiloton] nuclear warhead. . . such as at long last is being provided in the Mk-72, the bridge would have been put out of action, Instead of expending 2.5×10^6 pounds of high explosive in about 700 sorties, the mission could have been accomplished with at most *two* strike sorties and a few cover aircraft . . . . The collateral damage from [such] a ground [nuclear] burst . . . would be. . . negligible compared to that actualIy imposed with conventional explosives as currently delivered with free fall bombs. . . . [Moreover] burial to optimum depth (which maximizes cratering effects and minimizes fallout) is feasible with devices now under development.

July 13, 1971

talk to the National Classification Management Society

Almost my whole professional career has been involved with technical work which has had a running battle with classification. To be very frank with you I've never won an argument with a classification officer and I've never understood why I've continued to lose. . . .

In spite of our country's background in freedom. . . we all know there is a tremendous amount of secrecy and classification involved in government and private industry. Some of it is certainly warranted and will always be required if we are to have a competitive capitalistic industry. But there comes a time when secrets are no longer secrets and impedances imposed by secrecy or classification are no longer warranted. . . .

[For example] I believe that the philosophy or concept of embargoes on materials, products, and technology in today's world is archaic. . . . In fact. . . if the intent of the embargo concept [as embodied in the Battle Act of 195 1] was to guarantee U.S. conventional military superiority it has failed. . . .

Not so long ago the President announced that he was going to attempt to open trade with China. I don't believe there is a person here who doesn't believe that is a splendid idea. But, . . to pacify our basic fears, which I believe are no longer warranted, the White House quickly stated that of course we wouldn't allow the export of commerical jet aircraft or diesel locomotives. . . which the White House then stated that China very much wanted. . . . Do we really believe that in 1971 a nation of 750 million people shouldn't have commerical jet aircraft? . . . Do we believe that if they don't purchase them from us they won't be able to buy them from France or even Russia? Do we really believe that having jet commercial aircraft will jeopardize the security of the U.S.?. . .

Providing China with a modern airline with aircraft, ground equipment, airfield and navigational aids would be a real shot in the arm for our economy. We ought to sell what we can. . . . Why should ping pong players have to ride in DC-3's or coaI burning locomotives?

February 4, 1976

paper presented at the Annual Joint Meeting of the American Physical Society and the American Association of Physics Teachers

Chemical reactions give a few electron volts per interacting atom. Fission gives two hundred million electron volts per reacting nucleus. This factor of a hundred million has a favorable impact not only on the energy produced but also on the environment with regard to the amount of raw materials required and the wastes produced. A thousand megawatt coal plant produces six million cubic feet of ash per year, a fission plant less than a cubic yard.

Sooner or later the whole world will realize that they cannot turn their backs on the benefits of the nucleus. Today fission, hopefully in the next century fusion.

April 14, 1977

talk at Belgium American Chamber of Commerce Luncheon in honor of Dr. Agnew

... [Most of] the world's population. . . [has] great expectations. Fart of their expectations are due to the sort of instant discontent that we through the media have been beaming for many, many years. They expect in a very short time to achieve a standard of living that's commensurate with ours, and I would submit that we're not going to achieve this standard of living unless they have plentiful relatively inexpensive energy. This can be provided, but. . . only. . . through what I'll call technology. It's not going to be achieved through wishful thinking or abstinence in certain technologies.

April 19, 1977

letter to Congressman Jack F. Kemp

. . . I do not believe we can maintain a technology base or the necessary cadre of first-class scientists and engineers to enable the USA to have a nuclear weapons design capability for more than a few years if testing ceases.

September 8, 1977

testimony before Senate Foreign Relations Committee

. . . If it is the considered opinion of the Senate that the United States has no further needs now or in the future for new untested types of warheads having yields substantially greater than the 150 kilotons limit of this agreement, then the [threshold test ban] treaty [under consideration] will have no appreciable impact on our defense posture in the immediate future. However, if you believe that there will be requirements far new untested designs of yields considerably larger than 150 kilotons, then if this treaty is ratified our defense systems will eventually have to bear a penalty in payload weight, physical size, and perhaps even in the additional use of fissile materials. . . . It simply will not be prudent to put into the stockpile designs which represent a large extrapolation from tested designs...

I personally would not support any treaty further limiting nuclear testing until meaningful agreements on SALT and Mutual Balanced Reduction of Forces have been ratified. . . . I stress this relation to other arms control progress because we need some clear sign of Soviet restraint in their weapons build-ups and because our own nuclear posture must be appraised as a consistent whole. . . .

For those of you who may wish to remind me of the destruction caused by a nominal 15 kiloton bomb, may I remind you that I flew on the Hiroshima mission and have participated in the major thermonuclear tests which this country has conducted. As an aside, I firmly believe that if every five years the world's major political leaders were required to witness the in-air detonation of a multimegaton warhead, progress on meaningful arms control measures would be speeded up appreciably, October 2, 1977

talk at 1977 National Conference for Advancement of Research

I still remember when Seamans took over the AEC, he said, "ERDA will not be a warmed over AEC." He was right; except for the weapons program and a few other areas, it became a half-baked NASA . . . I believe the dismal track record of ERDA was due to the lack of appreciation of how fundamental [our] basic but relevant research is to the successful implementation of any development or engineering project. . . .

Hopefully, this attitude will not prevail [in] the DOE [under Schlesinger] . . . because of [his] past attitude when he was with the AEC. For tens of years under the most absurd secrecy . . . the AEC had been conducting research on centrifuges. Their engineering was superb, but their basic understanding . . . of how centrifuges really work, which involves complicated fluid dynamics, was lacking. After Schlesinger came on board. . . he simply directed that the weapons people, with their advanced, basic science capabilities in. . . fluid dynamics, be brought into the program. In a few months . . . the weapon design theorists attacked the problem, developed codes to analyze the action of the gas inside the centrifuge, and aIlowed the centrifuge to become a viable option. . . for uranium enrichment. Had Schlesinger not broken down the compartmentalization. . . the centrifuge developers would still be using an Edisonian, build-and-try technique with a six months turnaround time. . . .

Many people don't realize the. . . stimulus given to major scientific programs in the U.S. today, which started from work initiated through the weapon's supporting research program of the AEC . . . . Some originating at Los Alamos are:

1. SHERWOOD - controlled thermonuclear fusion
2. LAMPF - medium energy physics facility
3. ROVER - nuclear rocket research
4. LASER FUSION
5. JUMPER - laser isotope separation
6. VELA - nuclear test detection
7. SMES/SPTL - cryo-engineering
8. NUCLEAR SAFEGUARDS
9. GEOTHERMAL ENERGY

. . . the support of basic science is vital to any development work; it can't be programmed and micromanaged. It must be supported as if it were one of the art forms, which it really is.

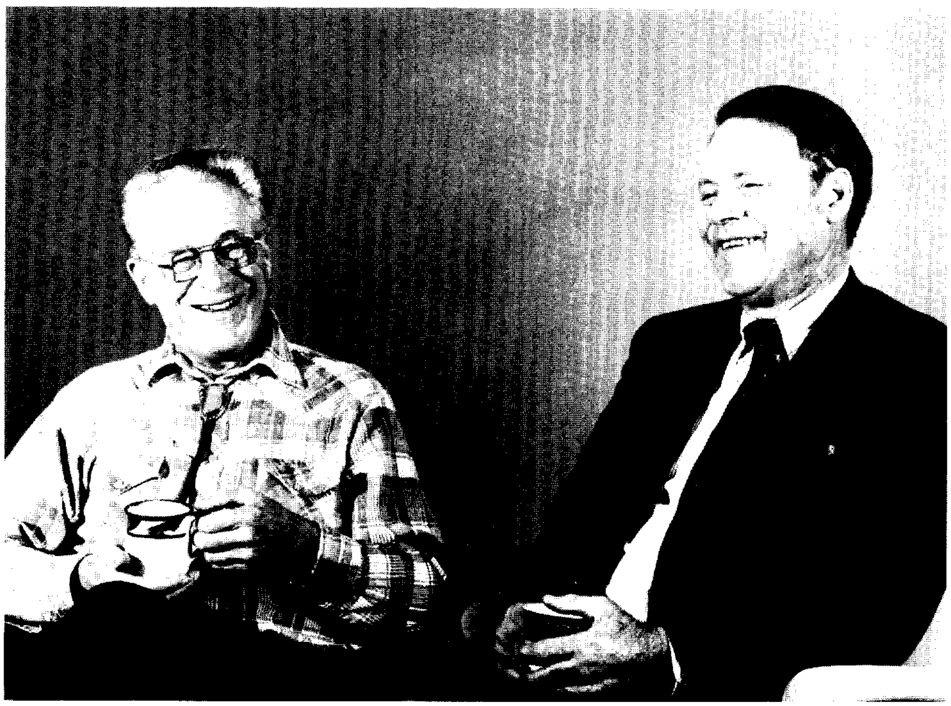
However, one can insist in these trying times, where we are confronted with specific problems, that for the most part research be conducted in relevant fields, but not that it be necessarily relevant today . . . . If one does not provide this freedom and enlightened management, then the country will end up with the run-of-the-mill, average, plodding, pseudo-research institutions, which will be busy supplying the last digit after the decimal point that is so dear to the handbook publishers. The innovative wild men and women who are always on the leading edge of science and technology will not be part of the team, And we need them.

1954

State Senate campaign slogan

"A person of integrity stays bought!"

The Times They Were a Changin'



Raemer Schreiber (left) joined the Laboratory in 1943. In the '50s he was the Leader of the Weapons and the Nuclear Propulsion divisions and then, in 1961, was appointed Technical Associate Director. He remained in that position after Agnew became Director until "Harold, in 1972, decided I was really Deputy Director, so he changed my title. " Robert Thorn (right), currently the Deputy Director, first joined the Laboratory's Theoretical Division in 1953. His numerous administrative positions included Theoretical Design Division Leader, Associate Director for Weapons, and, from March to July, 1979, Acting Director of the Laboratory.

SCIENCE: *Schreib, you were Technical Associate Director from 1962 to 1972 and as such were part of the transition between the Bradbury and Agnew eras. What do you feel was Agnew's vision of the Laboratory when he became Director?*

SCHREIBER: Only Harold can answer that question definitively. I do know he was always intensely proud of the capabilities of the Laboratory and did not feel that its expertise needed to be confined to nuclear physics, He was willing to tackle any scientific or technological problem worth solving. Generally he took the attitude, "If we don't have the experts, we can get them." You should remember that at this time reactor work was shifting over to commercial utilities, and the AEC was clamping down on new reactor concepts. Harold saw that the future of the Laboratory might well be in other directions than just pure nuclear physics.

SCIENCE: *Bob, you were the Theoretical Design Division Leader and then later Agnew's Associate Director for Weapons during the '70s. What do you feel he hoped to accomplish when he became Director?*

THORN: I think Harold felt we needed to regain the initiative in weapons development that we'd lost to Livermore. In 1970 this Laboratory was still largely a weapons lab, but Livermore was doing a better. more aggressive selling job and was pushing for the enhanced radiation weapons and all the strategic weapons—the nuclear warheads for Minuteman and Polaris. Their reputation was better than ours, or at least perceived to be so by some people. Harold's vision was to restore the luster that Los Alamos had lost. It's true that he thought the Laboratory was premier in all fields and he would undertake anything, but above all he wanted to be first in our principal mission of weapons development,

SCHREIBER: There's another aspect to the Bradbury-Agnew transition that I feel is also important to recognize, At the end of World War 11, when Norris became Director, a lot of people who had served during the war years on Laboratory advisory boards simply disappeared. Norris really didn't have an existing management structure to work with, so he was able to start with a clean slate. Twenty-five years later the Laboratory was firmly established, and Norris was working with a senior staff of people he'd worked with for years. He knew what they could do and what they were interested in doing, so he was able to take a low profile and run a fairly relaxed ship. But many of these people were also approaching retirement. Norris knew and they knew that major changes would have to be made in a few years. However, Norris did not want to make changes that would obligate the incoming director. When Harold took over he had the chance to assert his leadership at once. It was an appropriate time to reshuffle personnel and his reorganization took place over the first couple of years.

THORN: I agree. Both Oppenheimer and Bradbury operated with small staffs and were able to stay close to all aspects of the effort because there were only a very few major programs. For example, I think when

Harold took over there was the Weapons program. the Space Nuclear Reactor program, and the Fusion program. By the end of Agnew's directorate there were 600 programs! Harold realized that things were getting more complicated and set up two associate directors, one for weapons and one for research, to handle the technical programs. He inherited a Technical Board from Norris made up of the director's immediate staff, division leaders, and department heads, but as time went on this function was largely replaced by the associate directors working with their divisions,

SCHREIBER: In fact, Norris and Harold had different personalities, different approaches to management, and the Tech Board meetings show some of these differences. All major policy decisions under both directors were discussed or announced at these meetings. Norris' favorite technique was to state the question, perhaps offer some possible answers, and then sit back with his feet on the table and let people talk. He might pose some questions from time to time, but generally he let everyone have his say. Quite often a consensus would be reached, in which case he'd simply say, "OK, let's do it that way," Or there might be times when violent differences of opinion would emerge, Then he'd either rule one way or another or suggest that we adjourn and think it over some more. Harold preferred to research the subject first, make up his mind in advance, then announce his decision at a Tech Board meeting. He would listen to contrary arguments to see if anyone really couldn't live with the decision. As a result, he might modify his stand, but he did not encourage prolonged debate.

Harold could be fairly hard-nosed when it came to the shuffling of senior personnel. Perhaps he had to be since he was dealing with entrenched incumbents, but he also believed that the future of the Laboratory depended on bringing in fresh people with new ideas and on rotating responsibilities to provide management training. This was a deliberate stirring of the Laboratory by Harold. and he put his priorities for the Laboratory above the feelings of those displaced. On the other hand, he was quite compassionate in dealing with hardship cases anywhere in the Laboratory.

Agnew at Tinian in 1945.

One thing was the same under both directors: it was implicit that management get their jobs done without formal directives or instructions. The general attitude was, "If I have to tell you how to do it, you shouldn't be holding down that office."

SCIENCE: *How did management change from the beginning to the end of the Agnew era ?*

SCHREIBER: It got more complex. Because of the small number of major programs, interdivisional coordination under Bradbury was handled by steering committees or working groups usually chaired by one of the division leaders. As a result, program direction was quite decentralized and the Director's staff was small. But then the AEC discovered "program direction," which is a polite way of saying that it was building its staff to participate more directly in calling the shots out at its laboratories. Moreover, it was subdividing its budget and personnel to enforce compliance with its directives. This process has continued through the ERDA and DOE regimes and is largely responsible for the large growth in administrative positions in the laboratories themselves.

For example, the Budget Office under Bradbury had two men and a secretary. Harold had to set up the Financial Management Office which grew to about fifteen to eighteen people. Periodic reports and what were called Form 189's were required for every project. This resulted in an enormous amount of bookkeeping, so the accounting office had to grow. There were a number of requirements from Washington that Harold at first just flatly refused to comply with. He won some of these, but lost others.

THORN: In fact, by the end of Harold's tenure it was obvious to many. including

Harold and Norris about the time of the transition between the two directors in 1970.

Harold, that substantial management changes had to be made. The changes were largely necessary because of the increase in programs, program direction from Washington, and accountability. As a manager, you had to control and review the yearly proposals to make sure that they went to Washington in the proper form and that they were the kind of thing the Laboratory wanted to do. In addition you had divisions over which you had to exercise line management. So you were both program manager and line manager. And then you presumably were supposed to remain technically competent. It was just too much to do—too much for a director and two technical associate directors to do, Harold wisely held reorganization in abeyance and allowed his successor. Don Kerr. to implement his own management system.

SCIENCE: *Bob, getting back to Agnew's desire to regain the initiative in weapons development, what were the major accomplishments in the Weapons program in the '70s ?*

THORN: When Harold took over, Livermore was responsible for the development of all the strategic missile warheads, which were the big prestige items in the eyes of the public and the Defense Department. But Harold fought vigorously to acquire new warhead responsibilities.

SCHREIBER: Harold was a very aggressive salesman.

Thorn: Yes. He started the Weapons Program Office and the Weapons Planning Office. These were supposed to be part of what you might say was our marketing group. By backing up this group with the technical people in the design and engineering divisions, we could be more aggressive about going out and getting these weapons systems. He also tried to reinvigorate the Weapons program here by splitting the old Theoretical Division—the design part away from the theoretical physics part—so as to provide more emphasis to weapons design. As a result of these efforts, we were awarded responsibility during his tenure for the W76 used in the Trident warhead, the W78/Mark 12A used in the Minuteman III warhead, and the W80 used in the air-launched cruise missile warhead. Also, the Laboratory introduced the first enhanced radiation bomb into the stockpile and developed new versions of the air-carried B61, a general purpose bomb and warhead for short-range attack missiles. One of the weapons developments that Harold felt most proud about was the introduction of insensitive high explosive that makes the stockpiled weapons containing it much safer to handle. An accidental detonation that scatters radioactive plutonium becomes highly unlikely.

SCHREIBER: Another point is that Harold took over at the time when the national emphasis was shifting from aircraft to ballistic missiles, so the major weapon developments were aimed at matching the bomb to these new carriers. Microelectronics and the ability to communicate or to install elaborate instructions in missiles opened a new era in the mating of warhead to delivery system. Ideas such as smart missiles that could track a target or the concept of multiple independent re-entry vehicles (MIRVs) were growing. These ideas required new weapons, but not in the sense of changing the basic physics of the innards of the device. Rather they were new weapons in the sense of changing the configuration to match size, weight, and shape requirements of the missile warhead or in changing how the weapon was told to behave to match the safing, arming, and fuzing requirements of the delivery systems. These requirements led to significant and detailed changes involving highly intricate engineering of the warheads. Also changes were made to improve yield-to-weight ratios and to extend the useful stockpile lifetimes of the warheads. Because of the necessarily close relationship between warhead and delivery system, this period was one of very intensive collaboration with the

Defense Department.
THORN: The collaboration was revitalizing. Originally I think Los Alamos slipped because many of the people here had been in the business since the beginning—twenty-five years—and some of them had grown tired of the arms race. Their attention shifted to diversifying into other fields. As a result, the Laboratory was not putting the kind of attention into weapons development that a weapons lab should be putting into it. After all, we're not here to argue for arms control, we're here to design weapons. But in this period we started to participate more actively with the Defense Department, both by designing to meet their stated weapons needs and by developing our own ideas and trying to sell them.
SCIENCE: *The diversification into nonweapons programs, then, did not start with Agnew?*
SCHREIBER: In one sense, yes. There was a strong effort under Bradbury to diversify into nonweapons applications of nuclear energy, but this was generally limited to nuclear reactors and nuclear fusion. In the '60s there was considerable encouragement by the AEC to try out all sorts of ideas for building reactors, and Los Alamos had projects in nuclear rocket propulsion, the thermionic reactor for generating electricity directly. the graphite-based, ultra-high-temperature reactor, reactors in which the fuel was molten at operating temperatures, and so forth. It was a time when anybody who had an idea that would stand up under peer scrutiny could try it out. But, as I said earlier, about the time of the Bradbury-Agnew transition there was a budget squeeze, and the AEC curtailed support of new reactor work to concentrate on the commercial development of the light-water reactor and on research and development of the liquid-sodium-cooled breeder reactor. This created an immediate need at Los Alamos to find other activities for many of the people who had been in the field of reactor development.

Harold with Edward Teller in 1973.

Part of the need was satisfied by a push into energy programs. For example, the potential of lasers to do isotope separation and to initiate fusion reactions was brought to Harold's attention, and he authorized an immediate expansion of this work. A bit later the oil crisis of '73 and '74 stimulated interest in alternative energy sources, and that led to substantial programs in solar energy, hydrogen as a fuel, and hot dry rock geothermal systems. Other energy programs included synthetic fuels, fuel cells, and superconducting transmission lines. Our large computer facility made possible demographic and socio-economic studies of energy resources and energy distribution.
THORN: In fact, the push into the energy programs during the '70s was so vigorous that the Laboratory, rather than shrinking, almost doubled in size. Harold had correctly recognized that times were changing. He responded by infusing the Laboratory with a spirit of experimentation based on the expertise we'd acquired over the years dealing with multidisciplinary problems in weapons research. It was a period of excitement and challenge.

It was also true that many of the programs were unrelated to our principal mission, and the Laboratory lost a great deal of the cohesive spirit that bound it in its first twenty-five years. What happened was that in response to the energy crisis the AEC had its charter broadened: it could look into other energy programs besides nuclear. The government thought the way to solve the energy problem was with an influx of money, and the fastest way to get started was at the level of the national laboratory. Of course, they found some eager people here quite willing to work on these problems. But as far as having any overall coherent plan—that was missing! The result at the Laboratory was a multitude of programs. When everyone had been paid from the same source—the weapons program—you could

Harold spearheaded the drive for the Laboratory's National Security and Resources Study Center, shown here under construction in 1976.

walk up to somebody, ask him to do something, and he'd get it done. Today you ask, and he'll say, "I can't do that. I'm working on another program, and my sponsor won't allow me to work on yours unless you give me some money." That's an example of what I mean by a loss in the spirit of cohesiveness.

SCIENCE: *What were some of the outstanding nonweapons programs under Agnew?*

SCHREIBER: Well, as I mentioned before, laser fusion and laser isotope separation were initiated by Agnew. A great deal of excellent research has come out of those programs. There's LAMPF—the Los Alamos Meson Physics Facility—which was conceived in the Bradbury years, then realized in the Agnew years. LAMPF, of course, is a story in itself.

We have the new plutonium facility, which is the finest plutonium research and development facility in the country, perhaps in the world. That such a facility was necessary had been recognized at Los Alamos for years, but Harold was the one who convinced the AEC. The old DP site had been built in a hurry as a temporary facility and was being kept in a safe operable condition at considerable maintenance cost. So first the AEC had to be made aware that something should be done. If they were just going to shut the old site down, what then? There were two other reasons the decision was held up: environmental requirements had been changing so that it was hard to pin things down, and it was going to be a very expensive bit of construction because of the need for safeguards and protection against everything from a laboratory fire to an airplane crashing into the building. In essence the AEC was committing itself to having all plutonium research done at the new facility wherever it was built. Much of the selling was to point out the expertise in plutonium research that already existed here at Los Alamos. Construction of the new facility finally started in 1974.

The hot dry rock geothermal concept was an outstanding program under Agnew. Morton Smith should be given credit for initiating and selling this one—he probably made two thousand speeches on the subject. As I recall, preliminary exploratory work had been authorized by Bradbury, but a full-scale effort was not mounted until later when manpower, including chemists and materials fabrication people, became available when the Rover (space nuclear reactor) and UHTREX (ultra-high-temperature reactor) programs were halted.

In a similar vein, work on reactor safety analysis was a natural spin-off from the various experimental reactors that had been designed and built here. People who had been in the UHTREX and LAMPRE (molten plutonium reactor) programs and who were familiar with the safety requirements of reactors moved into that field.

THORN: I agree, Schreib, except I would attribute the reactor safety program more to Kaye Lathrop and other theoreticians who were using large computer codes for weapons simulation and started developing similar codes for reactor safety analysis. They expanded weapons transport codes by adding the appropriate equations of state, accounting for two-phase flow of water and steam, and so forth. But more important, they brought with them the experience of using large codes to model complex problems.

In contrast to many of the other nonweapons programs. the nuclear energy programs at Los Alamos have always complemented the weapons effort. Much of the work involves transport codes used in weapons calculations or involves the plutonium facility or provides useful neutronics data. In that sense, these programs have been cohesive, not divisive.

SCHREIBER: Nuclear Material Safeguards was another outstanding program: it was well under way toward the end of Bradbury's stewardship, then was expanded under Agnew. I was directly involved in its development but can take little credit since Bob Keepin was the founder and chief salesman. He badgered me into authorizing a small initial program, then parlayed that into a major effort by selling it to key officials in the AEC. He acquired equipment and laboratory area from defunct reactor programs using the "camel in the tent" approach. This approach comes from the old Arab story in which the camel outside the tent says his nose is freezing, so the owner tells him he can stick his nose in, then the camel says his ears are freezing, and so on. Bob used a lot of the equipment from the defunct UHTREX, including a building adjacent to it that had been built for reactor experiments. But the real success was the fact that he recognized a very real need—accountability and safeguards for fissionable materials—and then did something about it.

SCIENCE: *What about the theoretical effort?*

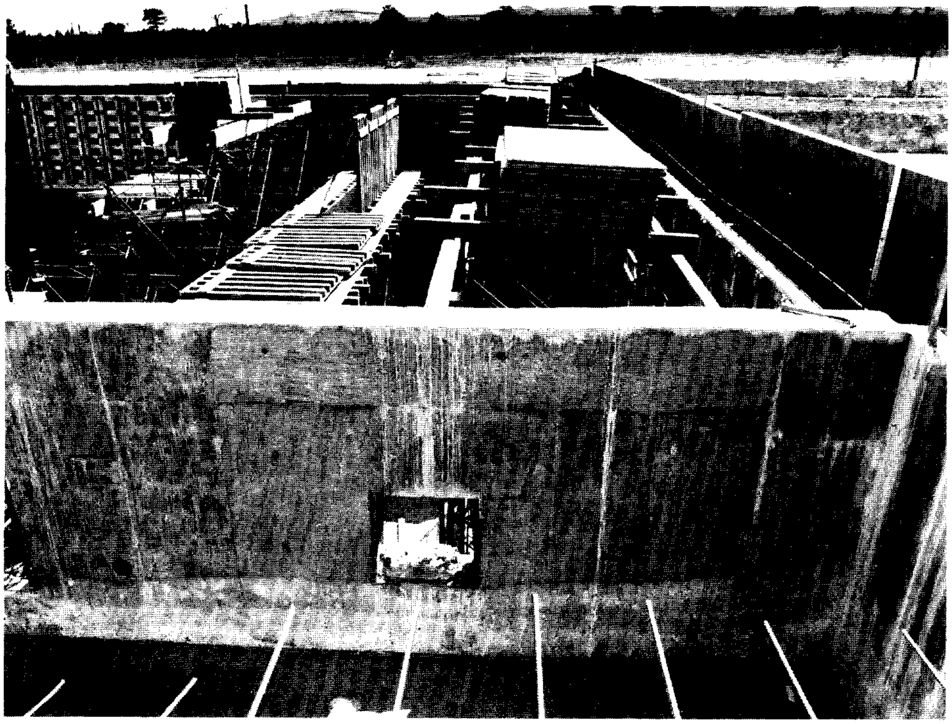
THORN: Well, Harold, although he was an experimentalist, respected theoretical physics, and he wanted a first-class theoretical research effort in the Laboratory. Peter Carruthers was hired by Harold and given that charter, which Pete was largely able to fulfill. Also, Harold started the Laboratory Fellows program to help bring eminent external scientists to the Laboratory. Early Fellows were Herbert Anderson, Richard Garwin, Gian-Carlo Rota, Bernd Matthias, and Anthony Turkevich. This program has been continued and expanded under Kerr, who has also instituted a Fellows program composed of outstanding scientists within the Laboratory. And there was a major expansion in computing under Harold, including purchase of the first Cray computers.

SCHREIBER: One of Harold's objectives was to find ways to finance the growth of basic research, including the theoretical efforts, up to a level of perhaps ten percent of the total Laboratory effort.

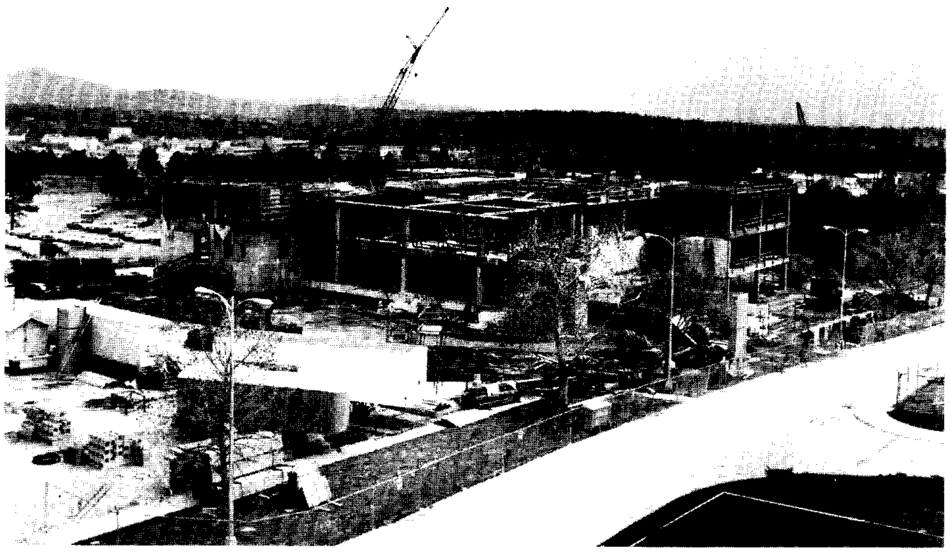
SCIENCE: *How did the funding sources and amounts change during this period?*

SCHREIBER: As we've already indicated, budgeting was not a major problem for most of Bradbury's tenure because the money came in a few large chunks accompanied only by general directives. However, the AEC eventually began to exert its muscle in program direction, and then the Laboratory had its first budget crisis in the early '70s with the cancellation of the UHTREX, LAMPRE, and Rover programs.

THORN: Essentially the entire experimental reactor program was wiped out, then Rover. plus there were cuts in the weapons program. The first thing that Harold did was to say, "Let's do reimbursables. Besides the AEC we'll work for the Defense Department, we'll work for any other federal agency." Harold was never just negative about a situation; he always had a solution or two. The idea of reimbursables was an important solution that not only helped the Laboratory survive a crisis, but opened new doors such as



Harold helped convince the AEC of the absolute necessity for a new plutonium research and development facility. Construction started in 1974.



The Helios facility was constructed during the mid '70s to further explore the use of the CO_2 laser as a driver for inertial confinement fusion. Helios is an eight-beam system with an output of 10 kilojoules in 1 nanosecond.

developing productive ties with industry.

SCHREIBER: The Laboratory had already done a limited amount of reimbursable work, but mostly at the initiative of the sponsor of the work. With the AEC cutbacks, active solicitation of reimbursable work was started and a full-time employee was assigned to sell the ideas. In the early period, this was encouraged by the AEC. However, when reimbursable work grew above ten percent of the AEC budget to the Laboratory, worries were expressed about possible wholesale layoffs if, for any reason, reimbursable work stopped. Most of the contracts were for a period of one or two years, so the worry was real, both to the AEC and to Laboratory management. An informal compromise was reached with the agreement that reimbursables would be held approximately to the ten-percent level.

As matters turned out later in the '70s, the AEC budgets grew and the Laboratory continued to expand. However, it 'was not all that easy. Each year's budget was a cliff-hanger, but Harold was an excellent salesman and knew how to bargain successfully.

THORN: He was indefatigable. He understood that good public relations were becoming necessary. He was good at it, but he needed to be. He traveled extensively, addressed groups, served on committees, and maintained contacts with Congressional delegations.

SCHREIBER: Considering the wholesale cuts at the beginning of the '70s, the Laboratory definitely needed that kind of effort.

THORN: Harold never stopped believing in or selling the expertise and the potential that exists in this Laboratory and its people. ■

The Laser Programs

by Keith Boyer

The laser programs of Los Alamos had their inception in 1968 when I was directing the test activities of the Nuclear Rocket Propulsion program (Project Rover) in Nevada. At that point decisions were being made that would shift much of the program's test activities over to Aerojet and Westinghouse, and it was an appropriate time to explore new activities.

The main concept of the Rover nuclear rocket was to generate a high-temperature exhaust stream for propulsion by passing a gas, such as hydrogen, through the hot core of a nuclear reactor. However, I thought that a system based on fusion rather than fission might provide an extremely high-temperature exhaust stream for efficient propulsion. One possibility was the "Orion" concept in which a series of thermonuclear explosions "pushes" the spacecraft by ablating a replaceable layer of material, such as water, off a pusher plate. This process could produce high thrust and a very high efficiency system.

But what would ignite the thermonuclear explosions? Because of my interest in lasers, I was aware of the development of a high-energy carbon dioxide (CO_2) gas-dynamic laser system by the Air Force Weapons Laboratory. Our calculations indicated that if the energy then predicted for this laser could be released in a short enough time (about a nanosecond) and focused uniformly onto a small pellet of thermonuclear fuel, an efficient fusion process might be achieved.

Another feature that made the gas-dynamic laser attractive to our program was the manner in which the laser's population inversion was generated. The CO_2 gas was pumped to higher energy states by heating the gas, then the inversion was formed with rapid cooling through an expansion nozzle. Our early systems could use the Rover reactor as the heat source for driving this laser at high energies. Thus, the investigation of laser fusion seemed appropriate. The Space Nuclear Propulsion Office in Washington agreed, and a modest effort was started that year at the Nevada Rover test site. Of course we recognized that fusion as a commercial energy source was the most important application and one that would surely precede any propulsion application, but we had found our first sponsor.

Design studies soon revealed difficulties in achieving the desired short, high-energy pulses at the low CO_2 pressures necessary in the gas-dynamic laser system, so other pumping mechanisms for the laser were considered, including optical, electrical, and chemical energy sources. Also, more information was needed about the effective absorption efficiency of the laser energy by appropriate targets, about the physics of the interaction process, and about energy transport and utilization in initiating fusion.

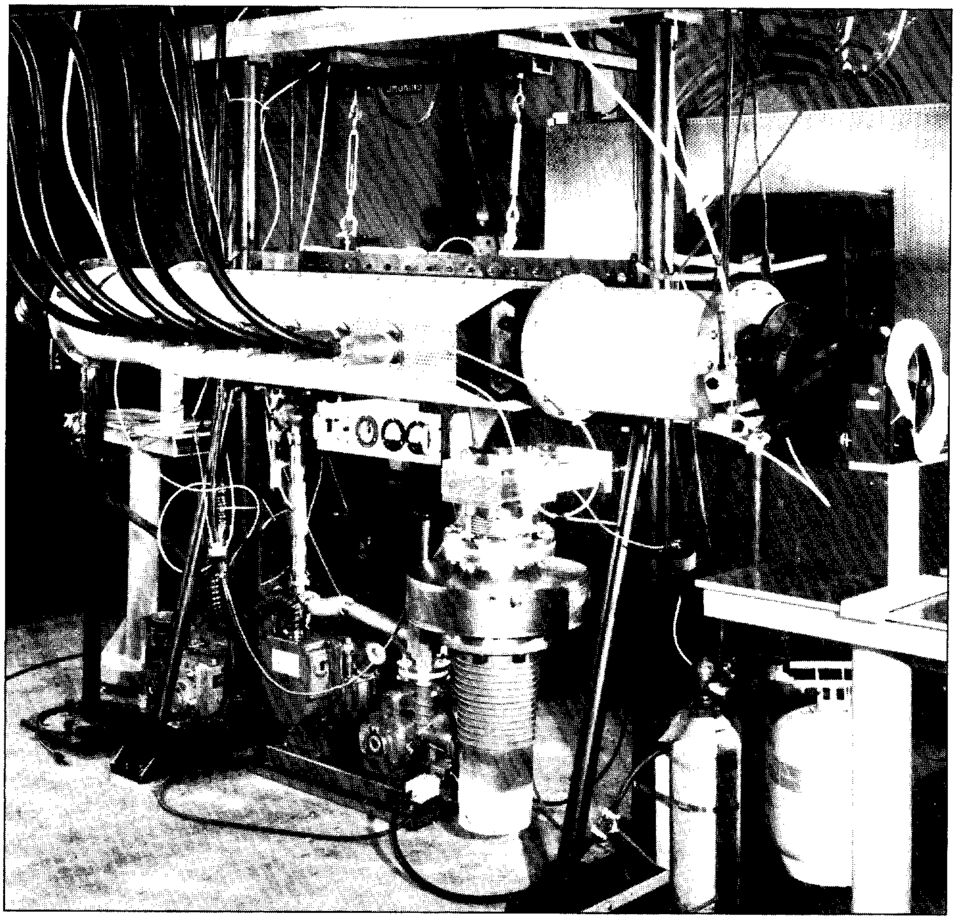
Raymond Pollock, a weapon designer, agreed to collaborate on this study and was able to derive the scaling laws and calculate the requirements to achieve thermonuclear burning of small pellets of fuel by assuming ideal interaction physics of the laser light with the target.

In early 1969 Bill Ogle, then the Weapons Testing Division Leader, agreed to authorize a small experimental exploratory effort. This activity included about ten staff members and initiated a three-pronged experimental effort: development of a one-joule, picosecond glass laser for the light-target interaction studies, investigation of electrical-discharge-pumped CO_2 lasers that could be scaled to high energy, and development of chemical lasers. Although chemical lasers would serve as backup for the undeveloped CO_2 laser, we intended to pursue both laser development and laser applications, and we recognized the potential of chemical lasers for studying photochemistry. For the CO_2 laser one of the early innovations, in which Charles Fenstermacher played a key role, was an electron-beam-controlled discharge capable of pumping large volumes of high-pressure CO_2 gas.

A year later we had established estimates of key parameters for laser fusion, such as laser energy, pulse width, and preliminary pellet design. We were able to outline a program designed to determine the feasibility of laser fusion, including several different laser options. About this time we became aware of other programs in various parts of the world, including those at Livermore, Sandia, the University of Rochester, the Lebedev Institute in the Soviet Union, and the Osaka University in Japan, but all of these were based on glass lasers. Moreover, apparently only the Los Alamos and Livermore programs initially considered a target design that used laser energy to compress the fuel strongly as well as to heat it, a technique that reduced the laser energy required by many orders of magnitude. This situation changed soon as the various programs, including a new one at KMS Fusion (a commercial venture), discovered the necessity of compression.

Harold Agnew, recognizing the importance of developing new and promising activities at Los Alamos, asked me in January 1971 to set up an expanded laser program. This program was run out of the Director's Office in order to enlist Laboratory-wide support. Our effort soon had a wide base of activities, including a theoretical group organized by Richard Morse in the Theoretical Divison; an interaction physics and target group under Gene McCall, who played a key role in the Laser Fusion program; a CO_2 laser development group under Fenstermacher; a glass laser group under Dennis Gill; and a chemical laser group directed by Reed Jensen. A series of seminars was established to review the existing state of laser technology and interaction physics and to explore new applications such as laser photochemistry.

By early 1972 the program had achieved sufficient size and complexity so that a new Laser Division was established. Two new groups were added, one on laser applications and one on target fabrication. At this time the first large CO_2 laser chain was being built and plans were in progress for a series of CO_2



One of the amplifiers used in the early '70s in a $C0_2$ laser chain that generated a l-nanosecond, 0.5-kilojoule pulse.

laser systems of increasing size, including a two-beam, 2-kilojoule laser later called Gemini; a six-beam, 10-kilojoule laser now operating under the name of Helios; and a 100-kilojoule system whose configuration was being debated and which evolved into the present Antares system.

The early interaction data was obtained using a 50-joule, picosecond glass laser, Meanwhile, work proceeded on development of a larger 500-joule, glass laser system. Frequency-conversion crystals were also planned to be used with this laser to give green light and ultraviolet light, although at lower energies. These latter frequencies were needed to explore fully the question of the most efficient wavelength for the laser fusion process, a question that has not yet been resolved. The chemical laser work proceeded with the development of hydrogen fluoride lasers, which promised to provide the highest energy output of any laser system.

A coordinating committee was established in Washington to provide guidance for the laser fusion programs in the United States with representation from the Division of Military Applications of the AEC, the Magnetic Fusion program, and the heads of the various AEC Laboratory laser programs. The Los Alamos budget approximately doubled each year through the early '70s,

Our plan to pursue a broadbased laser technology program included a small project in the Chemical Laser Group to investigate the use of laser energy to separate uranium isotopes. This particular activity captured the interest of Paul Robinson, who had transferred from the Rover Reactor Division, together with a number of other staff, as the Rover program decreased in size. Paul had earlier been active in the gas-dynamic laser effort in Nevada and now, together with Reed Jensen, played a major role in the isotope separation project. The separation was based on the photolytic dissociation of uranium hexafluoride vapor cooled by a supersonic expansion to permit isotopic selectivity using a combination of infrared and ultraviolet laser photons, This activity continued to grow until it was split off from the Laser Division as the Applied Photochemistry Division with Paul as Division Leader. This division also became involved in both high-repetition-rate, high-power laser development and in broad aspects of laser photochemistry. Projects included high-resolution laser spectroscopy, photochemical processing, laser sound generators for potential military uses, and chemical and biological warfare agent detectors. Although a recent Washington decision terminated the Los Alamos molecular uranium isotope separation process in favor of the Livermore atomic vapor process, the molecular process was close to engineering demonstration and was judged by many of us to be the superior process. In spite of the uranium decision a growing Los Alamos program on the separation of plutonium isotopes is doing well.

The laser fusion programs are still vigorous, but many problems have developed, and the final utility of laser fusion for energy production remains uncertain. Inflation and budget stretchouts reduced the design energy of the Antares $C0_2$ laser, which has just begun its checkout phase, from 100 to 40 kilojoules, The estimates of laser energy needed for a useful thermonuclear yield have risen from a few hundred kilojoules to a few megajoules. The longer wavelengths of both CO_2 and glass lasers produced undesirably large hot-electron components in the absorption process. The resulting self-generated magnetic fields are believed to reduce the lateral heat conduction that was originally counted on to symmetrize the implosion of the fuel pellet. Shorter wavelengths appear to be more satisfactory, and work is proceeding on ultraviolet excimer lasers, such as krypton fluoride, but the optics problems for these wavelengths are severe. Glass lasers can be frequency shifted to the third harmonic with good efficiencies, although the basic efficiency of the glass laser itself is too low to provide the driver for a laser fusion reactor. However, this technique is being pursued at other laboratories.

The Los Alamos program is now emphasizing investigation of physics problems of interest to the weapons programs, Because this effort appears to be increasingly productive, program funding and support is expected to continue. In spite of the apparent difficulties associated with the long wavelength of the $C0_2$ laser, it may be possible to find clever target designs that permit the many advantages of this laser to be used for successful initiation of the fusion process. Other laser activities, such as the Free Electron Laser program, are now expanding both the Laboratory's interest in and its commitment to laser technology. ■

The Reactor Safety Program

by Kaye D. Lathrop

Although Los Alamos has had a long history of individual contributors to the safety of reactors, including Hans Bethe, George Bell, and William Stratton, the reactor safety research program now conducted by the Energy Division began in 1972 in the Theoretical Division. At that time, in reactor physics and safety circles, there was a slowly increasing realization that our ability to predict the consequences of possible reactor accidents was woefully inadequate. The safety review process for the Fast Flux Test Facility at Richland, Washington had resulted in a heated and prolonged debate between the safety analysts at Argonne National Laboratory and the construction project managers at Hanford because the results of the safety analysis implied greatly increased design and construction expense. Somewhat earlier, the first major performance tests of a simulated light-water reactor emergency core-cooling system at the Semiscale Facility at Idaho Falls gave an unforeseen result. The emergency cooling water, instead of penetrating the core and cooling the system, simply flowed around the upper annulus of the apparatus and exited through the simulated pipe break. Although the Semiscale apparatus was about one-thousandth as large as an actual reactor, these disturbing results precipitated a lengthy set of hearings that culminated in a Code of Federal Regulations that limited the operating temperatures of existing and future reactors. Because of a lack of understanding of what would happen in a full-size reactor, these regulations embodied many "conservatisms" and in this sense were arbitrary.

So there existed a desperate need for an analytic predictive capability, especially because expense had prohibited and always would prohibit complete full-scale testing of safety systems. Jay Boudreau, William Reed, and I, members of the Transport Theory Group of the Theoretical Division, saw this need as an opportunity, each in a different way. Boudreau, who had written his doctoral thesis on possible supercritical configurations that might emerge from core rearrangements during fast reactor accidents, wanted to turn from his transport theory assignments to solve what he believed were truly important problems. Bill Reed, who had already demonstrated a brilliant mastery of computational transport theory, was anxious to extend his talents to hydrodynamics. And I had an implicit faith in the ability of a properly designed computer code to make correct predictions and was anxious for a new challenge. Further, in the reduction-in-force days of the early seventies, I needed new financial support for my group.

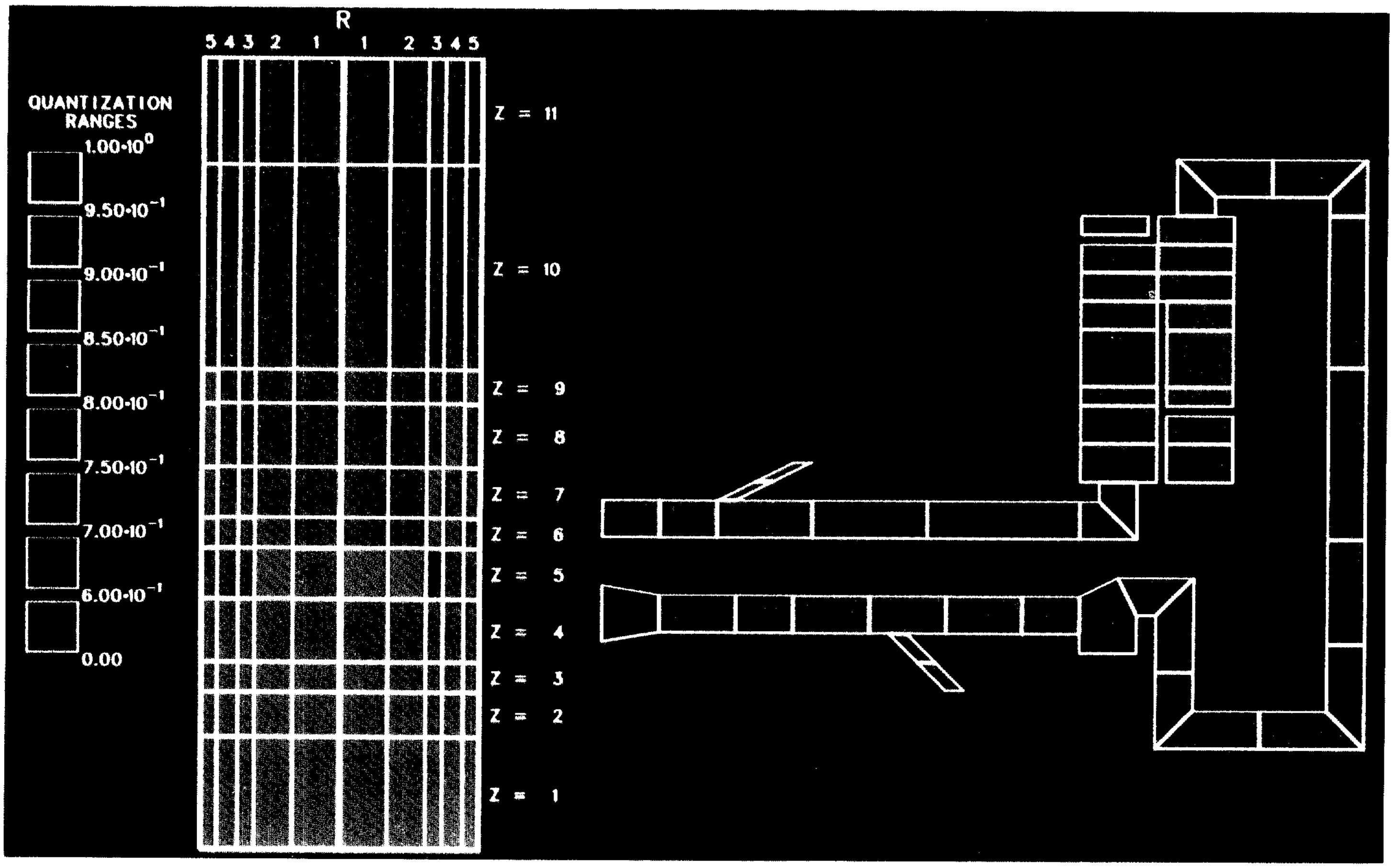
In my first 1972 foray to Washington, I was greeted by a skeptical branch chief with the sally, "Who are you, and what are your credentials?" However, in a widely attended Washington meeting on October 31, 1973, we presented a detailed proposal, authored by Jay Boudreau, Frank Harlow, Bill Reed, and Jack Barnes, for the development of the SIMMER (an acronym for S_n, implicit, multifield, multicomponent, Eulerian, recriticality) code to analyze fast reactor core-meltdown accidents. Although Los Alamos was outside the reactor safety community, the Laboratory's acknowledged leadership in computational methods and the existence of three groups in the Theoretical Division devoted to transport theory, hydrodynamics, and equation-of-state research convinced the AEC of our competence,

The proposal was funded, and work on SIMMER began in earnest in 1974. That same year, William Kirk and I began a more broadly based reactor safety research program on high-temperature gas-cooled reactors. Simultaneously, and almost as an afterthought, Reed and I agreed to develop a best-estimate computer code (subsequently named TRAC for transient reactor analysis code) to predict the effects of emergency core-cooling systems in light-water reactors. In retrospect, our self-confidence was astounding. We were blissfully ignorant of the difficulty of the task, and Los Alamos, despite long experience with high-temperature gas-cooled reactors and fast reactors, had no expertise with light-water reactors.

The Transport Theory Group grew rapidly in 1974 and 1975, becoming three groups in December of the latter year. Two of these groups formed the nucleus of the present 125-man reactor safety program in the Energy Division. The research of this program is the theme of the Summer/Fall 1981 issue of *Los Alamos Science.* The third group, headed by Warren Miller, remained as the Transport Theory Group of the Theoretical Division.

The success of the SIMMER and TRAC computer codes has been especially noteworthy because they must extrapolate. That is, they must make believable predictions outside the domain of experimental results. Versions of TRAC, in particular, have been used to predict results for dozens of experiments on many reactor components of scales up to full size and on integrated systems of various miniature scales. (The only full-scale, full-system data point for a light-water reactor emergency cooling system is Three Mile Island.) TRAC has a convincing predictive record. No other computer model of similar complexity, certainly not those of weapons design codes, can extrapolate with such confidence. SIMMER, while not yet as exhaustively compared with experiment as TRAC, has made two valuable predictions. First, contrary to previously accepted dogma, secondary and subsequent critical configurations can occur because of a core rearrangement during the course of a fast reactor accident. Second, and notwithstanding this first prediction, the energy released (and hence the containment expense) in fast reactor core-melt accidents is computed to be much less than previously predicted.

In addition to these technical achievements and of equal importance, the growth of the reactor safety program brought to Los Alamos many extremely capable people. These include Jim Jackson, who came from



Two examples of TRAC results. The graphic output shown here is color coded (left) according [o the fraction of vapor or steam in each computational cell. One example (middle) shows liquid water (blue) in the bottom of a pressurized-water reactor vessel filled with steam (red) following a postulated complete break in the largest coolant pipe leading into the vessel. The unique ability of TRAC to analyze 3-dimensional fluid motions in a vessel coupled to a full reactor system is proving valuable in addressing a wide variety of possible accidents in pressurized-water reactors. The output on the right shows steam-water flows in a loop of the Upper Plenum Test Facility (UPTF). Now in the design stage, this West German facility will include a full-sized vessel and several coolant loops to allow accurate simulations of fluid behavior during the core-reflooding stage of a large-break loss-of-coolant accident in a pressurized-water reactor. TRAC is being used extensively in the design of UPTF as part of a $300-million cooperative program among the United States, Japan, and West Germany.

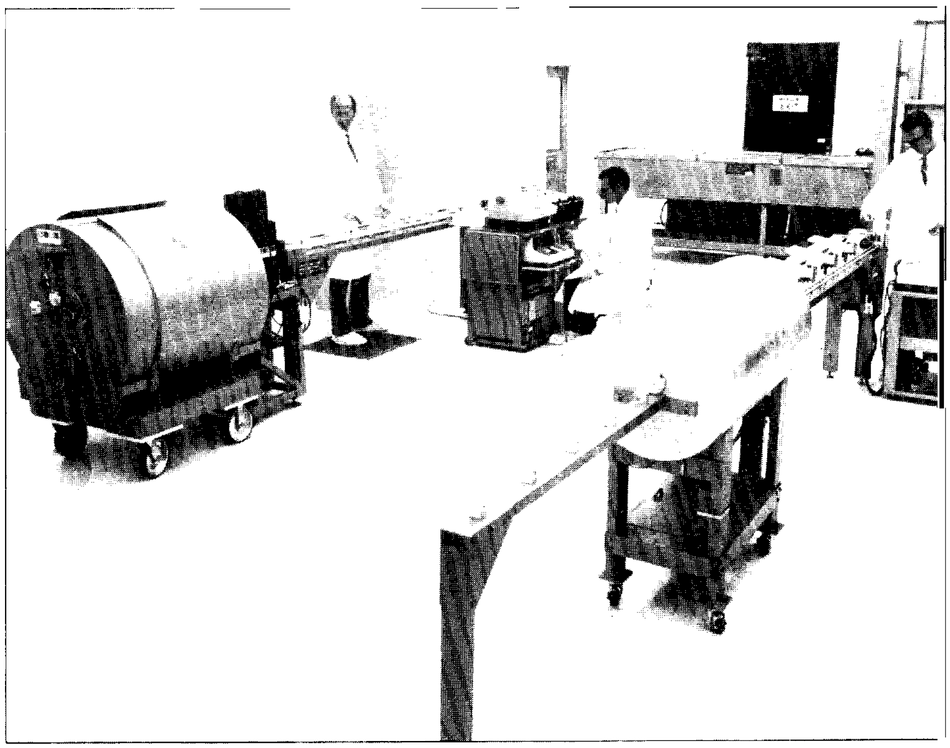
Brigham Young University to take charge of TRAC development during a crucial phase and is now head of the Energy Division; his deputy, Mike Stevenson. who came from Babcock & Wilcox via Argonne to head the high-temperature gas-cooled reactor analysis effort: Charlie Bell, who came from Atomics International to solve SIMMER heat-transfer and hydrodynamics problems; Walt Kirchner, who finished his doctorate at MIT in time to write TRAC heat-transfer routines: Dennis Liles, an expert in two-phase flow hydrodynamics from Georgia Tech who has been invaluable to TRAC development: John Mahaffy, a postdoctoral astrophysicist from the University of Illinois whose numerical hydrodynamics expertise has made TRAC faster; Rich Pryor, a Savannah River reactor physicist whose experience with methods and large codes was very valuable; Jim Scott, a Hanford fuel-behavior specialist; Ron Smith, from Argonne: Ken Williams. from Georgia Tech: Dominic Cagliostro, from SRI; John Ireland, from General Electric; Thad Knight, from EG&G; and many more. ■

The Nuclear Safeguards Program

compiled by Darryl B. Smith

"Los Alamos's interest in safeguards . . should not really surprise you. Our pioneering work in nuclear weapons has left us. . . with the profound concern that these devices never get used in anger, never get used surreptitiously, never get made by surprise. by theft. or by diversion," Dr. Norris E. Bradbury used these words in his welcoming remarks to the more than three hundred and fifty participants in the Second AEC Symposium on Safeguards Research and Development held in Los Alamos in October 1969.

Immediately following the end of World War II there was a hope that the proliferation of nuclear weapons could at least be delayed by means of rigid controls over all nuclear activities (the Baruch Plan, 1946). Despite efforts by the United States to maintain strict secrecy, by 1952 three additional nuclear weapons states had emerged, and several nations were seeking the benefits of nuclear electric power. In 1953, President Eisenhower announced the "Atoms for Peace" program to promote vigorously the peaceful use of nuclear energy while discouraging or preventing any military use. In the course of implementing this policy, the International Atomic Energy Agency (IAEA) was created in 1957 and entrusted with the international promotion and control of peaceful uses of nuclear energy.



Nondestructive assay of fast breeder reactor fuel. Two fuel-rod scanners developed by Los Alamos are being used here in 1974 at the Hanford Engineering Development Laboratory as part of their safeguards and quality control. The device on the l@ uses a computerized californium-252 system to measure both plutonium content with an accuracy of better than 0.5 percent and pellet-to-pellet uniformity of fissile material loaded into the rod. The system on the right uses a passive neutron-coincidence technique to measure plutonium-240 content, thus providing a cross-check with the first instrument.

The Los Alamos Nuclear Safeguards program began in 1966 when worldwide interest in nuclear energy for the production of electrical power was rapidly expanding. Bob Keepin, a nuclear physicist in the Nuclear Propulsion Division, had just returned to Los Alamos after two years as head of the Physics Section, Division of Research and Laboratories of the IAEA in Vienna, Austria, and was firmly convinced of the coming importance—both political and technical—of the worldwide nuclear safeguards problem. He was equally convinced that Los Alamos should launch a vigorous program to develop new nondestructive assay techniques and instruments that would in time provide the technical basis for meeting the increasingly stringent safeguards requirements that were inevitable. Following a lengthy series of briefings, hearings, buttonholing, and budget reviews with the AEC and the Congressional Joint Committee on Atomic Energy, the nation's first research and development program in safeguards was funded and launched at Los Alamos in December of 1966. Six months later, the AEC established a new Office of Safeguards and Materials Management (OSMM) as well as a Division of Safeguards in its Regulatory Branch. The Regulatory Branch is now the Nuclear Regulatory Commission (NRC). The OSMM is now the Department of Energy's Office of Safeguards and Security and still provides the lion's share of the $12 million Safeguards research and development program at Los Alamos.

Bob was named to head the new program, which began in a small laboratory at Pajarito Site replete with chipmunks in the offices and a rattlesnake on the doorstep. As the program grew, this space was augmented a year later by the addition of a second, larger laboratory at another site. With the encouragement and cooperation of Dick

In-line monitoring of uranium hexafluroide (UF_6) enrichment. This system, shown installed in 1975 at Goodyear's Atomic Gaseous Diffusion Plant in Piketon, Ohio, also uses two independent sensors developed at Los Alamos. The gamma enrichment meter measures the percentage of uranium-235; the neutron detector measures the percentage of uranium-234. This in-line instrument allows instantaneous isotopic analysis (to better than 0.5% accuracy), providing assurance of criticality safety during withdrawal into large cylinders as well as verification that the product selection meets the enrichment specifications. Because uranium-234 is also enriched in the diffusion process, its isotopic abundance in the product UF_6 provides useful diagnostic information for plant operation. The alpha-particle activity of uranium-234 is the principal source of neutrons emitted by enriched UF_6, and this neutron yield is an important signature for safeguards verification.

Baker, Chemistry-Materials Science Division Leader at the time, and the tolerance of Bill Maraman and his Plutonium Chemistry and Metallurgy Group, a special technical liaison committee was set up in 1967 to encourage cooperation among safeguards researchers and those staff whose group or division responsibilities were directly concerned with nuclear materials and equipment, This committee helped to identify needed, practical applications for testing and applying newly developed safeguards techniques to materials measurement, accountability, and safeguards problems. Such problems were not uncommon in the materials processing, fabrication, and recovery operations carried out routinely at the Laboratory's plutonium facility. The close liaison between safeguards researchers and the Laboratory's plutonium chemists and metallurgists significantly helped the Los Alamos Safeguards program get off to a head start in the safeguards field with a commanding lead that has been retained ever since.

The Agnew years saw the Los Alamos Safeguards research and development program grow by more than an order of magnitude. At the beginning of the '70s, most of the nuclear industry was unaware of the importance and economic impact the nondestructive assay techniques could have on their operations, so in the spring of 1970 Los Alamos fielded the Mobile Nondestructive Assay Laboratory (MONAL) to serve as a demonstration unit and assay laboratory and as a staging area for conducting in-plant assay using portable instrumentation. During the next few years, MONAL traveled to nuclear facilities nationwide, addressing special measurement problems. The first Los Alamos instrument installed in a nuclear facility for routine production use went to the General Electric fuel fabrication plant in Wilmington, North Carolina, in the spring of 1971 to assay reactor fuel rods. By the end of the decade, instruments and techniques developed by Los Alamos were in use throughout the world. In November 1973, the Safeguards staff conducted its first formal course in nondestructive assay techniques. By 1980 nearly seven hundred people had received training in safeguards techniques at Los Alamos, and currently about two hundred students participate each year in the eight to ten courses offered, including all new IAEA inspectors, who come to Los Alamos for their initial training.

Today, the Los Alamos Safeguards program is recognized worldwide as the fundamental source for state-of-the-art safeguards technology and has been designated as the DOE's lead laboratory in nuclear materials control and accountability research and development. It encompasses all aspects of the design, development, testing, and in-plant evaluation of new techniques, instruments, and integrated systems for safeguarding fissionable materials in all types of civilian and national defense nuclear facilities. ■

The Hot Dry Rock Program

by Morton C. Smith

It is not often possible to trace the ancestry and list the immediate family of a new idea, but in this regard—and some others—the Hot Dry Rock Geothermal Energy program is exceptional.

Since its establishment as Site Y of the Manhattan Project, the primary mission of Los Alamos National Laboratory has required information that could be acquired only from experiments done in nuclear reactors, and reactor expertise has always been one of its greatest strengths. It was therefore quite natural, when a national need appeared for higher-performance rocket-propulsion systems, that the Laboratory should propose the use of compact, gas-cooled nuclear reactors. The result was the Rover program.

One of the reactor concepts considered in the early days of the Rover program was Dumbo, a fast reactor with a refractory-metal-composite core built as a honeycomb structure. To demonstrate the heat-transfer characteristics of such a structure, a resistively heated laboratory-scale model of a core section was built and used to heat a hydrogen jet to above 3000 degrees Celsius. The demonstration was impressive, and when Dumbo was abandoned in favor of a graphite-core reactor, some of the Dumbo advocates felt that a gadget that good must have other uses. In particular, Robert M. Potter (now a Laboratory Fellow), after rereading the Edgar Rice Burroughs novel, *At the Earth's Core,* concluded that something like it could as well be pointed down as up and used to melt holes in rock more rapidly and efficiently than they could be produced by drilling or tunneling. The result, some years later, was the Subterrene program—development of a rock-melting earth penetrator.

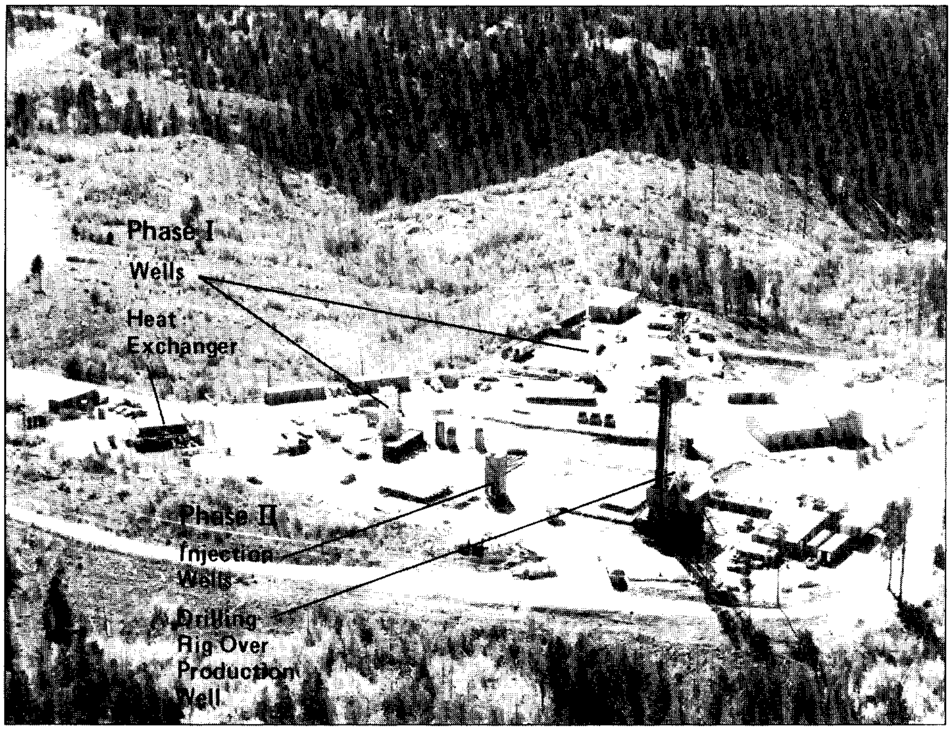
In 1970 the late Eugene S. Robinson assembled an *ad hoc* committee from several Laboratory divisions and disciplines to examine the possibilities and problems of the Subterrene. One of the obvious problems was disposal of the molten glass produced when a rock is melted. Again Potter had a suggestion. He had been reading about drilling in oil and gas fields and had learned about hydraulic fracturing—the use of fluid pressure to produce large cracks extending outward from the well to facilitate drainage of fluids into it, He proposed that sufficient pressure could be developed in the melt ahead of a penetrator to produce such cracks and force the glass into them, where it would freeze and remain. This idea was never actively pursued in the Subterrene program, but it appeared to the committee that hydraulic fracturing had many other possibilities. One of the most important of these, they concluded, was its use to create flow passages and heat-transfer surface in naturally heated crustal rock whose initial permeability was too low to be usefully productive of natural steam or hot water—"dry hot rock."

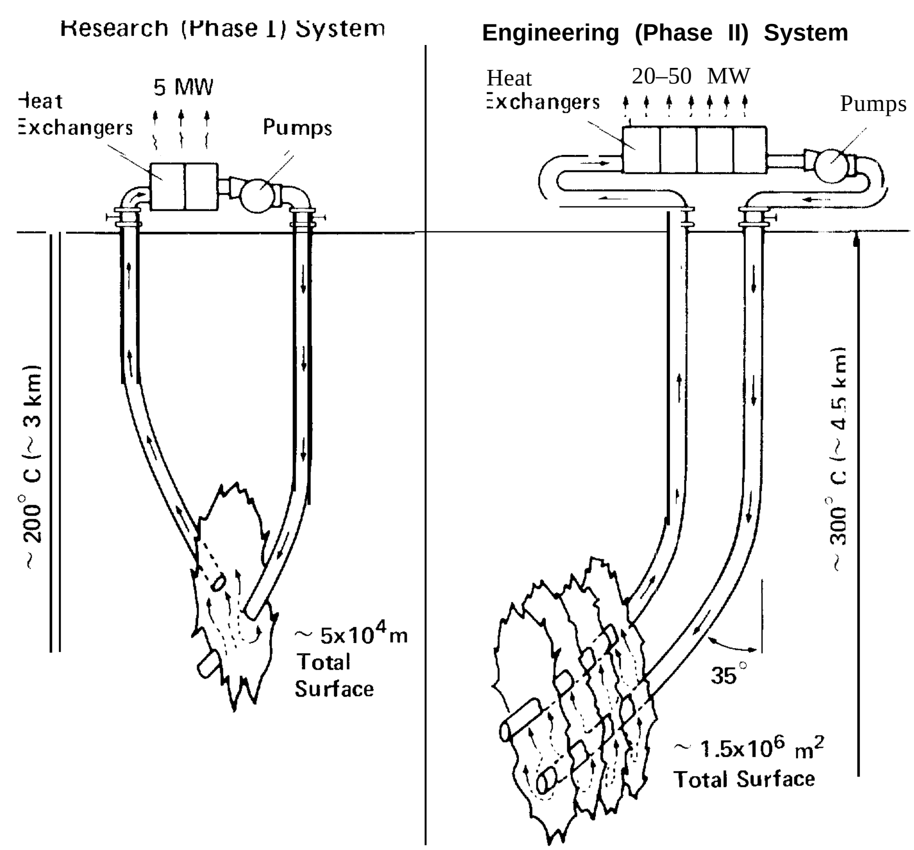
The method proposed by the committee was to drill a hole from the earth's surface to a sufficient depth to reach essentially impermeable rock at a usefully high temperature; to produce a large hydraulic fracture near the bottom of the hole; to drill a second hole from the surface to intersect that fracture; to pump water down the first hole to circulate through the fracture and extract heat from the rock around it; to recover the hot water through the second hole under sufficient pressure to prevent boiling; to extract its useful heat; to then return the water to the first hole to recirculate and extract more heat.

When the Subterrene program had been launched, Bob Potter and I assembled a group of volunteers and initiated a "Dry Hot Rock Geothermal Energy program" to investigate this concept. (The name was subsequently changed by someone in Washington who thought that "Hot Dry Rock" was more euphonious.) Initially the program was unofficial, unfunded, and supported largely by faith and the tolerance of Laboratory management. Most of the first year's work was done on weekends and holidays, and much of it in snow up to there. However. in 1971 the group managed to digest much of the existing information on geothermal areas and the equipment and techniques needed to create a dry hot rock energy system, and to begin a terrestrial heat flow study in the Jemez Mountains west of Los Alamos. In 1972 that study was concluded and, with discretionary research and development funds provided to the Laboratory by the Division of Military Application of the AEC, an exploratory hole was drilled in Barley Canyon—about 30 kilometers west of the Laboratory. The hole reached a final depth of 785 meters, penetrated about 143 meters of granitic basement rock, and had a bottomhole temperature of 100.4 degrees Celsius. With additional funding from the Division of Physical Research of the AEC. hydraulic-fracturing and pressurization tests were run in the lower part of the hole, and it was concluded that the basement rock was well suited to creation and containment of a pressurized-water heat-extraction loop.

With this encouragement and the prospect of substantial funding from the newly formed Division of Applied Technology of the AEC, an official 'Los Alamos Geothermal Energy Group was formed early in 1973, with myself as Group Leader. The anticipated funds materialized, and in 1974 a deeper exploratory hole was drilled at a more accessible and convenient location—on Fenton Hill, about 2.5 kilometers south of Barley Canyon. This hole reached a depth of 2930 meters and a rock temperature of 197 degrees Celsius. Experiments in it confirmed the observations previously made in Barley Canyon, but at greater depth and higher temperature.

In 1975 a second hole was drilled at Fenton Hill (photograph and figure) to a final depth of 3064 meters and a rock temperature of 205 degrees Celsius. A poor connection was made between hydraulic fractures produced from the two holes. After considerable experimentation and much development of new equipment and instruments, the connection was improved in 1977 by redrilling one of the holes, and in 1979 the





The photograph shows the hot dry rock geothermal site at Fenton Hill, looking southwest. Phase I of the project, shown schematically on the l@ side of the figure and with its two wells and heat exchangers labeled in the photograph, was completed in 1979 and has been producing heat at rates high enough that several hundred homes could be heated. It is hoped that when Phase II (labeled in photograph and right side of figure) is completed, heat production will be sufficient to demonstrate that a commercial electric power plant could be supported.

underground loop was enlarged by additional hydraulic fracturing (Phase I). With an air-cooled heat exchanger at the surface to dissipate the heat, this pioneering hot dry rock energy system has been operated intermittently since 1978 as a closed. recirculating pressurized-water loop. Heat has been produced at rates up to 5 megawatts (thermal), which would heat several hundred homes if there were that many nearby. The longest continuous run lasted nine months and had no detectable environmental effect. Some of the heat has been used to generate electricity in a 60-kilowatt binary cycle plant, but neither the temperature nor the rate of heat production was sufficient to support a commercial power plant. Therefore, a larger, deeper, hotter system (Phase 11) designed to demonstrate that capability is now being constructed at Fenton Hill.

While the objective of the Hot Dry Rock program has always been the very practical one of' making a vast, indigenous energy supply useful to man, the effort to do so has necessarily included a wide variety of supporting research and development activities—many of them done cooperatively with industrial organizations, university groups, and complementary programs at other laboratories and in other countries. To justify existence of the program, the very large resource base of thermal energy at accessible depths across the entire United States had to be evaluated. To implement the field program. it was necessary to develop drilling, well-completion, and hydraulic-fracturing equipment and techniques usable in very hot, inclined geothermal wells and also downhole instruments to log such wells and collect data in them. And to analyze and understand the information collected in the field has required both theoretical and laboratory studies of rock-water interactions, fluid and rock mechanics, heat transfer and transport, acoustic emissions, and other subjects. The program is broadly interdisciplinary and covers the entire spectrum from basic research to engineering application.

Since its inception, the Hot Dry Rock program has been supported primarily by the AEC and its successor agencies, ERDA and DOE, with supplementary support since 1980 by agencies of the governments of West Germany and Japan. However, the most important support has come from people like Harold Agnew, Director of the Laboratory during most of the history of the Hot Dry Rock program and always its most personable, articulate, and effective advocate. ■

Vintage Agnew

June 15, 1966

talk to Army group

. . . It does seem to me that with all the basic information we don't know, with all the problems in which we are involved, with all the deficiencies that exist in the world, that a scientist should in some degree. ., stick his head out of his office or his laboratory, whether he is a first year lab assistant or last year's Nobel laureate, and ask himself . . . Is the problem I'm working on one of those whose solution might directly help my colleagues or my fellow countrymen right now or in the future? If the scientist doesn't know, it is probably because in his narrow pursuit of his particular field he actually doesn't know what is going on around him. He may not have taken the time to even find out, or worse, he doesn't want to. This attitude worries me very much.

July 8, 1970

talk entitled Tactical Nuclear Operations

In the last 20 years we and other nations have been engaged in numerous arguments which resulted in physical combat. The political and military approach to these confrontations has been to rely on conventional weapons systems. Although we pretend to have a tactical nuclear capability, we have no doctrine for carrying out tactical nuclear warfare, nor do we seem interested in developing a tactical nuclear capability. Yet, if properly structured, it could conceivably deter these lesser wars—or at least make our forces more effective if they are challenged . . . .

Let me take as an example a particular military target in North Vietnam: the Thanh Hoa Bridge. This bridge is about 540 feet long. For military reasons we decided it had to be destroyed. . . .

We flew 657 strike sorties. In addition we employed approximately 300 supporting sorties. We dropped [2.5] million pounds of bombs, we lost 9 aircraft. In addition three optically guided Walleyes were launched at the bridge. Each of the Walleyes actually hit the bridge but the 750 pound warheads were insufficient to seriously damage it. We never were able to collapse a single span. Present rumors state that the bridge doesn't exist but is simply painted on the water. . . .

Had [the] Walleyes carried a [subkiloton] nuclear warhead. . . such as at long last is being provided in the Mk-72, the bridge would have been put out of action, Instead of expending 2.5×10^6 pounds of high explosive in about 700 sorties, the mission could have been accomplished with at most *two* strike sorties and a few cover aircraft . . . . The collateral damage from [such] a ground [nuclear] burst . . . would be. . . negligible compared to that actualIy imposed with conventional explosives as currently delivered with free fall bombs. . . . [Moreover] burial to optimum depth (which maximizes cratering effects and minimizes fallout) is feasible with devices now under development.

July 13, 1971

talk to the National Classification Management Society

Almost my whole professional career has been involved with technical work which has had a running battle with classification. To be very frank with you I've never won an argument with a classification officer and I've never understood why I've continued to lose. . . .

In spite of our country's background in freedom. . . we all know there is a tremendous amount of secrecy and classification involved in government and private industry. Some of it is certainly warranted and will always be required if we are to have a competitive capitalistic industry. But there comes a time when secrets are no longer secrets and impedances imposed by secrecy or classification are no longer warranted. . . .

[For example] I believe that the philosophy or concept of embargoes on materials, products, and technology in today's world is archaic. . . . In fact. . . if the intent of the embargo concept [as embodied in the Battle Act of 195 1] was to guarantee U.S. conventional military superiority it has failed. . . .

Not so long ago the President announced that he was going to attempt to open trade with China. I don't believe there is a person here who doesn't believe that is a splendid idea. But, . . to pacify our basic fears, which I believe are no longer warranted, the White House quickly stated that of course we wouldn't allow the export of commerical jet aircraft or diesel locomotives. . . which the White House then stated that China very much wanted. . . . Do we really believe that in 1971 a nation of 750 million people shouldn't have commerical jet aircraft? . . . Do we believe that if they don't purchase them from us they won't be able to buy them from France or even Russia? Do we really believe that having jet commercial aircraft will jeopardize the security of the U.S.?. . .

Providing China with a modern airline with aircraft, ground equipment, airfield and navigational aids would be a real shot in the arm for our economy. We ought to sell what we can. . . . Why should ping pong players have to ride in DC-3's or coaI burning locomotives?

February 4, 1976

paper presented at the Annual Joint Meeting of the American Physical Society and the American Association of Physics Teachers

Chemical reactions give a few electron volts per interacting atom. Fission gives two hundred million electron volts per reacting nucleus. This factor of a hundred million has a favorable impact not only on the energy produced but also on the environment with regard to the amount of raw materials required and the wastes produced. A thousand megawatt coal plant produces six million cubic feet of ash per year, a fission plant less than a cubic yard.

Sooner or later the whole world will realize that they cannot turn their backs on the benefits of the nucleus. Today fission, hopefully in the next century fusion.

April 14, 1977

talk at Belgium American Chamber of Commerce Luncheon in honor of Dr. Agnew

... [Most of] the world's population. . . [has] great expectations. Fart of their expectations are due to the sort of instant discontent that we through the media have been beaming for many, many years. They expect in a very short time to achieve a standard of living that's commensurate with ours, and I would submit that we're not going to achieve this standard of living unless they have plentiful relatively inexpensive energy. This can be provided, but. . . only. . . through what I'll call technology. It's not going to be achieved through wishful thinking or abstinence in certain technologies.

April 19, 1977

letter to Congressman Jack F. Kemp

. . . I do not believe we can maintain a technology base or the necessary cadre of first-class scientists and engineers to enable the USA to have a nuclear weapons design capability for more than a few years if testing ceases.

September 8, 1977

testimony before Senate Foreign Relations Committee

. . . If it is the considered opinion of the Senate that the United States has no further needs now or in the future for new untested types of warheads having yields substantially greater than the 150 kilotons limit of this agreement, then the [threshold test ban] treaty [under consideration] will have no appreciable impact on our defense posture in the immediate future. However, if you believe that there will be requirements far new untested designs of yields considerably larger than 150 kilotons, then if this treaty is ratified our defense systems will eventually have to bear a penalty in payload weight, physical size, and perhaps even in the additional use of fissile materials. . . . It simply will not be prudent to put into the stockpile designs which represent a large extrapolation from tested designs...

I personally would not support any treaty further limiting nuclear testing until meaningful agreements on SALT and Mutual Balanced Reduction of Forces have been ratified. . . . I stress this relation to other arms control progress because we need some clear sign of Soviet restraint in their weapons build-ups and because our own nuclear posture must be appraised as a consistent whole. . . .

For those of you who may wish to remind me of the destruction caused by a nominal 15 kiloton bomb, may I remind you that I flew on the Hiroshima mission and have participated in the major thermonuclear tests which this country has conducted. As an aside, I firmly believe that if every five years the world's major political leaders were required to witness the in-air detonation of a multimegaton warhead, progress on meaningful arms control measures would be speeded up appreciably, October 2, 1977

talk at 1977 National Conference for Advancement of Research

I still remember when Seamans took over the AEC, he said, "ERDA will not be a warmed over AEC." He was right; except for the weapons program and a few other areas, it became a half-baked NASA . . . I believe the dismal track record of ERDA was due to the lack of appreciation of how fundamental [our] basic but relevant research is to the successful implementation of any development or engineering project. . . .

Hopefully, this attitude will not prevail [in] the DOE [under Schlesinger] . . . because of [his] past attitude when he was with the AEC. For tens of years under the most absurd secrecy . . . the AEC had been conducting research on centrifuges. Their engineering was superb, but their basic understanding . . . of how centrifuges really work, which involves complicated fluid dynamics, was lacking. After Schlesinger came on board. . . he simply directed that the weapons people, with their advanced, basic science capabilities in. . . fluid dynamics, be brought into the program. In a few months . . . the weapon design theorists attacked the problem, developed codes to analyze the action of the gas inside the centrifuge, and allowed the centrifuge to become a viable option. . . for uranium enrichment. Had Schlesinger not broken down the compartmentalization. . . the centrifuge developers would still be using an Edisonian, build-and-try technique with a six months turnaround time. . . .

Many people don't realize the. . . stimulus given to major scientific programs in the U.S. today, which started from work initiated through the weapon's supporting research program of the AEC . . . . Some originating at Los Alamos are:

1. SHERWOOD - controlled thermonuclear fusion
2. LAMPF - medium energy physics facility
3. ROVER - nuclear rocket research
4. LASER FUSION
5. JUMPER - laser isotope separation
6. VELA - nuclear test detection
7. SMES/SPTL - cryo-engineering
8. NUCLEAR SAFEGUARDS
9. GEOTHERMAL ENERGY

. . . the support of basic science is vital to any development work; it can't be programmed and micromanaged. It must be supported as if it were one of the art forms, which it really is.

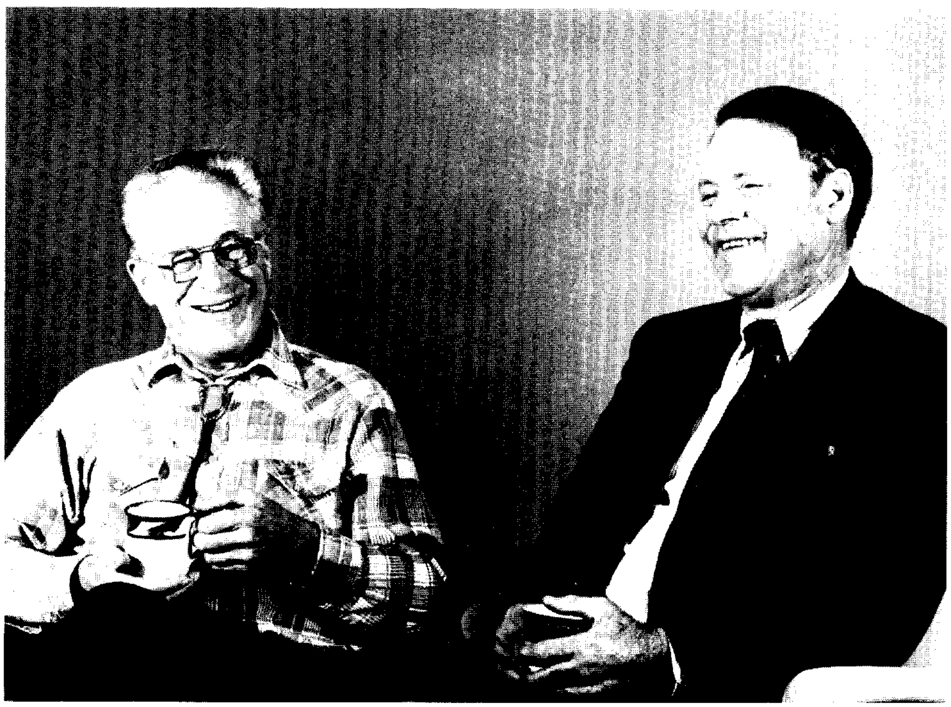
However, one can insist in these trying times, where we are confronted with specific problems, that for the most part research be conducted in relevant fields, but not that it be necessarily relevant today . . . . If one does not provide this freedom and enlightened management, then the country will end up with the run-of-the-mill, average, plodding, pseudo-research institutions, which will be busy supplying the last digit after the decimal point that is so dear to the handbook publishers. The innovative wild men and women who are always on the leading edge of science and technology will not be part of the team, And we need them.

1954

State Senate campaign slogan

"A person of integrity stays bought!"

The Times They Were a Changin'



Raemer Schreiber (left) joined the Laboratory in 1943. In the '50s he was the Leader of the Weapons and the Nuclear Propulsion divisions and then, in 1961, was appointed Technical Associate Director. He remained in that position after Agnew became Director until "Harold, in 1972, decided I was really Deputy Director, so he changed my title. " Robert Thorn (right), currently the Deputy Director, first joined the Laboratory's Theoretical Division in 1953. His numerous administrative positions included Theoretical Design Division Leader, Associate Director for Weapons, and, from March to July, 1979, Acting Director of the Laboratory.

SCIENCE: ***Schreib, you were Technical As-*** *sociate Director from 1962 to 1972 and as such were part of the transition between the Bradbury and Agnew eras. What do you feel was Agnew's vision of the Laboratory when he became Director?*

SCHREIBER: Only Harold can answer that question definitively. I do know he was always intensely proud of the capabilities of the Laboratory and did not feel that its expertise needed to be confined to nuclear physics, He was willing to tackle any scientific or technological problem worth solving. Generally he took the attitude, "If we don't have the experts, we can get them." You should remember that at this time reactor work was shifting over to commercial utilities, and the AEC was clamping down on new reactor concepts. Harold saw that the future of the Laboratory might well be in other directions than just pure nuclear physics.

SCIENCE: *Bob, you were the Theoretical Design Division Leader and then later Agnew's Associate Director for Weapons during the '70s. What do you feel he hoped to accomplish when he became Director?*

THORN: I think Harold felt we needed to regain the initiative in weapons development that we'd lost to Livermore. In 1970 this Laboratory was still largely a weapons lab, but Livermore was doing a better. more aggressive selling job and was pushing for the enhanced radiation weapons and all the strategic weapons—the nuclear warheads for Minuteman and Polaris. Their reputation was better than ours, or at least perceived to be so by some people. Harold's vision was to restore the luster that Los Alamos had lost. It's true that he thought the Laboratory was premier in all fields and he would undertake anything, but above all he wanted to be first in our principal mission of weapons development,

SCHREIBER: There's another aspect to the Bradbury-Agnew transition that I feel is also important to recognize, At the end of World War 11, when Norris became Director, a lot of people who had served during the war years on Laboratory advisory boards simply disappeared. Norris really didn't have an existing management structure to work with, so he was able to start with a clean slate. Twenty-five years later the Laboratory was firmly established, and Norris was working with a senior staff of people he'd worked with for years. He knew what they could do and what they were interested in doing, so he was able to take a low profile and run a fairly relaxed ship. But many of these people were also approaching retirement. Norris knew and they knew that major changes would have to be made in a few years. However, Norris did not want to make changes that would obligate the incoming director. When Harold took over he had the chance to assert his leadership at once. It was an appropriate time to reshuffle personnel and his reorganization took place over the first couple of years.

THORN: I agree. Both Oppenheimer and Bradbury operated with small staffs and were able to stay close to all aspects of the effort because there were only a very few major programs. For example, I think when

Harold took over there was the Weapons program. the Space Nuclear Reactor program, and the Fusion program. By the end of Agnew's directorate there were 600 programs! Harold realized that things were getting more complicated and set up two associate directors, one for weapons and one for research, to handle the technical programs. He inherited a Technical Board from Norris made up of the director's immediate staff, division leaders, and department heads, but as time went on this function was largely replaced by the associate directors working with their divisions,

SCHREIBER: In fact, Norris and Harold had different personalities, different approaches to management, and the Tech Board meetings show some of these differences. All major policy decisions under both directors were discussed or announced at these meetings. Norris' favorite technique was to state the question, perhaps offer some possible answers, and then sit back with his feet on the table and let people talk. He might pose some questions from time to time, but generally he let everyone have his say. Quite often a consensus would be reached, in which case he'd simply say, "OK, let's do it that way," Or there might be times when violent differences of opinion would emerge, Then he'd either rule one way or another or suggest that we adjourn and think it over some more. Harold preferred to research the subject first, make up his mind in advance, then announce his decision at a Tech Board meeting. He would listen to contrary arguments to see if anyone really couldn't live with the decision. As a result, he might modify his stand, but he did not encourage prolonged debate.

Harold could be fairly hard-nosed when it came to the shuffling of senior personnel. Perhaps he had to be since he was dealing with entrenched incumbents, but he also believed that the future of the Laboratory depended on bringing in fresh people with new ideas and on rotating responsibilities to provide management training. This was a deliberate stirring of the Laboratory by Harold. and he put his priorities for the Laboratory above the feelings of those displaced. On the other hand, he was quite compassionate in dealing with hardship cases anywhere in the Laboratory.

Agnew at Tinian in 1945.

One thing was the same under both directors: it was implicit that management get their jobs done without formal directives or instructions. The general attitude was, "If I have to tell you how to do it, you shouldn't be holding down that office."

SCIENCE: *How did management change from the beginning to the end of the Agnew era ?*

SCHREIBER: It got more complex. Because of the small number of major programs, interdivisional coordination under Bradbury was handled by steering committees or working groups usually chaired by one of the division leaders. As a result, program direction was quite decentralized and the Director's staff was small. But then the AEC discovered "program direction," which is a polite way of saying that it was building its staff to participate more directly in calling the shots out at its laboratories. Moreover, it was subdividing its budget and personnel to enforce compliance with its directives. This process has continued through the ERDA and DOE regimes and is largely responsible for the large growth in administrative positions in the laboratories themselves.

For example, the Budget Office under Bradbury had two men and a secretary. Harold had to set up the Financial Management Office which grew to about fifteen to eighteen people. Periodic reports and what were called Form 189's were required for every project. This resulted in an enormous amount of bookkeeping, so the accounting office had to grow. There were a number of requirements from Washington that Harold at first just flatly refused to comply with. He won some of these, but lost others.

THORN: In fact, by the end of Harold's tenure it was obvious to many. including

Harold and Norris about the time of the transition between the two directors in 1970.

Harold, that substantial management changes had to be made. The changes were largely necessary because of the increase in programs, program direction from Washington, and accountability. As a manager, you had to control and review the yearly proposals to make sure that they went to Washington in the proper form and that they were the kind of thing the Laboratory wanted to do. In addition you had divisions over which you had to exercise line management. So you were both program manager and line manager. And then you presumably were supposed to remain technically competent. It was just too much to do—too much for a director and two technical associate directors to do, Harold wisely held reorganization in abeyance and allowed his successor. Don Kerr. to implement his own management system.

SCIENCE: *Bob, getting back to Agnew's desire to regain the initiative in weapons development, what were the major accomplishments in the Weapons program in the '70s ?*

THORN: When Harold took over, Livermore was responsible for the development of all the strategic missile warheads, which were the big prestige items in the eyes of the public and the Defense Department. But Harold fought vigorously to acquire new warhead responsibilities.

SCHREIBER: Harold was a very aggressive salesman.

Thorn: Yes. He started the Weapons Program Office and the Weapons Planning Office. These were supposed to be part of what you might say was our marketing group. By backing up this group with the technical people in the design and engineering divisions, we could be more aggressive about going out and getting these weapons systems. He also tried to reinvigorate the Weapons program here by splitting the old Theoretical Division—the design part away from the theoretical physics part—so as to provide more emphasis to weapons design. As a result of these efforts, we were awarded responsibility during his tenure for the W76 used in the Trident warhead, the W78/Mark 12A used in the Minuteman III warhead, and the W80 used in the air-launched cruise missile warhead. Also, the Laboratory introduced the first enhanced radiation bomb into the stockpile and developed new versions of the air-carried B61, a general purpose bomb and warhead for short-range attack missiles. One of the weapons developments that Harold felt most proud about was the introduction of insensitive high explosive that makes the stockpiled weapons containing it much safer to handle. An accidental detonation that scatters radioactive plutonium becomes highly unlikely.

SCHREIBER: Another point is that Harold took over at the time when the national emphasis was shifting from aircraft to ballistic missiles, so the major weapon developments were aimed at matching the bomb to these new carriers. Microelectronics and the ability to communicate or to install elaborate instructions in missiles opened a new era in the mating of warhead to delivery system. Ideas such as smart missiles that could track a target or the concept of multiple independent re-entry vehicles (MIRVs) were growing. These ideas required new weapons, but not in the sense of changing the basic physics of the innards of the device. Rather they were new weapons in the sense of changing the configuration to match size, weight, and shape requirements of the missile warhead or in changing how the weapon was told to behave to match the safing, arming, and fuzing requirements of the delivery systems. These requirements led to significant and detailed changes involving highly intricate engineering of the warheads. Also changes were made to improve yield-to-weight ratios and to extend the useful stockpile lifetimes of the warheads. Because of the necessarily close relationship between warhead and delivery system, this period was one of very intensive collaboration with the

Defense Department.

THORN: The collaboration was revitalizing. Originally I think Los Alamos slipped because many of the people here had been in the business since the beginning—twenty-five years—and some of them had grown tired of the arms race. Their attention shifted to diversifying into other fields. As a result, the Laboratory was not putting the kind of attention into weapons development that a weapons lab should be putting into it. After all, we're not here to argue for arms control, we're here to design weapons. But in this period we started to participate more actively with the Defense Department, both by designing to meet their stated weapons needs and by developing our own ideas and trying to sell them.

SCIENCE: *The diversification into nonweapons programs, then, did not start with Agnew?*

SCHREIBER: In one sense, yes. There was a strong effort under Bradbury to diversify into nonweapons applications of nuclear energy, but this was generally limited to nuclear reactors and nuclear fusion. In the '60s there was considerable encouragement by the AEC to try out all sorts of ideas for building reactors, and Los Alamos had projects in nuclear rocket propulsion, the thermionic reactor for generating electricity directly. the graphite-based, ultra-high-temperature reactor, reactors in which the fuel was molten at operating temperatures, and so forth. It was a time when anybody who had an idea that would stand up under peer scrutiny could try it out. But, as I said earlier, about the time of the Bradbury-Agnew transition there was a budget squeeze, and the AEC curtailed support of new reactor work to concentrate on the commercial development of the light-water reactor and on research and development of the liquid-sodium-cooled breeder reactor. This created an immediate need at Los Alamos to find other activities for many of the people who had been in the field of reactor development.

Harold with Edward Teller in 1973.

Part of the need was satisfied by a push into energy programs. For example, the potential of lasers to do isotope separation and to initiate fusion reactions was brought to Harold's attention, and he authorized an immediate expansion of this work. A bit later the oil crisis of '73 and '74 stimulated interest in alternative energy sources, and that led to substantial programs in solar energy, hydrogen as a fuel, and hot dry rock geothermal systems. Other energy programs included synthetic fuels, fuel cells, and superconducting transmission lines. Our large computer facility made possible demographic and socio-economic studies of energy resources and energy distribution.

THORN: In fact, the push into the energy programs during the '70s was so vigorous that the Laboratory, rather than shrinking, almost doubled in size. Harold had correctly recognized that times were changing. He responded by infusing the Laboratory with a spirit of experimentation based on the expertise we'd acquired over the years dealing with multidisciplinary problems in weapons research. It was a period of excitement and challenge.

It was also true that many of the programs were unrelated to our principal mission, and the Laboratory lost a great deal of the cohesive spirit that bound it in its first twenty-five years. What happened was that in response to the energy crisis the AEC had its charter broadened: it could look into other energy programs besides nuclear. The government thought the way to solve the energy problem was with an influx of money, and the fastest way to get started was at the level of the national laboratory. Of course, they found some eager people here quite willing to work on these problems. But as far as having any overall coherent plan—that was missing! The result at the Laboratory was a multitude of programs. When everyone had been paid from the same source—the weapons program—you could

Harold spearheaded the drive for the Laboratory's National Security and Resources Study Center, shown here under construction in 1976.

walk up to somebody, ask him to do something, and he'd get it done. Today you ask, and he'll say, "I can't do that. I'm working on another program, and my sponsor won't allow me to work on yours unless you give me some money." That's an example of what I mean by a loss in the spirit of cohesiveness.

SCIENCE: *What were some of the outstanding nonweapons programs under Agnew?*

SCHREIBER: Well, as I mentioned before, laser fusion and laser isotope separation were initiated by Agnew. A great deal of excellent research has come out of those programs. There's LAMPF—the Los Alamos Meson Physics Facility—which was conceived in the Bradbury years, then realized in the Agnew years. LAMPF, of course, is a story in itself.

We have the new plutonium facility, which is the finest plutonium research and development facility in the country, perhaps in the world. That such a facility was necessary had been recognized at Los Alamos for years, but Harold was the one who convinced the AEC. The old DP site had been built in a hurry as a temporary facility and was being kept in a safe operable condition at considerable maintenance cost. So first the AEC had to be made aware that something should be done. If they were just going to shut the old site down, what then? There were two other reasons the decision was held up: environmental requirements had been changing so that it was hard to pin things down, and it was going to be a very expensive bit of construction because of the need for safeguards and protection against everything from a laboratory fire to an airplane crashing into the building. In essence the AEC was committing itself to having all plutonium research done at the new facility wherever it was built. Much of the selling was to point out the expertise in plutonium research that already existed here at Los Alamos. Construction of the new facility finally started in 1974.

The hot dry rock geothermal concept was an outstanding program under Agnew. Morton Smith should be given credit for initiating and selling this one—he probably made two thousand speeches on the subject. As I recall, preliminary exploratory work had been authorized by Bradbury, but a full-scale effort was not mounted until later when manpower, including chemists and materials fabrication people, became available when the Rover (space nuclear reactor) and UHTREX (ultra-high-temperature reactor) programs were halted.

In a similar vein, work on reactor safety analysis was a natural spin-off from the various experimental reactors that had been designed and built here. People who had been in the UHTREX and LAMPRE (molten plutonium reactor) programs and who were familiar with the safety requirements of reactors moved into that field.

THORN: I agree, Schreib, except I would attribute the reactor safety program more to Kaye Lathrop and other theoreticians who were using large computer codes for weapons simulation and started developing similar codes for reactor safety analysis. They expanded weapons transport codes by adding the appropriate equations of state, accounting for two-phase flow of water and steam, and so forth. But more important, they brought with them the experience of using large codes to model complex problems.

In contrast to many of the other nonweapons programs. the nuclear energy programs at Los Alamos have always complemented the weapons effort. Much of the work involves transport codes used in weapons calculations or involves the plutonium facility or provides useful neutronics data. In that sense, these programs have been cohesive, not divisive.

SCHREIBER: Nuclear Material Safeguards was another outstanding program: it was well under way toward the end of Bradbury's stewardship, then was expanded under Agnew. I was directly involved in its development but can take little credit since Bob Keepin was the founder and chief salesman. He badgered me into authorizing a small initial program, then parlayed that into a major effort by selling it to key officials in the AEC. He acquired equipment and laboratory area from defunct reactor programs using the "camel in the tent" approach. This approach comes from the old Arab story in which the camel outside the tent says his nose is freezing, so the owner tells him he can stick his nose in, then the camel says his ears are freezing, and so on. Bob used a lot of the equipment from the defunct UHTREX, including a building adjacent to it that had been built for reactor experiments. But the real success was the fact that he recognized a very real need—accountability and safeguards for fissionable materials—and then did something about it.

SCIENCE: *What about the theoretical effort?*

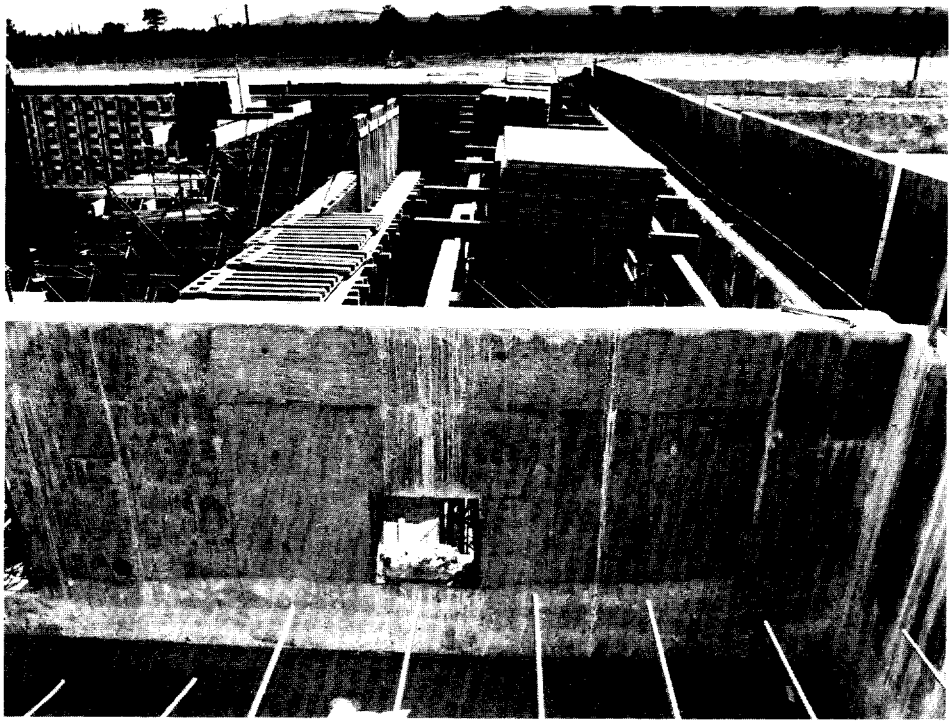
THORN: Well, Harold, although he was an experimentalist, respected theoretical physics, and he wanted a first-class theoretical research effort in the Laboratory. Peter Carruthers was hired by Harold and given that charter, which Pete was largely able to fulfill. Also, Harold started the Laboratory Fellows program to help bring eminent external scientists to the Laboratory. Early Fellows were Herbert Anderson, Richard Garwin, Gian-Carlo Rota, Bernd Matthias, and Anthony Turkevich. This program has been continued and expanded under Kerr, who has also instituted a Fellows program composed of outstanding scientists within the Laboratory. And there was a major expansion in computing under Harold, including purchase of the first Cray computers.

SCHREIBER: One of Harold's objectives was to find ways to finance the growth of basic research, including the theoretical efforts, up to a level of perhaps ten percent of the total Laboratory effort.

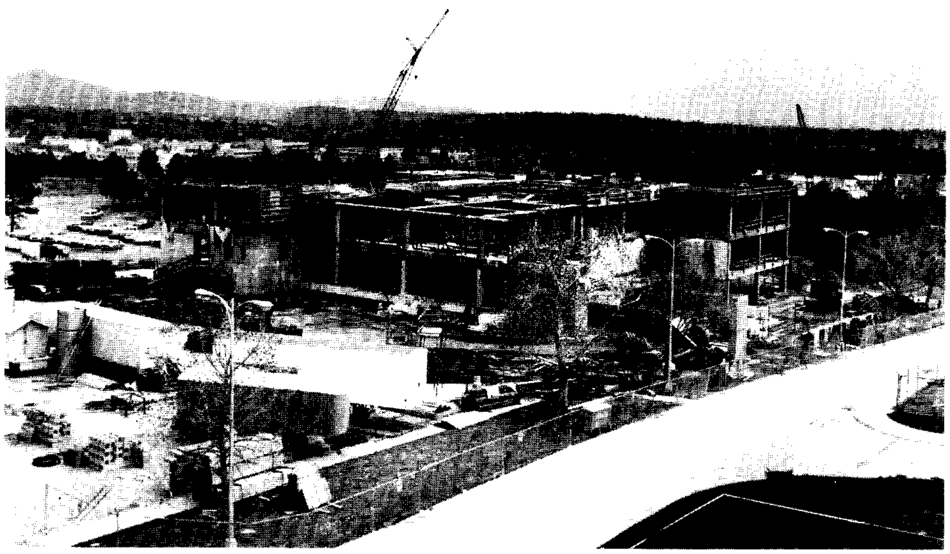
SCIENCE: *How did the funding sources and amounts change during this period?*

SCHREIBER: As we've already indicated, budgeting was not a major problem for most of Bradbury's tenure because the money came in a few large chunks accompanied only by general directives. However, the AEC eventually began to exert its muscle in program direction, and then the Laboratory had its first budget crisis in the early '70s with the cancellation of the UHTREX, LAMPRE, and Rover programs.

THORN: Essentially the entire experimental reactor program was wiped out, then Rover. plus there were cuts in the weapons program. The first thing that Harold did was to say, "Let's do reimbursables. Besides the AEC we'll work for the Defense Department, we'll work for any other federal agency." Harold was never just negative about a situation; he always had a solution or two. The idea of reimbursables was an important solution that not only helped the Laboratory survive a crisis, but opened new doors such as



Harold helped convince the AEC of the absolute necessity for a new plutonium research and development facility. Construction started in 1974.



The Helios facility was constructed during the mid '70s to further explore the use of the CO_2 laser as a driver for inertial confinement fusion. Helios is an eight-beam system with an output of 10 kilojoules in 1 nanosecond.

developing productive ties with industry.

SCHREIBER: The Laboratory had already done a limited amount of reimbursable work, but mostly at the initiative of the sponsor of the work. With the AEC cutbacks, active solicitation of reimbursable work was started and a full-time employee was assigned to sell the ideas. In the early period, this was encouraged by the AEC. However, when reimbursable work grew above ten percent of the AEC budget to the Laboratory, worries were expressed about possible wholesale layoffs if, for any reason, reimbursable work stopped. Most of the contracts were for a period of one or two years, so the worry was real, both to the AEC and to Laboratory management. An informal compromise was reached with the agreement that reimbursables would be held approximately to the ten-percent level.

As matters turned out later in the '70s, the AEC budgets grew and the Laboratory continued to expand. However, it 'was not all that easy. Each year's budget was a cliff-hanger, but Harold was an excellent salesman and knew how to bargain successfully.

THORN: He was indefatigable. He understood that good public relations were becoming necessary. He was good at it, but he needed to be. He traveled extensively, addressed groups, served on committees, and maintained contacts with Congressional delegations.

SCHREIBER: Considering the wholesale cuts at the beginning of the '70s, the Laboratory definitely needed that kind of effort.

THORN: Harold never stopped believing in or selling the expertise and the potential that exists in this Laboratory and its people. ■

The Laser Programs

by Keith Boyer

The laser programs of Los Alamos had their inception in 1968 when I was directing the test activities of the Nuclear Rocket Propulsion program (Project Rover) in Nevada. At that point decisions were being made that would shift much of the program's test activities over to Aerojet and Westinghouse, and it was an appropriate time to explore new activities.

The main concept of the Rover nuclear rocket was to generate a high-temperature exhaust stream for propulsion by passing a gas, such as hydrogen, through the hot core of a nuclear reactor. However, I thought that a system based on fusion rather than fission might provide an extremely high-temperature exhaust stream for efficient propulsion. One possibility was the "Orion" concept in which a series of thermonuclear explosions "pushes" the spacecraft by ablating a replaceable layer of material, such as water, off a pusher plate. This process could produce high thrust and a very high efficiency system.

But what would ignite the thermonuclear explosions? Because of my interest in lasers, I was aware of the development of a high-energy carbon dioxide (CO_2) gas-dynamic laser system by the Air Force Weapons Laboratory. Our calculations indicated that if the energy then predicted for this laser could be released in a short enough time (about a nanosecond) and focused uniformly onto a small pellet of thermonuclear fuel, an efficient fusion process might be achieved.

Another feature that made the gas-dynamic laser attractive to our program was the manner in which the laser's population inversion was generated. The CO_2 gas was pumped to higher energy states by heating the gas, then the inversion was formed with rapid cooling through an expansion nozzle. Our early systems could use the Rover reactor as the heat source for driving this laser at high energies. Thus, the investigation of laser fusion seemed appropriate. The Space Nuclear Propulsion Office in Washington agreed, and a modest effort was started that year at the Nevada Rover test site. Of course we recognized that fusion as a commercial energy source was the most important application and one that would surely precede any propulsion application, but we had found our first sponsor.

Design studies soon revealed difficulties in achieving the desired short, high-energy pulses at the low CO_2 pressures necessary in the gas-dynamic laser system, so other pumping mechanisms for the laser were considered, including optical, electrical, and chemical energy sources. Also, more information was needed about the effective absorption efficiency of the laser energy by appropriate targets, about the physics of the interaction process, and about energy transport and utilization in initiating fusion.

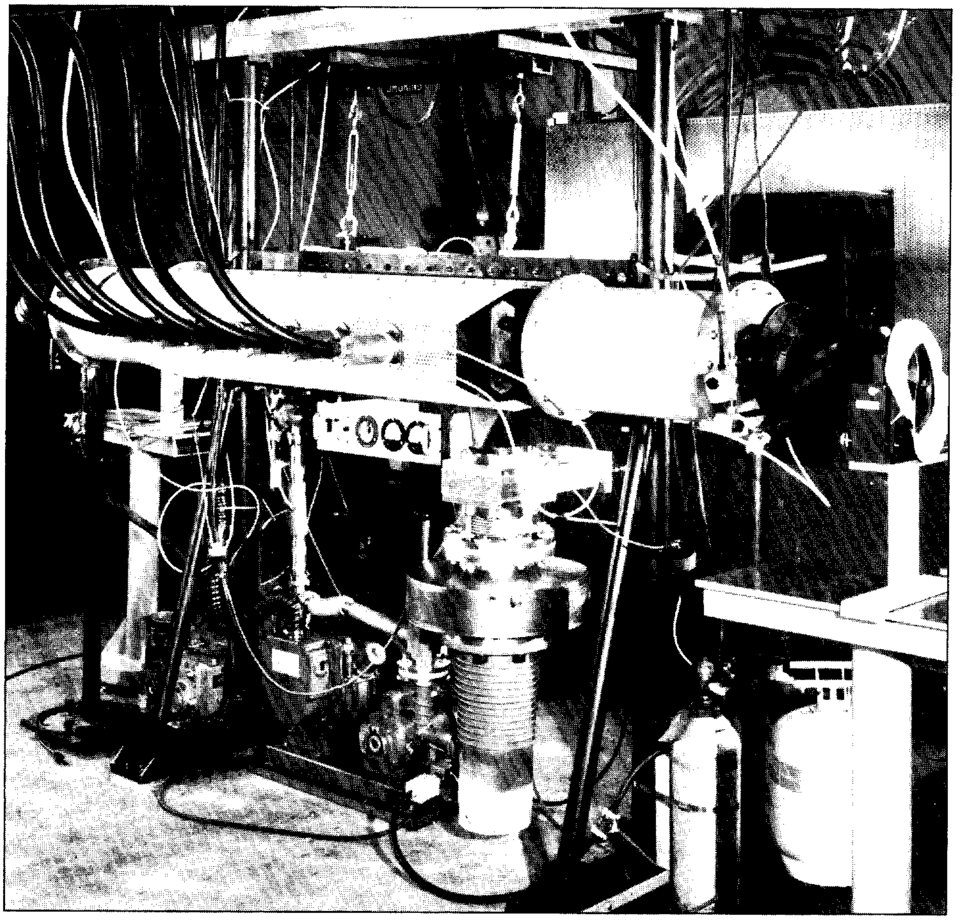
Raymond Pollock, a weapon designer, agreed to collaborate on this study and was able to derive the scaling laws and calculate the requirements to achieve thermonuclear burning of small pellets of fuel by assuming ideal interaction physics of the laser light with the target.

In early 1969 Bill Ogle, then the Weapons Testing Division Leader, agreed to authorize a small experimental exploratory effort. This activity included about ten staff members and initiated a three-pronged experimental effort: development of a one-joule, picosecond glass laser for the light-target interaction studies, investigation of electrical-discharge-pumped CO_2 lasers that could be scaled to high energy, and development of chemical lasers. Although chemical lasers would serve as backup for the undeveloped CO_2 laser, we intended to pursue both laser development and laser applications, and we recognized the potential of chemical lasers for studying photochemistry. For the CO_2 laser one of the early innovations, in which Charles Fenstermacher played a key role, was an electron-beam-controlled discharge capable of pumping large volumes of high-pressure CO_2 gas.

A year later we had established estimates of key parameters for laser fusion, such as laser energy, pulse width, and preliminary pellet design. We were able to outline a program designed to determine the feasibility of laser fusion, including several different laser options. About this time we became aware of other programs in various parts of the world, including those at Livermore, Sandia, the University of Rochester, the Lebedev Institute in the Soviet Union, and the Osaka University in Japan, but all of these were based on glass lasers. Moreover, apparently only the Los Alamos and Livermore programs initially considered a target design that used laser energy to compress the fuel strongly as well as to heat it, a technique that reduced the laser energy required by many orders of magnitude. This situation changed soon as the various programs, including a new one at KMS Fusion (a commercial venture), discovered the necessity of compression.

Harold Agnew, recognizing the importance of developing new and promising activities at Los Alamos, asked me in January 1971 to set up an expanded laser program. This program was run out of the Director's Office in order to enlist Laboratory-wide support. Our effort soon had a wide base of activities, including a theoretical group organized by Richard Morse in the Theoretical Divison; an interaction physics and target group under Gene McCall, who played a key role in the Laser Fusion program; a CO_2 laser development group under Fenstermacher; a glass laser group under Dennis Gill; and a chemical laser group directed by Reed Jensen. A series of seminars was established to review the existing state of laser technology and interaction physics and to explore new applications such as laser photochemistry.

By early 1972 the program had achieved sufficient size and complexity so that a new Laser Division was established. Two new groups were added, one on laser applications and one on target fabrication. At this time the first large CO_2 laser chain was being built and plans were in progress for a series of CO_2



One of the amplifiers used in the early '70s in a CO_2 laser chain that generated a l-nanosecond, 0.5-kilojoule pulse.

laser systems of increasing size, including a two-beam, 2-kilojoule laser later called Gemini; a six-beam, 10-kilojoule laser now operating under the name of Helios; and a 100-kilojoule system whose configuration was being debated and which evolved into the present Antares system.

The early interaction data was obtained using a 50-joule, picosecond glass laser, Meanwhile, work proceeded on development of a larger 500-joule, glass laser system. Frequency-conversion crystals were also planned to be used with this laser to give green light and ultraviolet light, although at lower energies. These latter frequencies were needed to explore fully the question of the most efficient wavelength for the laser fusion process, a question that has not yet been resolved. The chemical laser work proceeded with the development of hydrogen fluoride lasers, which promised to provide the highest energy output of any laser system.

A coordinating committee was established in Washington to provide guidance for the laser fusion programs in the United States with representation from the Division of Military Applications of the AEC, the Magnetic Fusion program, and the heads of the various AEC Laboratory laser programs. The Los Alamos budget approximately doubled each year through the early '70s,

Our plan to pursue a broadbased laser technology program included a small project in the Chemical Laser Group to investigate the use of laser energy to separate uranium isotopes. This particular activity captured the interest of Paul Robinson, who had transferred from the Rover Reactor Division, together with a number of other staff, as the Rover program decreased in size. Paul had earlier been active in the gas-dynamic laser effort in Nevada and now, together with Reed Jensen, played a major role in the isotope separation project. The separation was based on the photolytic dissociation of uranium hexafluoride vapor cooled by a supersonic expansion to permit isotopic selectivity using a combination of infrared and ultraviolet laser photons, This activity continued to grow until it was split off from the Laser Division as the Applied Photochemistry Division with Paul as Division Leader. This division also became involved in both high-repetition-rate, high-power laser development and in broad aspects of laser photochemistry. Projects included high-resolution laser spectroscopy, photochemical processing, laser sound generators for potential military uses, and chemical and biological warfare agent detectors. Although a recent Washington decision terminated the Los Alamos molecular uranium isotope separation process in favor of the Livermore atomic vapor process, the molecular process was close to engineering demonstration and was judged by many of us to be the superior process. In spite of the uranium decision a growing Los Alamos program on the separation of plutonium isotopes is doing well.

The laser fusion programs are still vigorous, but many problems have developed, and the final utility of laser fusion for energy production remains uncertain. Inflation and budget stretchouts reduced the design energy of the Antares CO_2 laser, which has just begun its checkout phase, from 100 to 40 kilojoules, The estimates of laser energy needed for a useful thermonuclear yield have risen from a few hundred kilojoules to a few megajoules. The longer wavelengths of both CO_2 and glass lasers produced undesirably large hot-electron components in the absorption process. The resulting self-generated magnetic fields are believed to reduce the lateral heat conduction that was originally counted on to symmetrize the implosion of the fuel pellet. Shorter wavelengths appear to be more satisfactory, and work is proceeding on ultraviolet excimer lasers, such as krypton fluoride, but the optics problems for these wavelengths are severe. Glass lasers can be frequency shifted to the third harmonic with good efficiencies, although the basic efficiency of the glass laser itself is too low to provide the driver for a laser fusion reactor. However, this technique is being pursued at other laboratories.

The Los Alamos program is now emphasizing investigation of physics problems of interest to the weapons programs, Because this effort appears to be increasingly productive, program funding and support is expected to continue. In spite of the apparent difficulties associated with the long wavelength of the CO_2 laser, it may be possible to find clever target designs that permit the many advantages of this laser to be used for successful initiation of the fusion process. Other laser activities, such as the Free Electron Laser program, are now expanding both the Laboratory's interest in and its commitment to laser technology. ■

The Reactor Safety Program

by Kaye D. Lathrop

Although Los Alamos has had a long history of individual contributors to the safety of reactors, including Hans Bethe, George Bell, and William Stratton, the reactor safety research program now conducted by the Energy Division began in 1972 in the Theoretical Division. At that time, in reactor physics and safety circles, there was a slowly increasing realization that our ability to predict the consequences of possible reactor accidents was woefully inadequate. The safety review process for the Fast Flux Test Facility at Richland, Washington had resulted in a heated and prolonged debate between the safety analysts at Argonne National Laboratory and the construction project managers at Hanford because the results of the safety analysis implied greatly increased design and construction expense. Somewhat earlier, the first major performance tests of a simulated light-water reactor emergency core-cooling system at the Semiscale Facility at Idaho Falls gave an unforeseen result. The emergency cooling water, instead of penetrating the core and cooling the system, simply flowed around the upper annulus of the apparatus and exited through the simulated pipe break. Although the Semiscale apparatus was about one-thousandth as large as an actual reactor, these disturbing results precipitated a lengthy set of hearings that culminated in a Code of Federal Regulations that limited the operating temperatures of existing and future reactors. Because of a lack of understanding of what would happen in a full-size reactor, these regulations embodied many "conservatisms" and in this sense were arbitrary.

So there existed a desperate need for an analytic predictive capability, especially because expense had prohibited and always would prohibit complete full-scale testing of safety systems. Jay Boudreau, William Reed, and I, members of the Transport Theory Group of the Theoretical Division, saw this need as an opportunity, each in a different way. Boudreau, who had written his doctoral thesis on possible supercritical configurations that might emerge from core rearrangements during fast reactor accidents, wanted to turn from his transport theory assignments to solve what he believed were truly important problems. Bill Reed, who had already demonstrated a brilliant mastery of computational transport theory, was anxious to extend his talents to hydrodynamics. And I had an implicit faith in the ability of a properly designed computer code to make correct predictions and was anxious for a new challenge. Further, in the reduction-in-force days of the early seventies, I needed new financial support for my group.

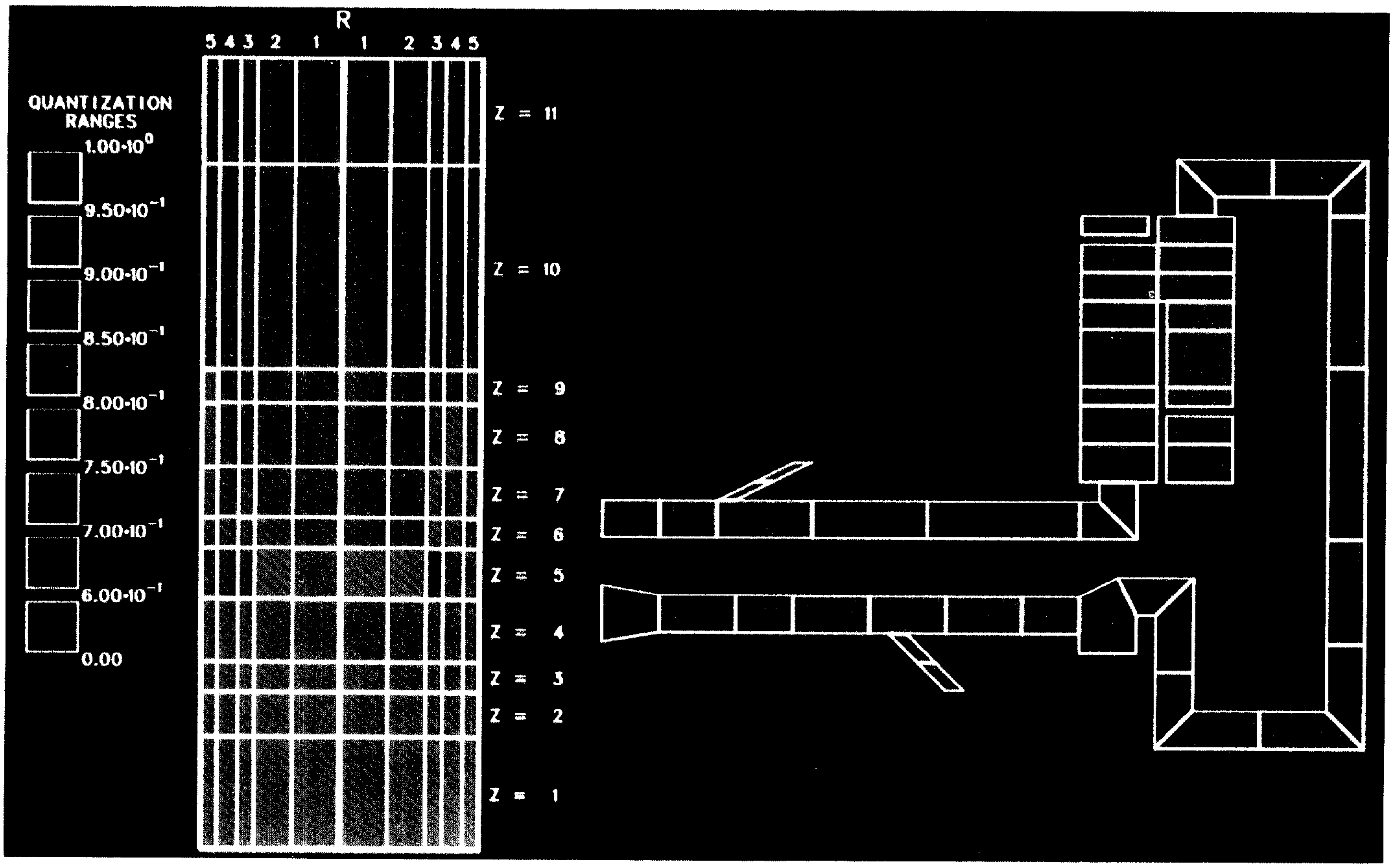
In my first 1972 foray to Washington, I was greeted by a skeptical branch chief with the sally, "Who are you, and what are your credentials?" However, in a widely attended Washington meeting on October 31, 1973, we presented a detailed proposal, authored by Jay Boudreau, Frank Harlow, Bill Reed, and Jack Barnes, for the development of the SIMMER (an acronym for S_n, implicit, multifield, multicomponent, Eulerian, recriticality) code to analyze fast reactor core-meltdown accidents. Although Los Alamos was outside the reactor safety community, the Laboratory's acknowledged leadership in computational methods and the existence of three groups in the Theoretical Division devoted to transport theory, hydrodynamics, and equation-of-state research convinced the AEC of our competence,

The proposal was funded, and work on SIMMER began in earnest in 1974. That same year, William Kirk and I began a more broadly based reactor safety research program on high-temperature gas-cooled reactors. Simultaneously, and almost as an afterthought, Reed and I agreed to develop a best-estimate computer code (subsequently named TRAC for transient reactor analysis code) to predict the effects of emergency core-cooling systems in light-water reactors. In retrospect, our self-confidence was astounding. We were blissfully ignorant of the difficulty of the task, and Los Alamos, despite long experience with high-temperature gas-cooled reactors and fast reactors, had no expertise with light-water reactors.

The Transport Theory Group grew rapidly in 1974 and 1975, becoming three groups in December of the latter year. Two of these groups formed the nucleus of the present 125-man reactor safety program in the Energy Division. The research of this program is the theme of the Summer/Fall 1981 issue of *Los Alamos Science.* The third group, headed by Warren Miller, remained as the Transport Theory Group of the Theoretical Division.

The success of the SIMMER and TRAC computer codes has been especially noteworthy because they must extrapolate. That is, they must make believable predictions outside the domain of experimental results. Versions of TRAC, in particular, have been used to predict results for dozens of experiments on many reactor components of scales up to full size and on integrated systems of various miniature scales. (The only full-scale, full-system data point for a light-water reactor emergency cooling system is Three Mile Island.) TRAC has a convincing predictive record. No other computer model of similar complexity, certainly not those of weapons design codes, can extrapolate with such confidence. SIMMER, while not yet as exhaustively compared with experiment as TRAC, has made two valuable predictions. First, contrary to previously accepted dogma, secondary and subsequent critical configurations can occur because of a core rearrangement during the course of a fast reactor accident. Second, and notwithstanding this first prediction, the energy released (and hence the containment expense) in fast reactor core-melt accidents is computed to be much less than previously predicted.

In addition to these technical achievements and of equal importance, the growth of the reactor safety program brought to Los Alamos many extremely capable people. These include Jim Jackson, who came from



Two examples of TRAC results. The graphic output shown here is color coded (left) according [o the fraction of vapor or steam in each computational cell. One example (middle) shows liquid water (blue) in the bottom of a pressurized-water reactor vessel filled with steam (red) following a postulated complete break in the largest coolant pipe leading into the vessel. The unique ability of TRAC to analyze 3-dimensional fluid motions in a vessel coupled to a full reactor system is proving valuable in addressing a wide variety of possible accidents in pressurized-water reactors. The output on the right shows steam-water flows in a loop of the Upper Plenum Test Facility (UPTF). Now in the design stage, this West German facility will include a full-sized vessel and several coolant loops to allow accurate simulations of fluid behavior during the core-reflooding stage of a large-break loss-of-coolant accident in a pressurized-water reactor. TRAC is being used extensively in the design of UPTF as part of a $300-million cooperative program among the United States, Japan, and West Germany.

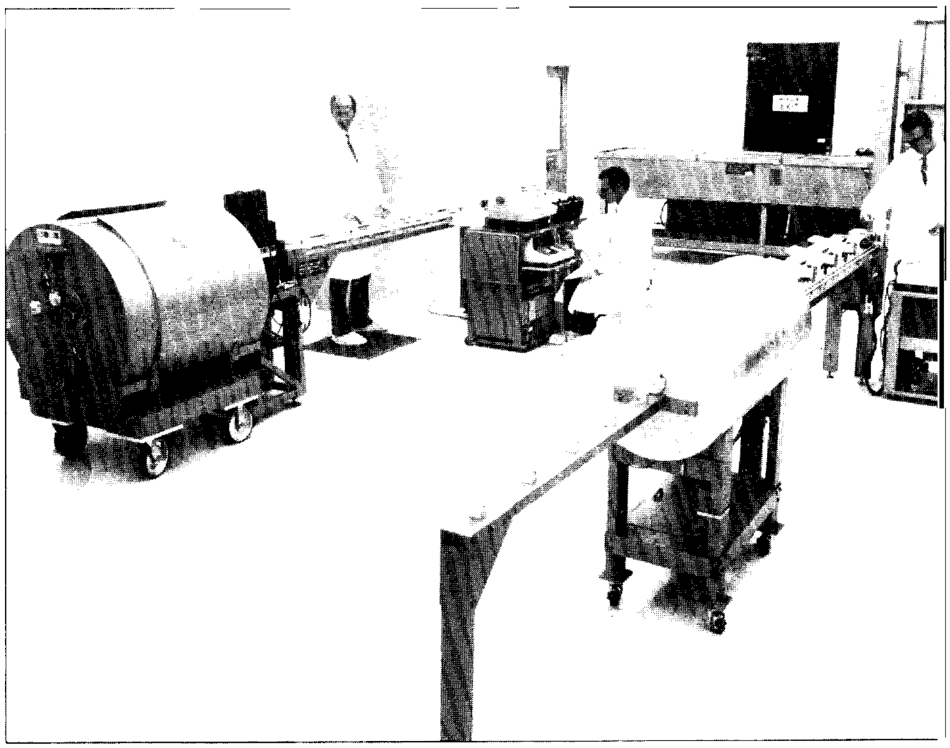
Brigham Young University to take charge of TRAC development during a crucial phase and is now head of the Energy Division; his deputy, Mike Stevenson. who came from Babcock & Wilcox via Argonne to head the high-temperature gas-cooled reactor analysis effort: Charlie Bell, who came from Atomics International to solve SIMMER heat-transfer and hydrodynamics problems; Walt Kirchner, who finished his doctorate at MIT in time to write TRAC heat-transfer routines: Dennis Liles, an expert in two-phase flow hydrodynamics from Georgia Tech who has been invaluable to TRAC development: John Mahaffy, a postdoctoral astrophysicist from the University of Illinois whose numerical hydrodynamics expertise has made TRAC faster; Rich Pryor, a Savannah River reactor physicist whose experience with methods and large codes was very valuable; Jim Scott, a Hanford fuel-behavior specialist; Ron Smith, from Argonne: Ken Williams. from Georgia Tech: Dominic Cagliostro, from SRI; John Ireland, from General Electric; Thad Knight, from EG&G; and many more. ■

The Nuclear Safeguards Program

compiled by Darryl B. Smith

"Los Alamos's interest in safeguards . . should not really surprise you. Our pioneering work in nuclear weapons has left us. . . with the profound concern that these devices never get used in anger, never get used surreptitiously, never get made by surprise. by theft. or by diversion," Dr. Norris E. Bradbury used these words in his welcoming remarks to the more than three hundred and fifty participants in the Second AEC Symposium on Safeguards Research and Development held in Los Alamos in October 1969.

Immediately following the end of World War II there was a hope that the proliferation of nuclear weapons could at least be delayed by means of rigid controls over all nuclear activities (the Baruch Plan, 1946). Despite efforts by the United States to maintain strict secrecy, by 1952 three additional nuclear weapons states had emerged, and several nations were seeking the benefits of nuclear electric power. In 1953, President Eisenhower announced the "Atoms for Peace" program to promote vigorously the peaceful use of nuclear energy while discouraging or preventing any military use. In the course of implementing this policy, the International Atomic Energy Agency (IAEA) was created in 1957 and entrusted with the international promotion and control of peaceful uses of nuclear energy.



Nondestructive assay of fast breeder reactor fuel. Two fuel-rod scanners developed by Los Alamos are being used here in 1974 at the Hanford Engineering Development Laboratory as part of their safeguards and quality control. The device on the l@ uses a computerized californium-252 system to measure both plutonium content with an accuracy of better than 0.5 percent and pellet-to-pellet uniformity of fissile material loaded into the rod. The system on the right uses a passive neutron-coincidence technique to measure plutonium-240 content, thus providing a cross-check with the first instrument.

The Los Alamos Nuclear Safeguards program began in 1966 when worldwide interest in nuclear energy for the production of electrical power was rapidly expanding. Bob Keepin, a nuclear physicist in the Nuclear Propulsion Division, had just returned to Los Alamos after two years as head of the Physics Section, Division of Research and Laboratories of the IAEA in Vienna, Austria, and was firmly convinced of the coming importance—both political and technical—of the worldwide nuclear safeguards problem. He was equally convinced that Los Alamos should launch a vigorous program to develop new nondestructive assay techniques and instruments that would in time provide the technical basis for meeting the increasingly stringent safeguards requirements that were inevitable. Following a lengthy series of briefings, hearings, buttonholing, and budget reviews with the AEC and the Congressional Joint Committee on Atomic Energy, the nation's first research and development program in safeguards was funded and launched at Los Alamos in December of 1966. Six months later, the AEC established a new Office of Safeguards and Materials Management (OSMM) as well as a Division of Safeguards in its Regulatory Branch. The Regulatory Branch is now the Nuclear Regulatory Commission (NRC). The OSMM is now the Department of Energy's Office of Safeguards and Security and still provides the lion's share of the $12 million Safeguards research and development program at Los Alamos.

Bob was named to head the new program, which began in a small laboratory at Pajarito Site replete with chipmunks in the offices and a rattlesnake on the doorstep. As the program grew, this space was augmented a year later by the addition of a second, larger laboratory at another site. With the encouragement and cooperation of Dick

In-line monitoring of uranium hexafluroide (UF_6) enrichment. This system, shown installed in 1975 at Goodyear's Atomic Gaseous Diffusion Plant in Piketon, Ohio, also uses two independent sensors developed at Los Alamos. The gamma enrichment meter measures the percentage of uranium-235; the neutron detector measures the percentage of uranium-234. This in-line instrument allows instantaneous isotopic analysis (to better than 0.5% accuracy), providing assurance of criticality safety during withdrawal into large cylinders as well as verification that the product selection meets the enrichment specifications. Because uranium-234 is also enriched in the diffusion process, its isotopic abundance in the product UF_6 provides useful diagnostic information for plant operation. The alpha-particle activity of uranium-234 is the principal source of neutrons emitted by enriched UF_6, and this neutron yield is an important signature for safeguards verification.

Baker, Chemistry-Materials Science Division Leader at the time, and the tolerance of Bill Maraman and his Plutonium Chemistry and Metallurgy Group, a special technical liaison committee was set up in 1967 to encourage cooperation among safeguards researchers and those staff whose group or division responsibilities were directly concerned with nuclear materials and equipment, This committee helped to identify needed, practical applications for testing and applying newly developed safeguards techniques to materials measurement, accountability, and safeguards problems. Such problems were not uncommon in the materials processing, fabrication, and recovery operations carried out routinely at the Laboratory's plutonium facility. The close liaison between safeguards researchers and the Laboratory's plutonium chemists and metallurgists significantly helped the Los Alamos Safeguards program get off to a head start in the safeguards field with a commanding lead that has been retained ever since.

The Agnew years saw the Los Alamos Safeguards research and development program grow by more than an order of magnitude. At the beginning of the '70s, most of the nuclear industry was unaware of the importance and economic impact the nondestructive assay techniques could have on their operations, so in the spring of 1970 Los Alamos fielded the Mobile Nondestructive Assay Laboratory (MONAL) to serve as a demonstration unit and assay laboratory and as a staging area for conducting in-plant assay using portable instrumentation. During the next few years, MONAL traveled to nuclear facilities nationwide, addressing special measurement problems. The first Los Alamos instrument installed in a nuclear facility for routine production use went to the General Electric fuel fabrication plant in Wilmington, North Carolina, in the spring of 1971 to assay reactor fuel rods. By the end of the decade, instruments and techniques developed by Los Alamos were in use throughout the world. In November 1973, the Safeguards staff conducted its first formal course in nondestructive assay techniques. By 1980 nearly seven hundred people had received training in safeguards techniques at Los Alamos, and currently about two hundred students participate each year in the eight to ten courses offered, including all new IAEA inspectors, who come to Los Alamos for their initial training.

Today, the Los Alamos Safeguards program is recognized worldwide as the fundamental source for state-of-the-art safeguards technology and has been designated as the DOE's lead laboratory in nuclear materials control and accountability research and development. It encompasses all aspects of the design, development, testing, and in-plant evaluation of new techniques, instruments, and integrated systems for safeguarding fissionable materials in all types of civilian and national defense nuclear facilities. ■

The Hot Dry Rock Program

by Morton C. Smith

It is not often possible to trace the ancestry and list the immediate family of a new idea, but in this regard—and some others—the Hot Dry Rock Geothermal Energy program is exceptional.

Since its establishment as Site Y of the Manhattan Project, the primary mission of Los Alamos National Laboratory has required information that could be acquired only from experiments done in nuclear reactors, and reactor expertise has always been one of its greatest strengths. It was therefore quite natural, when a national need appeared for higher-performance rocket-propulsion systems, that the Laboratory should propose the use of compact, gas-cooled nuclear reactors. The result was the Rover program.

One of the reactor concepts considered in the early days of the Rover program was Dumbo, a fast reactor with a refractory-metal-composite core built as a honeycomb structure. To demonstrate the heat-transfer characteristics of such a structure, a resistively heated laboratory-scale model of a core section was built and used to heat a hydrogen jet to above 3000 degrees Celsius. The demonstration was impressive, and when Dumbo was abandoned in favor of a graphite-core reactor, some of the Dumbo advocates felt that a gadget that good must have other uses. In particular, Robert M. Potter (now a Laboratory Fellow), after rereading the Edgar Rice Burroughs novel, *At the Earth's Core,* concluded that something like it could as well be pointed down as up and used to melt holes in rock more rapidly and efficiently than they could be produced by drilling or tunneling. The result, some years later, was the Subterrene program—development of a rock-melting earth penetrator.

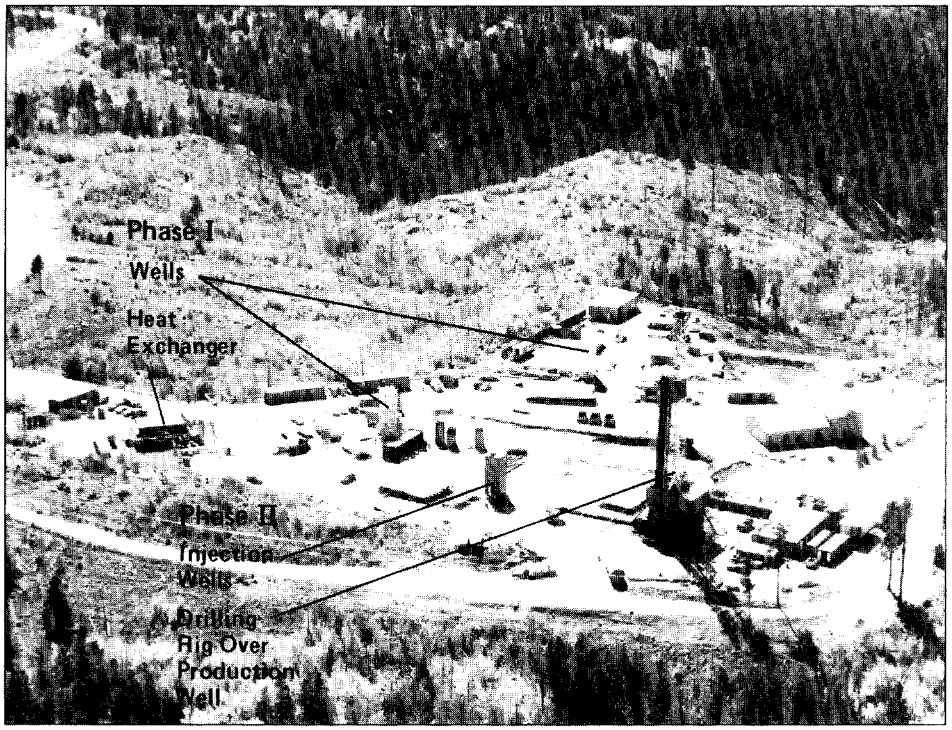
In 1970 the late Eugene S. Robinson assembled an *ad hoc* committee from several Laboratory divisions and disciplines to examine the possibilities and problems of the Subterrene. One of the obvious problems was disposal of the molten glass produced when a rock is melted. Again Potter had a suggestion. He had been reading about drilling in oil and gas fields and had learned about hydraulic fracturing—the use of fluid pressure to produce large cracks extending outward from the well to facilitate drainage of fluids into it, He proposed that sufficient pressure could be developed in the melt ahead of a penetrator to produce such cracks and force the glass into them, where it would freeze and remain. This idea was never actively pursued in the Subterrene program, but it appeared to the committee that hydraulic fracturing had many other possibilities. One of the most important of these, they concluded, was its use to create flow passages and heat-transfer surface in naturally heated crustal rock whose initial permeability was too low to be usefully productive of natural steam or hot water—"dry hot rock."

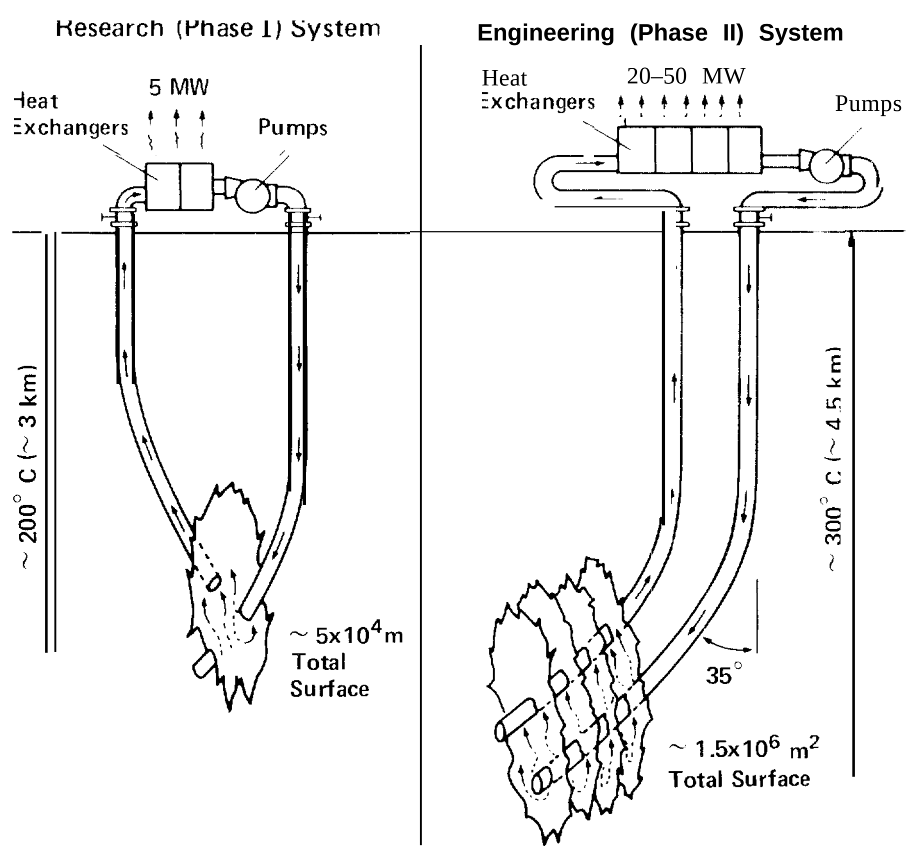
The method proposed by the committee was to drill a hole from the earth's surface to a sufficient depth to reach essentially impermeable rock at a usefully high temperature; to produce a large hydraulic fracture near the bottom of the hole; to drill a second hole from the surface to intersect that fracture; to pump water down the first hole to circulate through the fracture and extract heat from the rock around it; to recover the hot water through the second hole under sufficient pressure to prevent boiling; to extract its useful heat; to then return the water to the first hole to recirculate and extract more heat.

When the Subterrene program had been launched, Bob Potter and I assembled a group of volunteers and initiated a "Dry Hot Rock Geothermal Energy program" to investigate this concept. (The name was subsequently changed by someone in Washington who thought that "Hot Dry Rock" was more euphonious.) Initially the program was unofficial, unfunded, and supported largely by faith and the tolerance of Laboratory management. Most of the first year's work was done on weekends and holidays, and much of it in snow up to there. However. in 1971 the group managed to digest much of the existing information on geothermal areas and the equipment and techniques needed to create a dry hot rock energy system, and to begin a terrestrial heat flow study in the Jemez Mountains west of Los Alamos. In 1972 that study was concluded and, with discretionary research and development funds provided to the Laboratory by the Division of Military Application of the AEC, an exploratory hole was drilled in Barley Canyon—about 30 kilometers west of the Laboratory. The hole reached a final depth of 785 meters, penetrated about 143 meters of granitic basement rock, and had a bottomhole temperature of 100.4 degrees Celsius. With additional funding from the Division of Physical Research of the AEC. hydraulic-fracturing and pressurization tests were run in the lower part of the hole, and it was concluded that the basement rock was well suited to creation and containment of a pressurized-water heat-extraction loop.

With this encouragement and the prospect of substantial funding from the newly formed Division of Applied Technology of the AEC, an official 'Los Alamos Geothermal Energy Group was formed early in 1973, with myself as Group Leader. The anticipated funds materialized, and in 1974 a deeper exploratory hole was drilled at a more accessible and convenient location—on Fenton Hill, about 2.5 kilometers south of Barley Canyon. This hole reached a depth of 2930 meters and a rock temperature of 197 degrees Celsius. Experiments in it confirmed the observations previously made in Barley Canyon, but at greater depth and higher temperature.

In 1975 a second hole was drilled at Fenton Hill (photograph and figure) to a final depth of 3064 meters and a rock temperature of 205 degrees Celsius. A poor connection was made between hydraulic fractures produced from the two holes. After considerable experimentation and much development of new equipment and instruments, the connection was improved in 1977 by redrilling one of the holes, and in 1979 the





The photograph shows the hot dry rock geothermal site at Fenton Hill, looking southwest. Phase I of the project, shown schematically on the l@ side of the figure and with its two wells and heat exchangers labeled in the photograph, was completed in 1979 and has been producing heat at rates high enough that several hundred homes could be heated. It is hoped that when Phase II (labeled in photograph and right side of figure) is completed, heat production will be sufficient to demonstrate that a commercial electric power plant could be supported.

underground loop was enlarged by additional hydraulic fracturing (Phase I). With an air-cooled heat exchanger at the surface to dissipate the heat, this pioneering hot dry rock energy system has been operated intermittently since 1978 as a closed. recirculating pressurized-water loop. Heat has been produced at rates up to 5 megawatts (thermal), which would heat several hundred homes if there were that many nearby. The longest continuous run lasted nine months and had no detectable environmental effect. Some of the heat has been used to generate electricity in a 60-kilowatt binary cycle plant, but neither the temperature nor the rate of heat production was sufficient to support a commercial power plant. Therefore, a larger, deeper, hotter system (Phase 11) designed to demonstrate that capability is now being constructed at Fenton Hill.

While the objective of the Hot Dry Rock program has always been the very practical one of’ making a vast, indigenous energy supply useful to man, the effort to do so has necessarily included a wide variety of supporting research and development activities—many of them done cooperatively with industrial organizations, university groups, and complementary programs at other laboratories and in other countries. To justify existence of the program, the very large resource base of thermal energy at accessible depths across the entire United States had to be evaluated. To implement the field program. it was necessary to develop drilling, well-completion, and hydraulic-fracturing equipment and techniques usable in very hot, inclined geothermal wells and also downhole instruments to log such wells and collect data in them. And to analyze and understand the information collected in the field has required both theoretical and laboratory studies of rock-water interactions, fluid and rock mechanics, heat transfer and transport, acoustic emissions, and other subjects. The program is broadly interdisciplinary and covers the entire spectrum from basic research to engineering application.

Since its inception, the Hot Dry Rock program has been supported primarily by the AEC and its successor agencies, ERDA and DOE, with supplementary support since 1980 by agencies of the governments of West Germany and Japan. However, the most important support has come from people like Harold Agnew, Director of the Laboratory during most of the history of the Hot Dry Rock program and always its most personable, articulate, and effective advocate. ■

1979–

THE KERR YEARS

Challenges and Prospects

by Donald M. Kerr

On this occasion of the 40th anniversary of the founding of the Los Alamos Laboratory. I would like to shape in broad outline my hopes for the Laboratory in the next decade. Though some of what I will say may go beyond what might be labeled as realistic, we must have such high hopes, for they help us stretch our capabilities. I will also address some substantial obstacles that could, if not countered, negate our best attempts to help the nation solve some of its pressing problems.

My first hope is that Los Alamos scientists will play a prominent role in reshaping the defense posture of America through efforts along three lines—arms control, nuclear weapons, and advanced weapons concepts.

The people of this planet have no more important task than to subdue the spiraling arms race and to eliminate the fear that, by accident or by design, nations might eliminate large portions of life on this earth by engaging in a massive nuclear exchange. While science cannot solve the political problems that snarl arms control talks, improved technology in satellite surveillance, seismic detection, and information analysis can help decrease the possibility of agreement violations through surprise actions, clandestine activities, or new developments. Such technological assistance is not likely to be the key element in advancing attempts to curb the arms race but may be useful if political developments become favorable.

Our nation's efforts toward arms control must be made from a position of strength. And that strength depends on being at the forefront of all scientific areas likely to yield new military applications. In the area of nuclear weapons, Los Alamos can make the following specific contributions.

- Encourage the modernization, where appropriate, of nuclear warheads to provide the best safety and security features technology can offer.
- Assure the effectiveness of nuclear weapons over a wider range of operating conditions.
- Improve the protection of warheads against newly developed electronic countermeasures designed to defeat our weapons.
- Develop new means of making our weapons more effective

against hardened targets in the Soviet Union.

- Improve the techniques for defending our own strategic forces from a first strike.
- Determine the feasibility of newer weapons, including those involving particle beams and lasers.

Finally, Los Alamos can contribute to the nation's defense through efforts in what we call advanced weapons concepts. This Laboratory was created to meet what was viewed as the most critical defense issue facing the country in World War II—the possibility that our enemies were developing a weapon based on new science and new technology. It is vital that the critical military needs currently facing the nation be met in a similar fashion today.

One advanced weapons development would be the introduction of truly intelligent weapon systems to the battlefield. Such systems have been discussed and popularized, but the immensely difficult task of developing them, although possible, remains to be done. I have in mind a weapon system including multiple sensing techniques coordinated by sophisticated electronics and computing capabilities. The intelligent weapon system would be integrated into an overall battlefield posture involving land, sea, and air forces.

Ten years or so ago the prospects for artificial intelligence were oversold, and work in that area received a bad name. But significant developments over the past decade suggest that now is the time to initiate its application. Already a number of techniques for using computers as expert systems are in the early stages of application. For example, one computer manufacturing company is using a modest form of artificial intelligence to establish the appropriate configurations of computer systems for purchasers. A computer programmed with more than two thousand rules and fed the requirements of the purchaser determines the configuration of equipment that best meets those requirements. Another and perhaps the most widely noted example is the use of computers in medical diagnosis to help physicians make the complex judgments required of them when faced with multiple symptoms and test results. In over 95 per cent of the tests thus far, diagnoses made by the computer agree with those of expert physicians.

The eventual goal in a military context is a weapon system that can be sent into a battle situation to sense and analyze many complex, perhaps rapidly varying factors, such as terrain, environmental conditions, and the nature and movement of enemy forces and weapons. The system, controlled by artificial intelligence, would make the decision as to which of its weapons to deploy and in what manner they would best be utilized. Such a system may sound far-fetched to some, but the technology required has progressed to the point that it should be vigorously pursued.

A nation possessing an intelligent weapon system would have a great tactical and psychological advantage over its enemy. Furthermore. smart weapon systems equipped with today's advanced nonnuclear warheads could displace low-yield, short-range nuclear weapons and thus considerably reduce the tension associated with the posting of nuclear weapons close to an enemy's borders.

Research along these lines should be pursued, and Los Alamos, together with Livermore and Sandia, can make important contributions in the next ten years, if properly supported and freed of extensive program strings, milestones, and reporting requirements. Modest funding of a few million dollars per year to each of the weapon-related national laboratories would be a sufficient beginning.

There are many other exciting advanced weapons concepts; I will mention only a few. We have ideas for antiterrorist technology that could reduce the impact of threats in many areas. We see means for detecting and protecting against chemical and biological threats. And we see a possibility of developing microwave weapons, which could become very important as electronics becomes more and more integrated into the battlefield.

My second hope is that the Laboratory will make major contributions to solving a problem that has commanded great public attention—the problem of supplying the energy needs of the nation and the world. The Laboratory has devoted a substantial effort to energy programs during the past decade, and it is my hope that as these efforts reach maturity in the coming decade, they will bear technological fruit in the following forms.

- Safety and engineering advances that will make nuclear power a more acceptable approach when the world turns again to this energy source, as I believe it eventually will.
- Nuclear waste disposal techniques that will satisfy public concerns.
- Techniques for extracting fossil fuels from the earth that will provide greater efficiency and worker safety and cause less pollution and environmental damage.
- Practical fuel cells that will power many diverse activities, from transportation to materials production.
- Geothermal projects that will tap the heat of the earth's mantle to provide a clean and safe supply of heat and electricity.
- Advances in renewable energy technologies that will allow for decentralized energy supplies so necessary in rural America and in many developing nations.

Controlled fusion is a major area in which we have made and continue to make important contributions to the development of a new energy source for future use. Since the early 1950s Los Alamos has played a major role in the international development of magnetic confinement science and technology. This cooperative effort has led to such a high level of sophistication that demonstration of energy break-even, using the mainline tokamak approach, seems assured during this decade, The ability to confine reactor-grade plasmas for times close to those required for thermonuclear ignition is an enormous scientific accomplishment that could not have been achieved without the resources that national laboratories, universities, and industry brought to bear on this problem.

At the same time it is clear to me that the demonstration of scientific feasibility on the tokamak will not automatically assure its economic feasibility as a power-producing system. It is likely that proof of commercial feasibility will fall to a different fusion concept whose inherent confinement requirements reduce engineering complexity and therefore cost to the point where it can become a practical system for the nation to adopt, or perhaps commercial feasibility will fall to much more advanced tokamak systems yet to be developed.

I believe the work going on at Los Alamos will play a significant role in developing a power-producing fusion reactor. I am encouraged in this respect by recent successful developments in our Reversed-Field Pinch and Compact Toroid programs because the efficient confinement properties of these schemes provide the magnetic fusion program with a new possible end-product: the compact, high-power-density reactor. This new approach efficiently utilizes resistive copper magnets and therefore differs qualitatively from the conventional reactor models, based on superconducting magnets, in greatly reducing the size, mass, complexity, and cost of a reactor and the time required for reactor development. These alternative fusion concepts are at an earlier stage of scientific development than the tokamak. Their potential for resulting in a significantly better commercial product provides the rationale for support in a well-balanced and prudent national program. Ideally, in such a program the allocation of resources will permit the full potential of these alternative concepts to be realized so that their best reactor attributes can merge with the more mature development base for the mainline approach to produce an optimized fusion system.

Diverse funding of numerous approaches is the best means for overcoming the great technical challenges posed by controlled fusion. If such funding occurs, I believe that Los Alamos can develop fusion power systems that are smaller, cheaper, and more easily maintained. Such developments may enhance the willingness of society to adopt this form of technology.

My third hope concerns the application of the Laboratory's expertise in physics, chemistry, and engineering to the new challenges in the fields of biology and medicine. Two instruments of fundamental importance to biomedical research have been developed at Los Alamos. These are the liquid scintillation spectrometer, which makes possible simultaneous counting of different radioisotopes, and the flow cytophotometer, which allows rapid analysis and isolation of individual cells. The latter development resulted in the establishment at Los Alamos of the National Flow Cytometry Resource. Current activities give me confidence that the next decades will see developments of similar importance to biology and medicine.

For example, improvements in flow cytometry now allow rapid identification and separation of chromosomes. This capability, coupled with powerful recombinant DNA techniques, opens new approaches in cell biology and genetics. The chromosome rearrangements characteristic of tumor cells can now be closely scrutinized, and this information may provide insight into the origins and abnormal behavior of cancer cells. With similar techniques cultured plant cells may be manipulated to produce new crop varieties with desired genetic characteristics, such as disease resistance and environmental tolerance.

Another example is the development of noninvasive techniques for analyzing human functions with minimal discomfort to the patient. In one such technique a nuclear magnetic resonance coil is used to follow the course of metabolic processes from outside a patient's body. The coil detects important intermediate products of metabolism that have been labeled with a suitable magnetic isotope, such as carbon-13. The labeled materials are available from the Laboratory's Stable Isotope Production Facility, which pioneered in the field of stable isotopes for biomedical research.

The Laboratory is also developing advanced physical techniques for biological and medical applications. Examples include rapid, precise identification of microorganisms based on their scattering of circularly polarized light and detailed structural analysis of biological macromolecules based on neutron and x-ray spectroscopy.

Another venture into the realm of biology exploits our computing capability—the largest in the world—to compile and make available to the scientific community a library of genetic sequences. Los Alamos has recently been designated as the site of the national DNA sequence data bank. This data bank will contribute significantly to unraveling the mysteries of DNA.

The Laboratory has a major responsibility in developing secure alternative energy sources such as shale oil. Experimental shale retorts and advanced capabilities in cellular and genetic toxicology provide the opportunity to choose extraction and processing methods that produce the least harmful pollutants. This will involve using the advanced techniques described above to study the effect of pollutants on cells.

It is my hope that, with strong inputs from academia and industry, the advanced physical, theoretical, and computational capabilities of Los Alamos will contribute to a decade of imaginative and striking benefits in the areas of biomedical research, energy development, and environmental science.

My fourth hope is that the Laboratory will continue to involve an increasing number of scientists from universities and industry in its activities. We have already made great progress in this area by establishing three centers designed to reach aggressively beyond our borders: a branch of the Institute of Geophysics and Planetary Physics, the Center for Nonlinear Studies, and the Center for Materials Sciences,

In terms of new efforts, I see the following possibilities.

- That not one but two or three of the world's most Powerful computers will be available beyond the bounds of our security fences for use by collaborating scientists from other institutions.
- That more and more students and faculty will become familiar with our activities and facilities by choosing to pursue research at Los Alamos.
- That our staff will increasingly aid in the transfer of technical information to industry and to universities by sharing in joint exchange appointments.

It is, of course, impossible to mention all significant advances expected in a laboratory as diverse as Los Alamos. But one final hope is that we will be surprised by some unexpected development or discovery that derives from the exploration of new questions and new possibilities. The very nature of scientific research makes such surprises possible, and for this reason basic research is a fundamental element in our plans.

To realize the hopes that I have outlined, difficult scientific problems will have to be confronted, pursued, and conquered. But those efforts now face challenges beyond the inherent scientific difficulty.

A changed political and social climate challenges these hopes. Some voices now question the major mission of the Laboratory. They ask, "Why is the Laboratory still engaged in weapons work?" That question often comes from those who believe that the thousands of nuclear warheads now in our arsenal are more than adequate and that no more effort in this scientific area is needed, These people deserve a reply.

Three chief factors drive our continued efforts in weapons, I touched on two of these above but their importance leads me to reiterate. The first is the extent to which potential enemies of the United States are making technological advances that could jeopardize the defense posture of the United States. This issue led to the creation of the Manhattan Project during World War II, and it is still a valid concern in the present political climate. Our political leaders generally feel that their ability to influence world affairs is affected by the extent to which the United States maintains technological supremacy in the defense area.

The second factor is the need for solutions to technical problems that may inhibit accords on arms control. Any agreement on this subject rests heavily on the ability to determine that its provisions will be followed by each signatory, The inability to verify compliance has created stumbling blocks in past negotiations. The Laboratory must assist in developing new verification techniques, for they maybe a critical link in reaching the goal of arms control. The Laboratory will also be called upon to help policy makers understand the capabilities and limitations of current approaches to verification,

The third factor is the certain knowledge that the pursuit of science inevitably yields ideas for new technologies that have a wide variety of applications, including military ones. The choice to develop the new military applications is the nation's. But the nation cannot choose to stop the scientific effort that creates those applications without also stifling development in other human endeavors. Science is neither compartmentalized within itself nor isolated from its surroundings. New scientific ideas have a way of leaping traditional boundaries among fields of science and of creating vast and unforeseen changes in the economic and political fabric of society.

Another challenge facing the Laboratory is the idea of some that our research activities be transferred to academia and industry, You might ask, "What is the place of Los Alamos in the midst of the country's large and sprawling research community?" After all, research efforts at universities have grown substantially since World War H, and industry has also seen reason to invest in research and development.

I believe there is a clear place for Los Alamos and other national laboratories. That place goes beyond weapons work, which the government obviously must control directly, to other areas of research in which a strong national interest justifies the presence of a federally supported laboratory.

For example, many areas of research-a notable example being nuclear fusion—face such inherent difficulties that they will yield results only over a very long term, Industry will not be inclined nor financially able to enter such areas. Another example is the area of research on the protection of workers, the public, and the environment from technologies new or old. Here the profit motive of industry may bring into question their objective assessment.

National laboratories such as Los Alamos can address these issues, and, in fact, Los Alamos is extraordinarily well equipped to do so. Our scientific computing capabilities are unsurpassed. We have the experience of dealing with military agencies and understand their needs and procedures. We can work in a way sometimes referred to as vertical integration: that is, we can develop an idea for, say, an instrument all the way from conception to production engineering. Our activities range from undirected basic research to production engineering of devices that weigh tons. We can transform ideas or bits of Nature's secrets into products useful to mankind. Of the thousands of laboratories in the nation only a small handful match this Laboratory's capabilities.

The world is increasingly specialized, compartmentalized, separated into isolated parts. The concept of integrated teamwork bringing mathematicians, physicists, chemists, biologists, engineers, and economists together for a sustained effort is not a tradition at very many institutions. In fact, it seldom happens. It is difficult to bring about. In many places it is impossible. At Los Alamos it is the usual practice. It is the way we have conducted business from the beginning.

The third challenge facing the Laboratory in the next decade concerns the level of financial support for its activities, particularly for basic research. Funding reductions can harm our work in important ways, and basic research often suffers more harm than other areas because sponsors are inclined to view it as less important than work closely coupled with an approaching milestone.

In the mid 1970s Congress established a new budget process in recognition of the need to review federal economic policy and to reduce the federal deficit. The resulting tighter budgets and economic policies have affected virtually all the Laboratory's activities and present a most serious challenge. My hopes for Los Alamos cannot be realized unless increased funding is available. The requested increases are modest but essential and represent a valuable investment for the nation.

The Laboratory is being asked to make sure that its work in major programs connects directly to program objectives that will yield usable technological applications. This emphasis must not be overdone, and in some cases that line has already been passed. When investigations have reached the stage at which such requests are appropriate, the emphasis may help us do what we want to do—to show that our work can solve national problems and lead to benefits for the nation and the world.

But we must constantly guard against demands for immediate, practical benefits from science. When basic questions are still being explored, when answers are only beginning to appear, and when technological applications are only dimly perceived, then questions of practical benefit must be deferred. If we at the Laboratory do our job well, we will open new areas of science that eventually will yield benefits. The nation must allow competent scientists to explore those areas and to confront the difficulties that may take years to overcome, satisfied that this investment is worthwhile. Budgetary restraints must not be allowed to force out all but research that is immediately applicable, for that course would amount to eating the seed corn of future harvests.

Let me conclude with a final challenge—the desire of some that science should overcome the tangled web of politics and assure that all its results are used only in positive ways. Such a desire is natural, but it is too much to expect of any single sector of society.

At the end of World War II, those at Los Alamos learned with the rest of the world that technical developments were beyond the control of the small group of scientists who pleaded that the results of their work be used solely for peaceful purposes. That control rests with the broader institutions of society. Today we continue to pursue the unanswered questions of science in the belief that our efforts will enhance the peace and prosperity of the world. The ultimate hope of those of us-at Los Alamos is that the voices for peace will prevail in all decisions that affect the use of our endeavors. ■

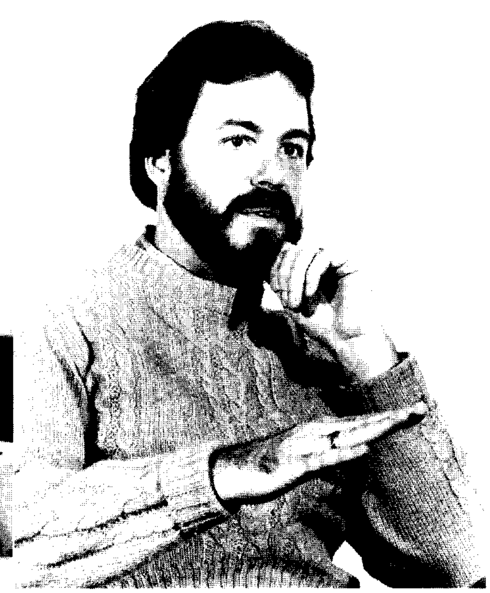
What's Happening Now...

What better way to learn about the state of the Laboratory—its present excitement and its future possibilities—than to talk with some of the outstanding scientists at Los Alamos. We chose ten who represent a wide spectrum of fields and asked them to share their personal views on the mission of the Laboratory, the current work, the management of research, and some pragmatic directions for the future.

SCIENCE: *I know that many of you chose to come to Los Alamos for personal reasons and are enthusiastic about its setting, its people, and your own work here. But Los A lames has always been a*

Dan Baker

Stirling Colgate



Brian Crawford

Rocky Kolb

Jeremy Landt

mission-oriented Laboratory, and I wonder how you view that mission and your role in it?

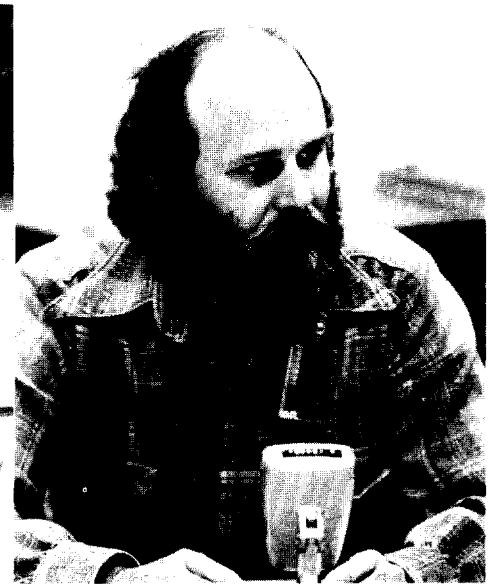
BAKER: Let me suggest a definition of the main mission of the Laboratory. Our mission is to provide input on all energy and national security issues that have a scientific or technological component. Is that general enough?

WHEATLEY: Yes. but I wonder whether the Laboratory's management has firmly in mind what technologies and ultimate applications we should be seeking.

HECKER: I personally feel that national security is our most important mission. Essentially, the country has entrusted to us and to Livermore their nuclear defense.

Sig Hecker

Steven Howe



Mac Hyman

Steve Rockwood

John Wheatley

Dan Baker on Space Science

The Vela satellite program to detect nuclear explosions in space has led scientists at Los Alamos to satellite exploration of the magnetosphere and of a wide variety of other space phenomena. Some of the instruments aboard such spacecraft have been designed to measure the interplanetary medium and planetary bow shocks, and we are doing theoretical studies in support of these observations. A related study is our work on cosmic particle acceleration. The information about energization of particles at interplanetary shocks may have applicability to shocks of much more cosmic proportions, such as those presumed to exist in supernova remnants.

We are also exploring the interplay between the solar wind [the hot, expanding corona of the sun] and the magnetic field of the earth. This interplay produces the magnetic structure we call the magnetosphere, the tenuous plasma region that makes up the uppermost part of the earth's atmosphere. We are doing computer modeling of the entire magnetosphere and, furthermore, are developing computer network links to many other institutions involved in similar work.

In a more practical vein we are using our advancing technology to do experiments in which we release chemical tracers into the ionsphere or even deeper into the magnetosphere to learn in what way these additives may modify the outer parts of the earth's environment.

Still another project is attempting to use an existing satelite in a different and innovative way. The International Sun-Earth Explorer 3 [ISEE-3] spacecraft has been orbiting at the L-I

LANDT: Certainly the Laboratory is aware of its obligation to help the country defend itself and to maintain a balance of technologies. Right now I am assigned to the Weapons Advanced Concepts Program Office, which was begun a year ago to try in a practical way to determine which technologies really make a difference for the national defense so that the country won't throw its money away on the wrong things. The Laboratory management is very interested in addressing this issue, and they have put dollars behind it and people to work on it.

ROCKWOOD: Today the government's method of doing business is very much applied and mission-oriented. Although basic research is also essential to our national security mission, it is often overlooked, and the national laboratories are handcuffed in this area by administrative limitations. People here have to be clever in extracting from their mission-oriented programs good basic results in science. I think Los Alamos has been rather successful at that.

WHEATLEY: Do you think mission orientation is a good thing? As a matter of principle?

ROCKWOOD: Moderation in all things.

BAKER: I think we must tight this trend toward applied work only, toward everything having an immediate payoff. A national laboratory should play as active a role in basic research as any laboratory. The country will suffer in the long run if we don't.

ROCKWOOD: Often the most exciting and fundamentally useful part of a program is not its stated objective but some unplanned spin-off. In the laser isotope separation program, spectroscopists working to explain the spectrum of the octahedral molecule UF_6 discovered that the octahedral symmetry group had originally been analyzed incorrectly and had been wrong in the literature for years. Even a very applied program may yield results of use to basic science.

BAKER: That's certainly been true in space physics. The Vela satellite program to detect nuclear explosions deep in space was a mission-oriented project, and we continue to have test and verification activities. To accomplish that practical goal we had to place instrumentation on the spacecraft to measure the environment. As a result, many properties of the magnetosphere were discovered.

Now the space physics groups are involved in a number of activities on collisionless shock waves, cosmic particle acceleration, the interplay between the solar wind and the earth's magnetic field, and the exploration by the International Sun-Earth Explorer 3 satellite of the night side of the earth.

SCIENCE: *How do you get funds for all these activities?*

BAKER: In a variety of ways. We have been able to obtain reimbursable funding from NASA [National Aeronautics and Space Administration] for some of our projects. But the continuing money from the weapons program gives us more stability than we could ever obtain from reimbursable funding alone. When we get our funding from the DOE [Department of Energy] or from the Laboratory, we are better able to make long-range plans. It's fortunate for us that the Europeans are also participating in many of our scientific satellite programs because the European Space Agency plans much further ahead than NASA does.

HYMAN: There are some problems with diversified funding. The Mathematical Modeling and Analysis Group in the Theoretical Division is almost completely basic research, and we also have been obtaining some support from outside the Laboratory. The largest block grant we have supports only one and one-half staff members. Because our funding comes in such little pieces, we are perpetual job hunters and odd jobbers—always knocking on a different door.

ROCKWOOD: The country hasn't learned how to fund basic science at all. Research doesn't integrate with time. Each administration

point on the sunward side of the earth for about four years. The L-1 point, the sun-earth Lagrangian point, can be thought of as an imaginary center of mass around which the satellite has been traveling in a large looping orbit. Now this satellite has been moved into the earth's distant magnetotail and is orbiting well downstream on the night side of the earth. It will be the first spacecraft to explore that region in space. To accomplish the move, the satellite's gas-jet thruster, which ordinarily performs minor station-keeping orbital adjustments, was used to move the craft in such a way that it encountered the moon's gravitational pull and got a lunar gravitational assist to kick it deep into the magnetotail. It is not in a stationary orbit, and thus the lunar encounters must occur every one to three months in order to keep the satellite deep in the magnetotail. Eventually another lunar push will occur, and ISEE-3 will go on to intercept a comet. This will be the first time that any spacecraft has gotten close to a cometary body.

Bob Farquhar, a very creative guy at NASA who seemingly can move any satellite anywhere you want using any other celestial object, helped with the ISEE-3 project and has also helped to plan what is called the International Solar-Polar Mission. Because we don't have enough energy in most launch vehicles to get significantly out of the ecliptic plane [the plane of the earth's orbit], we are sending a satellite out to Jupiter to get a large gravitational kick from that massive planet. The spacecraft will then move above the ecliptic plane and travel high over the sun's pole, another previously unexplored region. ●

comes in and has a new policy. Basic science suffers more from these oscillations than it would from a low level of sustained funding. And I believe Los Alamos suffers more from funding oscillations and changes in direction than other national laboratories. Our normal attrition rate is about 4 per cent per year. Any change in direction by more than that amount involves moving people around. People's skills are not always totally applicable to a different program, and those who are not absorbed by other parts of the Laboratory are not absorbed by the town at all. It is this very closed environment, which drastically constrains our flexibility, that I see as a major problem for the Laboratory. It always has been so.

Returning to the question of the funding of basic research, I feel that, although the government can't just pour out money and expect nothing in return except good intentions, the funding "pendulum" has swung too far toward applied activities.

WHEATLEY: Some of you would say that Los Alamos ought as a matter of principle to devote some fraction of its work to purely unqualified basic science, the sole motive being to understand things better and to develop knowledge or whatever-to have fun, really. I would like to suggest that perhaps that's not true. Perhaps it is our responsibility to articulate the possible relationship between our work and some appropriate mission of this Laboratory. I am not thinking of explicit applications, necessarily. Let me give you a personal example. I think that it is appropriate that my work in thermal and condensed-matter physics should feed into thermal technology. broadly defined, that is to say, into technologies that involve the concepts of energy, work, heat, temperature, and so on.

Right now I am working on heat engines. I had set myself a semipractical problem that no one in industry would define as practical of course—but it was. It had to do with producing cold very simply. I had an idea for doing that with acoustics, so I started playing around with the idea, developing it, and soon—meaning one year later—I found that what I was doing seemed to me to have very broad implications. Now I have put possible applications off to one side, and I am looking strictly at the basic science. at the fundamentals of it. I think I have identified what I regard as a new principle applying to heat engines in a very general sense. I do feel a responsibility ultimately to be able to draw a connection between the basic scientific work I do and some technology.

KOLB: I don't feel that way at all, There is a real necessity for nonmission. For fifteen years people have been looking at magnetic monopoles, intensively, just for pleasure, and for the past five or six years have been studying grand unified gauge theories—same motivation. Recently, Rubakov in Russia and Callan at Princeton have proposed that monopoles can catalyze proton decay, can just completely convert the rest mass of protons into energy. It will be another five years before it's worked out. Now something like that would have a tremendous payoff. would be comparable to Otto Hahn's discovery of fission. But it never could happen in a mission-oriented environment. No one told these people they should study monopole structure because it might have important applications. And no government agency has told me I should be studying them, either.

WHEATLEY: I'm not waiting to be told what I should do, either. For instance, I would feel perfectly tine studying spin-polarized hydrogen, a project in which 1 am very interested. Nor can I tell you what gadget that might be used in, but I do see that it is part of the foundation for thermal physics and that we ought to understand it.

KOLB: I don't choose research projects by wondering if they will have any impact on technology.

BAKER: Aren't you thinking of beam weapons systems using

monopoles?

KOLB: If I think about it, it is only after doing the basic science.

HOWE: Is it necessarily the basic researcher's responsibility to come UP with the utility of it? There are, perhaps. other people who are more interested in the engineering side, so they take the proton-monopole catalysis concept that Rocky mentioned and say. "Well, let's develop starship drives: let's design power reactors!"

SCIENCE: *Rocky, how do you choose your research projects? You 've said how you don 't choose them.*

KOLB: I don't know, actually, I don't know what I am going to be doing tomorrow or when I go back to my office. I read the literature and see what other people are doing. This communication is very important. I follow the direction the work is going.

HYMAN: You may recognize a problem as being important, but in the end the choice is subjective. A question gets under your skin, and you can't let loose until you understand it. That's the driving force behind science—the need to understand. As far as Rocky's responsibility to the Laboratory, that has become clear as he's talked, His obligation is to push back the frontiers of basic science—that's his job description. At the same time every scientist has a responsibility to the overall health of the Lab. Whenever you discover something that could be applied in a programmatic effort, you go down the hall, knock on doors, and make sure the right people know about what you have done.

KOLB: When I first read about Callan and Rubakov's work on monopole-catalyzed proton decay, I was at Aspen, and I said, "Well, I have to get back to Los Alamos and tell people about this." but then Stirling and I decided it couldn't work, so I didn't go knocking on doors.

WHEATLEY: Coming back to the missions of the Laboratory, I understand why we should be doing some basic science and much fundamental technology, that is. research on problems whose ultimate objectives are fully seen. However, my own view concerning applied work and hardware is that if you have a particular, well-defined job to do, the private sector would probably do it better.

HECKER: I would disagree, John. The weapons mission is a specific job, and we have done it very well.

WHEATLEY: The weapons case is rather special because of the national security problem. Suppose that you took the secrecy requirements away.

BAKER: In fact, private industry does secret work, builds all the components. We provide the overall science and technology. I don't think secrecy is the defining factor, The national laboratories are most effective doing both the theory and the design development of jobs that are high risk and from which an industry couldn't expect a profit in a short term. Fusion is another example.

HYMAN: Our exceptional facilities also give us an edge over industry. The two thousand scientists at Los Alamos comprise a pool

Sig Hecker on Materials Science

Materials are the *sine qua non* for new technology. At Los Alamos we have been in the business of processing new materials for technological needs from the very beginning. Now materials processing is becoming more sophisticated as we learn to exploit our understanding of materials on an atomic level. Our work on rapid solidification and ceramic processing exemplifies this trend.

So-called rapid-solidification-rate materials are made by cooling the liquid state very rapidly, on the order of a million degrees per second. The rapid solidification avoids equilibrium decomposition and consequently affords the opportunity to create materials with new and novel structures, For example, if you smash a liquid metal between an anvil and hammer or spin it against a cooled, rotating wheel, you can create a metallic glass, that is, an amorphous metal rather than a metal with the normal polycrystalline structure. Properties of metals depend critically on their crystal structure, or, more specifically, on the defects in the crystal structure. By creating an amorphous metal, we eliminate grain boundaries, which contain many defects and are therefore places where corrosion begins. Consequently, these metallic glasses have good corrosion resistance as well as high strength. Our rapid solidification work at Los Alamos has been applied mostly to processing actinides.

Our work in ceramics processing is aimed at a new class of structural materials for high-temperature environments, such as those involved in fuel processing and power generation. For example, a ceramic turbine might be used to achieve higher operating temperatures and higher efficiencies.

State-of-the-art work is being done in two areas: processing of dense ceramics without densification additives and growth of ceramic whiskers, The ceramics of greatest interest to us, silicon carbide and silicon nitride, must be made at relatively low temperatures to avoid decomposition. A densification additive forms a glass phase between the powder particles and essentially glues the particles together. Unfortunately, during high-temperature service, in a turbine for example, the glue turns glassy and

the ceramic loses strength. To eliminate the need for an additive, we have developed a technique for making an extremely fine, extremely reactive powder that shows great promise of densifying at low temperatures. We form the fine powder particles, which have diameters on the order of hundreds of angstroms, by a plasma-assisted chemical vapor-deposition process. In this process the constituents, such as silicon and carbon, are carried by appropriate gases and are reacted in a hot argon plasma. We are also using the Laboratory's expertise in shock loading to activate ceramic powder containing larger diameter particles. The idea is to produce a large concentration of defects on the surface of the particles before attempting to consolidate them.

Ceramic whiskers, a field in which we are the world leader, are long, single-crystal fibers of, for example silicon carbide or silicon nitride, with diameters that vary from less than a micron to maybe ten microns. These single crystals are grown by a process called the vapor-liquid-solid process. They are essentially defect free and have enormous strengths, from ten to fifty times that of structural steel. We are now trying to incorporate the whiskers into a composite material-a glass matrix, a ceramic matrix, or a glass-ceramic composite—to make high-temperature materials. Essentially, we are using processing science to control the strength and the ductility of materials on a microstructural level.

Another area that is not new, but extremely fascinating, is the actinides. In the last few years a marriage of condensed-matter physics, chemistry, and metallurgy has helped us to understand the intriguing electronic and magnetic properties of these elements and, in particular, how they determine the macroscopic properties of plutonium, uranium, and americium. For plutonium, especially, the only way to understand it is to understand the role of its bonding *f* electrons. For example, because the *f*-electron wave functions possess odd symmetry, bonding of these electrons favors unusual crystal structures with low symmetry. People in academic circles are now becoming very interested in the actinides because they offer new physics. ■

of knowledge found in only a very few places. Also we have five Crays and a complete set of shops.

WHEATLEY: We do have a complete set of shops, but it costs fifty-five dollars an hour to use them.

HYMAN: But they are at our disposal.

COLGATE: Just for a moment let me reduce the main missions and the main capability of this Laboratory to plain terms. Suppose we didn't have a Laboratory. Why would Congress, the politicians, want to start one? The only reason would be because they were scared: scared of losing the country—that's our national security mission—or scared of losing our way of life and our power—that's the energy mission. Fear for the future motivates the existence of this Laboratory. Politicians would never fund science from purely altruistic motives, and purely educational business would be in the universities where it belongs. But how do you make sure that a new idea doesn't come up to bite you from the rear, as Sputnik did? You have the most brilliant people around to think up all the new ideas that are possible before someone else thinks of them. So the basic capability of this Laboratory is its brilliant individual scientists. If someone wants you to come to the Laboratory, why do you accept? Because people here are doing the most exciting research in your field, and because you believe in your own ability,

ROCKWOOD: There's something I worry about, and I'd like to mention it here. At moments of international crisis, programs for the national laboratories are easily defined. But during periods of uncertainty about the future, and especially during periods of economic stress, the selection of programs is not so simply made. One of the strengths of Lo's Alamos internally is its great freedom of thought—freedom to disagree, to discuss openly with management the pros and cons of particular technical endeavors. It makes us stronger to have had these discussions and to look at all sides of a problem before going into it. But we should speak with only one voice to the external world. We don't need two, three, half a dozen people showing up in the same office in Washington, each with a different opinion as to which major programs the Laboratory should be pursuing.

SCIENCE: *While you more or less agree that the development of high technology for national security is the Los Alamos mission, the specific emphases and manner of carrying it out remain open to discussion. Perhaps we should turn now to some of the specific areas of research and development that are clearly important. Carson Mark has commented that many of the problems in technology development are materials problems. Sig, would you tell us what is being done at Los A lames in this area?*

HECKER: Our materials science effort demonstrates the exciting and productive relationship that exists between theory and experiment. It is one of the beauties of this Laboratory that metallurgists, physicists, and chemists work side by side. Our main interest in materials

processing, without question, has always come from the weapons program. Weapons designers, be they physicists or engineers, come to us with requests that to them seem exceedingly simple and to us almost impossible, at least at first glance. For example, the physicists wouldn't hestitate to ask us for structural air, that is, something with no density but enormous strength. Faced with sophisticated problems for years and years, we've learned how to tailor-make many special materials.

We have also done some basic research in materials science, and in the past few years we have begun to apply our understanding of materials on an atomic level to materials processing. One example is rapid-solidification-rate technology to make amorphous metals with high strength and good corrosion resistance, Another is ceramics processing; we are attempting to make materials for high-temperature environments, such as composites containing single-crystal ceramic whiskers.

LANDT: Electronics is another field that combines ideas and applications; it's partly software and partly hardware, and it's a crucial part of future technologies. I would like to put before you a statement by Dr. DeLauer, Undersecretary for the Department of Defense. Dr. DeLauer insists that electronics is the most critical of all technologies for the maintenance of peace, and he claims that "Further development of the electronics technology base of the United States is as important to defense today as the atomic bomb in World War II."* I think it's time the Laboratory took its electronics seriously.

BAKER: There are, however, a lot of good electronics firms.

LANDT: We are working on several projects that could make significant contributions in electronics—areas that private industry is not touching. These include high-speed electro-optic switches and thermionic integrated circuits that have important military as well as commerical potential. We are also developing high-power microwaves from lasers. This is research that could not be done without the exceptional computer and experimental facilities at Los Alamos.

SCIENCE: *Since we have mentioned speaking freely, I'd like to ask Steven whether there's anything he can tell us about weapons design work.*

HOWE: Most of what we do is classified, but I can say that we work to get better codes, better computational abilities to describe the processes in the weapon, to put in the things we do know so that the things we have to extrapolate can be better estimated. In the year I have been here we have come up with several interesting pursuits. One is in low-energy nuclear physics: there is a process that we think exists in the weapon but that we don't account for in the codes. This particular development is interesting because we have shared it with Livermore, and we have collaborated with them in getting it into the codes and making estimates. We also do secondary design work on weapons materials, attempting to understand basic processes. Generally we aim to satisfy the military requests and to come up with smaller, more efficient devices. We are continually looking at new

Jeremy Landt on Electronics

Our heavy reliance on the world of electronics has led Los Alamos into several fledgling projects that show great promise for the future. One is the development of the high-speed electro-optic switch, which can be used to probe integrated circuits with pulse widths of 50 picosecond or less. Understanding of semiconductor physics on these short time frames is essential for development of reliable, very high-speed integrated circuits for future weapons systems. The first generation of very high-speed integrated circuits is largely based on extrapolations of existing technology. To go beyond will require new technologies and understanding that industry does not have at present.

Another device under development is the thermionic integrated circuit, which is inherently hardened to radiation and EMP phenomena. Before research on this device began at Los Alamos an attempt to commercialize the technology failed because the basic physics was not understood. We could use this device to instrument nuclear and geothermal systems, as well as in military applications.

The area I find most exciting, however, is the broad area of high-power microwaves. We are working on novel generation mechanisms as well as novel applications. One new generation scheme involves the Helios laser, the "Laboratory's high-power carbon dioxide laser. Large numbers of hot electrons are generated in high-power laser targets. A carbon dioxide laser produces far more hot electrons than do lasers operating at shorter wavelengths. We are presently investigating ways of converting these electrons to high-power microwaves. The power levels achieved to date are very impressive and probably can be emproved much more. At present this research cannot be done anywhere else in the world. Los Alamos has both the computer codes to handle the flow of particles in electrornagnetic fields and the experimental facilities to benchmark the codes. ■

"Richard D. DeLauer, "The Force Multiplier, " IEEE Spectrum, *October 1982, p. 37.*

Brian Crawford on Life Sciences

Several exciting things are happening in life sciences. We are using laser-based flow cytometric methods to separate chromosomes from mammalian including human, genomes. DNA from these isolated chromosomes can be cloned by recombinant DNA methods, allowing studies of the basic structure and functional organization of the chromosome. Los Alamos is one of perhaps three labs with the requisite expertise in biophysics and molecular biology to perform this work, and recent NIH [National Institutes of Health] funding to establish a Flow Cytometry National Resource is fostering progress in this area.

We are also working on cellular oncogenes. These genes are thought to control the evolution of the normal cell toward malignant change. The isolation, that is, the cloning, of such genes by recombinant DNA methods and the reinsertion of these genes into normal cells, by a process known as DNA-mediated gene transfer, permit us to study how specific oncogene expression can result in cancerous change. We are also studying the role that gene rearrangement, which can result for example from chromosome damage, can play in the initiation and progression of cancer. This work relates to DOE concerns regarding the effects of both ionizing radiation and the by-products of fossil-fuel development and consumption.

Another exciting development is the establishment of an NIH-funded DNA sequence database in the Theoretical Division. Sequencing, or decoding, of the genetic code in cloned fragments of DNA is meaningful only if such information can be stored, retrieved readily, and analyzed. Just consider for a moment that each mammalian organism expresses on the order of fifteen thousand distinct genes in a cell—not to mention that each cell has DNA encoding for an amount of unexpressed information hat is several orders of magnitude greater. Software development for the analysis of the stored sequences will be pursued oncomitantly with this Herculean bookkeeping effort. ■

things and attempting to improve the codes both in X Division where we do theoretical weapons design and in T [Theoretical] Division. We do interesting work. and I find it kind of sad that we can't tell everybody about it. Clearly we could do better if we could talk to people.

BAKER: Do you find it difficult to get rewards from your work because you can't talk with more people about what you do, can't publish results?

HOWE: In some sense your ideas are rewards in themselves. If they work, you know you have made a gain, perhaps even contributed to unclassified scientific efforts like inertial confinement fusion, which is also being studied in our division.

SCIENCE: *Is it difficult to pick up information you need because your problems are classified?*

HYMAN: I really think it is. It is frustrating on all sides not to be able to express an interesting scientific question in the context where it arises. You notice the difference at national physics meetings between the typical scientist and those working only on classified problems, The ones working on unclassified problems can go to the blackboard and describe everything in minute detail, get immediate feedback, and also know that people will go home and continue thinking about the problem. When people first come to X and T Divisions. they continue to go to physics conventions as they did before. But if they work only on classified problems, often within the first few years their attendance drops off very fast. Some just stop attending national meetings and interacting with the outside world.

At the Center for Nonlinear Studies [CNLS] we are trying to encourage interactions between the classified and unclassified research areas by organizing mixed workshops. In these workshops the first two or three days are unclassified and uncleared university scientists are encouraged to attend and speak. On the last day classified questions related to national security are addressed, and the attendance is limited. The last such conference was a joint X-Division/CNLS workshop in February on interface instabilities.

A problem we have not been able to overcome is that numerical results generated by a classified code are classified-even when the physics model, the data tables, and the numerics used in the run are unclassified. This restriction greatly inhibits interactions with computational physicists outside the Laboratory.

SCIENCE: *How open is the communication between T and X divisions ?*

HOWE: We rely heavily on our communication with T Division people.

HYMAN: Mostly it's between people you've worked with for years or know from the coffee machine. And the interchange is more limited now that the two divisions have been physically separated. We are trying to get more joint seminars so that we can indeed hear what people doing unclassified research learn in the outside world and then relate it to our needs.

HECKER: It is a poor substitute to have to depend on T Division for your information.

HOWE: It doesn't really work.

SCIENCE: *Is an effort being made to change the situation?*

HYMAN: Yes, there's been a change in the attitude of management.

In T Division we've always been very strongly encouraged to publish at least one paper, if not more, a year and to present at least one, if not more, at a national meeting. Some of the same emphasis is now appearing in X Division.

HOWE: We are getting more new people straight out of universities, and I think those who are new are interested in the national meetings. Getting back to our relationship with T Division, I would like to see us, as designers, integrate better with the work in T Division. For example, we really don't have a well-defined effort to do nuclear physics type research in the weapons physics business. We do our job for the military. They say, "We want this beast," and so we take what the codes can give us, and we design the creature. The T Division staff doesn't have this limitation and their work in nuclear physics is relevant to what we do in weapons.

BAKER: I know that some people in X Division work enthusiastically with the space groups. They have a number of large computer codes that they like to test on a variety of systems to see just how well the codes predict behavior. The magnetosphere is a large plasma system with magnetic fields; they like to try to model that. We do such modeling, too, and like to compare the results of our different codes.

SCIENCE: *I want to ask you about the young people in the weapons program. Are they there because the problems are interesting or because they have some feeling of commitment to the development of new weapons?*

HOWE: Many of them are in there because they did their theses in areas used in weapons research. Weapons development is such a multidisciplinary field; everything in the world is involved in making this thing go. Chemistry, physics, nuclear engineering, hydrodynamics—almost any field you name is involved. I would say people's motivations vary.

HYMAN: Many people have come into the weapons field because at one time they recognized that controlling fusion is one of the most important unsolved physics problems of the century. Much of the knowledge and data needed to crack the controlled fusion problem is classified. Once in the system, people find the weapons-related problems equally or even more fascinating and rewarding.

HECKER: Because of the strictures of classification, people rarely choose to come to the Laboratory to do materials research for the weapons program. People come here to do research in other areas and then wind up working on weapons problems because they are so interesting. We do have a corps of extremely dedicated people who build prototype hardware, develop our local shots, and design Nevada Test Site shots. But the tight ring of security really stops the flow of ideas from the outside in. Our metallurgists working on plutonium have been so strictly limited that we have tried to give them a cross section of other work, but an enormous amount of materials expertise remains outside the reach of the Laboratory.

SCIENCE: *A new Center for Materials Science has been created at the Laboratory, as well as the Center for Nonlinear Studies and a branch of the Institute for Geophysics and Planetary Physics. Are these centers aimed at alleviating the communications problem?*

HECKER: Yes. Don Kerr has recognized the overall problem. The Center for Materials Science has brought us in close contact with first-class materials science people outside the Laboratory.

HYMAN: The Center for Nonlinear Studies has had a similar impact. We sponsored over three hundred visitors last year. Besides the week-long conference each year, we have a number of workshops in areas we've chosen to target. One target this year is understanding the creation, stability, and evolution of patterns, fronts, and interfaces. There will also be workshops on cellular automata, implicit methods of differential equations, fracture mechanics, science underground, synthetic metals, and biopolymers. And what is even better than solving immediate problems is bringing together from the Laboratory, industry, and universities people on a one-to-one basis—establishing relationships that can continue for many, many years.

BAKER: In contrast, the Institute for Geophysics and Planetary Physics is directed toward interactions with professors and their students. We are a resource of the University of California in particular, and we now have a number of their graduate students working here for a year or two.

KOLB: This type of interaction not only helps us; it brings in people who then discover what is going on in the Laboratory. Half the people taking part in this discussion had their first contact with the Laboratory either as graduate students or as postdocs. Both the graduate student program and the postdoc program are really excellent ways for the Laboratory to recruit good people. I strongly believe it would be to the long-term benefit of the Laboratory to enlarge these programs and the visitor program as well.

ROCKWOOD: We should also work closely with the universities to make both students and faculty aware of the directions in applied science and the particular types of people that we see we are going to need. We can give universities access to such facilities as LAMPF, Antares, and Helios as research laboratories for their students; in return they may become more familiar with this Laboratory and be more responsive to our future needs.

HYMAN: In line with this thinking I should point out that the Graduate Research Assistant program is probably the most effective and least expensive of all of our advertising. But it's under-utilized, and I'd like to see it used more.

CRAWFORD: The closer our contact with graduate students, the better off we are, I think. It's a way of advertising the incredible potential and diversity of this place—some of it realized and some still untouched. It's difficult to overstate the importance of the Laboratory's diverse capabilities. I think there's a real need to keep

Laboratory Support for Basic Research

The Laboratory has always recognized the need to support a wide variety of basic research, and for most of the Laboratory's history, that research was funded entirely by the weapons program. During the 1970s, however, budgetary constraints made it increasingly difficult to maintain the level of so-called Weapons supporting Research, and in 1975 concern about its steady decrease prompted Harold Agnew to found the New Research Initiatives program as a supplement. However, despite the Laboratory's growth and widened spectrum of activities, Weapons supporting Research funds continued to be t.he dominant means of Laboratory support for basic research.

In fiscal year 1982 Donald Kerr combined and expanded the Weapons Supporting Research and New Research Initiatives programs with establishment of the Institutional supporting Research and Development program. This new program incorporated the following principles, many of which required new and extensive plans on the part of everyone involved.

- The program should be Laboratory wide and should include a broad spectrum of research and development related to all Laboratory programs,
- Projects should be consistent with and in support of the Laboratory's basic missions.
- Funds should be distributed according to a fair scheme that encourages corn-petitive proposals and ensures optimum investment of resources.
- Support should be derived proportionately from all Laboratory programs,
- Accountability of funds should be reasonable and consistent with normal practice.

The ISRD program has definitely improved the manner in which discretionary research funds are allocated and the status of funded projects is reviewed, Considerable freedom is exercised by the Laboratory's associate directors in organizing and evaluating projects under their directorates. As is usual with any new program, some shortcomings have been recognized and some evolution is expected. It is evident that, in spite of the healthy challenge of submitting competitive proposals, there have been too many proposals and they have, for the most part, been too long. Paperwork is being reduced, arid a system of triennial, rather than annual, review is being developed for some projects.

The accompanying table lists the distribution of ISRD funds among various broad categories in fiscal year 1982.

DISTRIBUTION OF ISRD FUNDS IN FISCAL YEAR 1982

Research Category	Allocated Percentage of Total Funds
Materials Science and Chemistry	32%
Program Development and Applied Technology (Energy and Defense)	25%
Mathematics, Techniques, and Computer Modeling	13%
Nuclear Physics and Nuclear Chemistry	1 l%
Medium and High-Energy Physics	8%
Plasma Physics and Astrophysics	4%
Earth and Space Sciences	4%
Life Sciences	3%

not just students, but the whole country, informed about what we're doing and can do. One important example in life science research is the new DNA sequence data base being established in the Theoretical Division and funded by the National Institutes of Health. This will be a comprehensive computer-based library of DNA sequences designed specifically as a resource for scientists around the world who are doing recombinant DNA research. Eventually we may be able to produce a computer-based, electronic journal that bypasses conventional publication. Scientists could submit their DNA sequence data for review and receive results in recombinant DNA research electronically.

SCIENCE: *How do new projects such as the DNA sequence library get started?*

HOWE: First someone has to have an idea and that usually happens quite informally. We sit around and talk and suddenly some guy comes up with a neat idea.

COLGATE: 'That's right. Some of us don't know one another very intimately. but sooner or later we will meet, I will bump into John and start talking about cryogenic systems for fractional charge separation using superfluid liquid helium as a charge separation drift chamber.

ROCKWOOD: Once the idea is hatched, you might try it out with what is called bootlegging. You do the experiment or the calculations at your own discretion, but generally with the knowledge of the group leader, division leader, or whoever else is involved. If the idea shows real promise you may be funded through Institutional Supporting Research and Development [ISRD] money. This is the Laboratory's discretionary fund, It has traditionally been used for basic research,

but more recently it has also been used to fund new applied programs, I, for one, believe the applied programs should receive an equal share of this money. This is our investment in the programs of the future, and, in the final analysis, only programs pay the Laboratory's bills.
HECKER: The fact that this Laboratory has the foresight to take a meaningful fraction of its total income and plow it back as discretionary research is fantastic. At many other places the discretionary research money is more like one per cent. We do have an enormous opportunity for internal research. Of course, there has been a lot of upheaval recently about having to write proposals every year for ISRD money.
COLGATE: I think proposals are a darn good idea. I never did have to do them at Livermore. Then at the university I ended up having to write twelve a year. They are never easy, but they are really worth it.
BAKER: They do help people who didn't know what they were doing to think about their work a little more, but on the other side of that coin I think management can really be an obstacle.
COLGATE: Yes, if proposals are not reviewed correctly, you end up with a mess. Most proposals are now judged by the Laboratory management and the Senior Fellows, but this does not always constitute peer review.
HECKER: I agree that we do need more accountability than we used to have. However, one simply cannot set up an environment to do good basic research if proposals are required on a yearly basis. Also, the people making the decisions have become farther and farther away from the people who really know what is going on. I'd like the authority and the responsibility for research programs to rest with the divisions. By all means have an advisory panel of outside peer experts to judge the quality of the research, and if the results aren't good, then fire the division management.
BAKER: I've found that the handing out of Institutional Supporting Research money is based too much on historical factors rather than on quality of research. There is no competition in the true sense, that is, based on demonstrated scientific competence.
HECKER: That problem has been addressed to some extent. Two years ago six working groups were set up to look at areas that were not well represented traditionally, and I know that materials science has been receiving more support recently.
COLGATE: Perhaps the ultimate mechanism is, once again, the individuals. To my mind the Lab is put together of people who have an absurd sense of ego; that is, they have the drive and the motivation to back their own original ideas.
HYMAN: It's true that most projects have started with individuals who were aware that something was about ready to break. They went out and wrote proposals; they got up on their soapboxes; they sold their ideas and started small. Sometimes the ideas fizzled out, but other times they turned into whole divisions.

Rocky Kolb on Cosmology

"Cosmos" is the Greek word meaning order, and the basic goal of cosmology is to understand the universe on the basis of physical law. By applying physics to what we see in the universe, we endeavor to understand the structure of galaxies and the origin and large-scale structure of the universe.

Within the past five years or so some very interesting and very bold particle physics theories have been hypothesized. They model the physics of incredibly small scales-down to Planck's scale, which is about 10^{-33} centimeter. These theories are extrapolations, but there is some physical basis to them and they imply certain things about the universe. For example, they predict proton decay and the existence of magnetic monopoles. If these predictions are correct, then we now have models of the structure of matter under unbelievably extreme conditions of density and temperature, and we are in a position to study the very, very early universe. By the early universe we used to mean 1 minute or 1 second after the big bang. Now we can talk about 10^{-33} or 10^{-38} or 10^{-40} second because we believe we have a model of the underlying physics with which to do the astrophysics and cosmology.

Some practical questions we might answer are how many magnetic monopoles are expected to be around, what are their properties, and how would one look for them. Another possible insight is understanding the asymmetry of the universe in baryons-that is, why there aren't an equal number of baryons and antibaryons. Unfortunately the big bang is not an experiment that you would want to-or could-duplicate.

Study of the early universe leaves an interesting unanswered question: why the universe is so old, If you look at the Einstein equations that describe the evolution of the universe, the only

BAKER: Jerry, what reception do you find to suggestions being made by the Weapons Advanced Concepts people?
LANDT: Very good in general, but there are some people who resist change and don't like to see things at the Laboratory change.
HOWE: I find in the weapons program that you can have a wonderful idea either in software or in hardware, and, fine, they will help you develop it and make the best calculations possible. But then they fail to implement it. Furthermore, we are being urged to develop our own codes rather than just to borrow from Livermore. And in fact we do have several new ones, but I find there is some resistance to changing several hundred thousand lines of a code and putting in the new stuff. The same kind of reluctance appears in the hardware; it takes several years to get a materials idea implemented.
KOLB: Is that a management problem?

time scale that appears is the Planck time, which is about 10^{-45} second. It is rather hard to understand why today, ten billion years, or 10^{60} Planck times, after the big bang, the universe hasn't either recollapsed or expanded to an extent that the gravitational attraction of the matter is irrelevant in the expansion. Today we cannot determine whether the universe will expand forever or eventually recontract, since the kinetic energy of expansion is almost equal and opposite to the gravitational potential energy. This seems to imply that in the initial expansion the kinetic energy balanced the gravitational energy to something like one part in 1056—essentially a zero-energy system. This conundrum has a possible explanation if the universe underwent a strong first-order phase transition. An active field now is phase transitions in the early universe. This is a true interdisciplinary field, bringing in particle physics, general relativity, and statistical mechanics,

Our investigations may also have a number of reciprocal implications for particle physics. It has become fashionable every time a particle physics model is proposed to look for the astrophysical impact of it, You try to see whether the new model does things to the universe that you can't allow. For example, does it lead to too much mass density in the universe? Another example is monopole-catalyzed proton decay, Colgate and I have pointed out that such decay would have a terrible environmental impact on neutron stars. The work we have done leads us to believe that either monopoles do not catalyze proton decay or that monopoles don't exist, which would really be a shame because their existence would have enormous practical implications. ■

HYMAN: It is somewhat a management problem in that the codes have been allowed to grow unstructured for so many years that they have become the unmanageable things they are.

HOWE: It may be an external problem—one caused by whoever is using the weapons.

CRAWFORD: The external response to new ideas probably varies greatly from agency to agency. The Office of Health and Environmental Research, which oversees much of the research in the Life Sciences Division, is quite receptive to new programs.

COLGATE: Other offices of the DOE are also receptive. For example, Rocky has had ISRD support for some time doing far-out research in cosmology relating to conditions in the early universe. But what's really relevant is that last year the Office of High Energy and Nuclear Physics saw tit to pick up part of his funding. Nothing ventured, nothing gained!

SCIENCE: *With regard to external support for new ideas, the Laboratory is encouraging more interactions with industry. How will this affect the Laboratory?*

ROCKWOOD: I would say that a closer union of this Lab and industry would be mutually beneficial. The best single thing that has happened is that the DOE may now allow patent rights to remain with a funding company. Private industry can now put some money into a national lab without losing all rights to patents that emerge from the work. For instance, an industrial organization that wants to get involved in a new venture requiring a group of plasma physicists wouldn't have to hire twenty of their own while they got started. Instead, they could hire our expertise in that area to help them get started—a healthy collaboration.

WHEATLEY: I really think that is right.

ROCKWOOD: I see us starting to make some progress. We have money coming from Westinghouse to help look for a method of enriching certain isotopes that they are interested in as a company. They would have refused to invest this money in us a year ago,

BAKER: The hot dry rock project is a related example. Money is coming from a variety of sources, such as the Japanese government and the German government, as well as our own government.

SCIENCE: *We hire the people and they fund them?*

ROCKWOOD: They hire our people, if you will. They contract to us to do a specific task that saves industry from building up a highly specialized group of people they don't need for the long term.

HYMAN: The kind of basic research a lot of us do is oriented toward the very large problem with very limited applications. Take the supercomputers. There just aren't that many supercomputers out there. Most vendors can't afford to support the effort needed to develop new algorithms and software that push these computers to their limits. Yet it is quite appropriate for us to do that here.

HOWE: I can forsee that industry funding might compete with basic research for a person's time. Since it is near-term support, you are going to have managers saying, "AH right, we want you guys to work on this project for Westinghouse, and you have to put aside your basic research for now."

ROCKWOOD: I think rather that industry will be wanting to use basic research that we have already completed. But I won't say that conflicts will never arise. They'll have to be worked out.

CRAWFORD: If we become closely allied with both universities and private industry, perhaps we will be able to function more as a research and development organization—taking ideas from university programs and assigning teams of researchers well qualified to test the feasibility of such ideas—with the goal of technology transfer to private industry.

SCIENCE: *Gentlemen, it seems that our relationship with industry may undergo a change. What other changes would you like to see*

happen in the future? I know I'd like to hear about the proposal for an underground laboratory.

KOLB: Los Alamos has a proposal to build such a laboratory at the Nevada Test Site. It would be operated as a user facility, like LAMPF, and would make possible an entire class of very sensitive elementary particle experiments that require shielding from the normal above-ground radiation levels.

Los Alamos is a good laboratory for this facility because, first of all, we have strong groups in theoretical particle physics and in astrophysics. The interdisciplinary work of the facility would require a broad base in many areas of physics. We would aim to learn about neutrino oscillation and determine neutrino masses, topics that would have a large impact on our understanding of galaxy formation. We would have a chance to detect proton decay, which would go a long way toward telling us how much we understand about the origin of baryon symmetry. We could also learn many things about cosmic-ray physics and the large-scale structure of the universe. And a facility like that would generate technology in building detectors and in doing state-of-the-art experiments.

HOWE: I would like to see us expand in the space utilization business. We have a great deal of expertise in basic physics research and materials sciences, but we don't have much of a program for utilizing space.

HECKER: At the expense of the Jet Propulsion Laboratory?

HOWE: JPL is mostly involved in planetary exploration, and NASA is doing hardware development. Perhaps Los Alamos should begin programs to utilize the shuttle, to utilize the space station if it gets built.

BAKER: Those things are being considered, but so far the effort is fragmented.

WHEATLEY: There currently is an interesting cooperative program between the Center for Nonlinear Studies and the Center for Materials Sciences, having to do with conductive polymers. Wouldn't it be good to have such a program between the Institute for Geophysics and Planetary Physics and the Center for Materials Science on materials processing problems for space? We talked with a fellow from NASA who is in charge of their program for materials processing in space. That is really interesting physics—and chemistry and metallurgy and what you would call materials science.

HOWE: That is an important point. Probably the Weapons Advanced Concepts people are looking at orbital devices, but if someone comes up with an idea for an experiment to go on the shuttle, we have no one in the Laboratory who could translate the idea into shuttle-compatible hardware, as far as I know. NASA would have to be contacted. There is no given laboratory in the country to interface with industry and provide shuttle compatibility.

CRAWFORD: I'd like to see the Materials Science and Technology and the Electronics divisions combine research in their areas with the space program to develop alloys, circuits, etc. in space stations. It would be an ideal opportunity for cooperation with the private sector, and it could foster the rebirth of the space programs. It could place us at the forefront of university-industry cooperation with national laboratories.

HOWE: I'd also like to see us involved in the defense angle. The military consults with the Laboratory on a lot of concepts now, and we should have the capability of consulting in the area of space utilization.

LANDT: Interchange takes place along a number of avenues, but there are no hard and fast rules.

BAKER: We clearly have many of our eggs in the space basket for communication and for intelligence gathering, and our reliance on space is likely to grow. It is certainly something the Laboratory is interested in.

HOWE: The Air Force recently created the Space Technology Center in Albuquerque. We could have a good interaction with that phase of the military, and it would be an ideal way for the Laboratory to get involved in the space program.

SCIENCE: ***Are there any other similar areas? How about computer science in terms of the future?***

HYMAN: The way that the inside of a computer works is going to change completely in the next few years, and unless we rethink how to write programs, we won't fully exploit the potential power of the new machines. Some people saw this years ago and asked that we prepare new algorithms ***before*** the machines arrived. Slowly the proposals went through the Laboratory and through Washington. Now, finally, we have a viable research group in the Computing Division developing new methods for machines not yet built.

There are two similar computer projects still at the proposal stage that come to mind. The first is a CAD/CAM [computer-aided design/computer-aided manufacturing] effort to model three-dimensional surfaces on the computer with a very interactive user interface. The second project is in artificial intelligence and would have many applications within the Laboratory, from providing a reliable friendly user interface for our complex computer network to applications in nuclear safeguards.

The proposal to form an artificial intelligence group at Los Alamos surfaced about a year ago, and by now it is well polished and dog-eared at the corners. A group of about thirty of our scientists meet regularly and sponsor classes and talks from visiting and Laboratory experts.

Just how speculative do you want me to be about future scientific computing?

SCIENCE: *Go ahead, speculate.*

HYMAN: All the major physics codes at this Laboratory have many similar components. At the lowest level, they use trigonometric functions—sines, cosines, and tangents. In the early days of computing, everyone had his own favorite procedure for these elementary functions, but gradually the better ones were included in the mathematics program library. In the ’60s and ’70s higher level routines for solving linear systems of equations, integrating ordinary differential equations, handling one-dimensional interpolation, and other moderately complicated procedures were developed and included in the computer library. But then in the late ’70s the trend slowed down and in some cases stopped. Right now we have no appreciable effort developing the next generation of mathematics support software. If such a group existed, it would be writing even higher level routines: multidimensional interpolation and differentiation programs, grid generation and adaptive mesh routines that adjust the solution algorithm to the boundary of the problem and the structure of the solution, routines to help solve large systems of sparse nonlinear equations, and routines to incorporate the boundary conditions into a discrete approximation of the physics model,

For this new software to be successful, it must be compatible with existing techniques and be simple enough that in a trial run potential users can observe tangibly better results than with existing methods. The software packages that are most readily accepted are those that behave like the existing ones—only work better.

Industries and most universities that develop new software are too far removed from the production code programmers to interact with them and obtain the essential feedback. Also, the production codes are run on the most powerful computers available and those writing the software must have access to these machines. This means that we at the national computing centers should be writing the next generation of high-level mathematics support routines to be used in our production codes. At the same time we really should be getting together more with the scientists in industry and universities who are writing mathematics software. This means having a much more active visitor program in math software development and providing easy, long-distance access to our supercomputers.

CRAWFORD: I agree that we should forge ahead in our computer work, both the hardware and the software. Our national security will depend partly on our ability to lead the supercomputer field.

HYMAN: We need a coordinated effort like Japan’s. Japan already dominates in applying robotics in industry. Through its Ministry of International Trade and Industry, it has identified other projects it plans to complete by 1990. One project is a high-speed computer whose capability is at least ten times that of the Cray-1. Another is a fifth-generation computer that will implement artificial intelligence—the number of inferences per second would be a hundred to a thousand times current technology. Losing our technological edge in these areas would have serious repercussions on both our economic and our national security.

CRAWFORD: I would like to insert another note of warning. Recombinant DNA techniques are ridiculously simple to master. The United States could suffer from foreign nations or even terrorist groups employing biological or chemical weapons. Our Laboratory is an ideal place—we have both physical isolation and classified research ability—to establish a defense program against such agents. Biological and chemical agents can and will be used by those with a cause, however ill conceived. Countermeasures like specific antitoxins are within reach of our present capability. The nation should move forward in preparing these defenses.

LANDT: To close this discussion, I would like to spend a minute or two talking about future defense. Historically this Lab has developed the nuclear side, but now we should try to get people to think about the other side, the nonnuclear. There is an antinuclear movement in this country and the world. Advances in electronics are going to permit some conventional munitions to have the same military impact as nuclear ones, and we should take advantage of that. These are some of the things the Weapons Advanced Concepts people are thinking about.

ROCKWOOD: I also believe the Laboratory should be expanding into nonnuclear weapons for defense. It appears that the nuclear age has, if you will, made the world “safe” for conventional warfare. Conflicts such as the kind in Vietnam, the Falkland Islands, and the Middle East seem those most likely to occur, and the ever-increasing role of high-technology weapons in those conflicts is a matter of which we must be cognizant. We are a nation that aspires to defend itself not by massive uses of people, but as much as possible by the use of high technology—and that means us here at Los Alamos. ■

The Participants

DAN BAKER: I got my Ph.D. at the University of Iowa with Jim Van Allen in 1974 and then went to Caltech as a Research Fellow in the Physics Division, While there I collaborated over a period of a couple of years with people from Los Alamos. In 1977 I came to Los Alamos for a job interview and was impressed with the interests and abilities of the people I encountered. I decided to join a group in the Physics Division involved in high-altitude physics, where I then worked for two or three years on satellite instrumentation and data interpretation. Since October of ‘81 I’ve been Leader of the Space Plasma Physics Group in the Earth and Space Sciences Division, which, I might add, is better known simply as Heaven and Earth Division.

STIRLING COLGATE: I came to Los Alamos primarily because the then Director of the Laboratory, Harold Agnew, and the then Leader of the Theoretical Division, Peter Carruthers, persuaded me to come. I had been a staff physicist at Lawrence Livermore Laboratory for twelve years and then President of New Mexico Tech for ten years. I realized that the type of research I knew best would utilize the facilities of a major national laboratory. My work in inertial fusion continues, and the ability to do astrophysics, atmospheric research, and tectonic engineering in an environment where my advice is respected and my research work is encouraged is a privilege beyond measure. In addition, becoming recognized as a theoretical physicist after initially being an engineer in the Merchant Marine and then being an experimental physicist for many years is a very great privilege, indeed. Explosions turn me on—from firecrackers to testing nuclear bombs at Eniwetok, from using the Lab's codes to calculate supernova explosions to preventing volcanic ones. Our universe started with an explosion, is tilled with explosions, and by far the most extraordinary and singular one is the explosion of intelligent life.

BRIAN CRAWFORD: I was actively recruited by the Laboratory while I was completing work for my Ph.D. at Johns Hopkins University. The Genetics Group of the Life Sciences Division needed someone to investigate the basic mechanisms by which ionizing radiation, chemicals, or other agents cause gene mutation and/or malignant transformation in cells. I had the specific skills required because my thesis had involved study of the genetic mechanisms of chemical carcinogenesis. I was encouraged to apply for one of the Laboratory's Oppenheimer Fellowships, which I received in time to begin work in the summer of 1981. Since I came, I have been applying recombinant DNA methods to research on the genetic events underlying carcinogenesis. What attracts me to this Lab are its advanced facilities and, above all, its cooperative atmosphere—theoreticians are working closely with biophysicists and biochemists in very sophisticated studies.

SIG HECKER: I grew up in Austria but moved to Cleveland when I was thirteen. Indeed, I had never been west of Toledo until I came here as a summer graduate student in 1965. My visit was brought about by a gentleman from the Laboratory's recruiting office who showed me a brochure containing lovely photos of New Mexico mountains. Once here I liked the marriage of basic science and applied technology at the Laboratory. After receiving my Ph.D. from Case Institute of Technology, now Case Western Reserve University, I returned to Los Alamos as a postdoc in 1968, attracted by the excellent funding and the chance to do basic research in metal deformation. In 1973 I came as a staff member after three years in the Physics Department of General Motors. I've worked ever since in materials science, principally in plutonium metallurgy and in actinides, although I've worked on a number of projects related to the space power and basic energy programs. Two years ago I joined the Division Office of what is now the Materials Science and Technology Division.

STEVEN HOWE: I'm another of those students who keep turning up. I started coming here as a summer student in 1975 and did that for the next two years. Then in January '78 I came to do my thesis research at the Weapons Neutron Research Facility at LAMPF. After receiving my degree from Kansas State University, I spent a year at Kernforschung Zentrum in Karlsruhe and then returned as a staff member in September '81. I'm in the Thermonuclear Applications Group in the Applied Theoretical Physics Division.

JAMES (MAC) HYMAN: I was indirectly introduced to Livermore and Los Alamos at the same time. I was interviewed for my graduate fellowship, a Hertz Fellowship, by someone from Livermore, and he asked, "What are you doing this summer?" I worked that summer at Livermore, and it was the first time I saw mathematicians and physicists working in close coordination with experimentalists. It was just great—except the temperature was 115 degrees. My boss at Livermore had been here during the war, and he said, "Where you really want to go next is Los Alamos." So I did, and it evolved into a full-time job after I got my degree from the Courant Institute. I work on numerical methods and software for large systems of differential equations, equations that model the physics experiments. It's partly physics, partly computer science, and mostly mathematics.

EDWARD (ROCKY) KOLB: I received my Ph.D. at the University of Texas in '78. I interviewed here for a postdoc position, but I went to Caltech instead. Then I came here as an Oppenheimer Fellow rather than going to a university, because here I could spend 100 per cent of my time doing research rather than teaching and sitting on committees. I was attracted by the people I would have a chance to work with. It was really the people who brought me here. I did my Ph.D. in elementary particle theory, and now I'm into cosmology and astrophysics, high-energy astrophysics. I'm in the Theoretical Astrophysics Group and I work closely with the Elementary Particles and Field Theory Group, an overlap that's possible here for someone not in a traditional discipline. At universities people seem more locked into compartments: there's one person in nuclear physics, one person in atomic physics, and so forth, and it's not easy to move into new fields. Here at Los Alamos you can move quickly into exciting fields as they open up.

JEREMY LANDT: The country in the western part of my home state of South Dakota is very much like the country here, so perhaps that was a factor in my initial attraction to Los Alamos. I came here in 1967 as a summer graduate student and liked the facilities and the people. When I completed my research work at Stanford, there weren't too many jobs available at Los Alamos in the areas I had studied—radiopropagation, electromagnetic theory, and that kind of thing. But there were at Livermore, so I spent a few very enjoyable

years there. But I got tired of all the people and the hassle, and when something opened up here, I applied and came back in 1975. Except for the past year, my stint here has been spent in the Electronics Division. I have worked on electronic identification systems, EMP calculations, application of radar and other electronic techniques to mapping underground fractures for the hot dry rock project, plus a little nuclear magnetic resonance work, so I have dabbled in this and that. At present I'm working in the Weapons Advanced Concepts Program Office. We're supposed to be looking at wonderful new things; we're finding lots of wonderful old things that other people have thought of.

STEVE ROCKWOOD: After finishing my doctorate at Caltech in 1969, I went into the Air Force as my obligation to the country during the Vietnam era and spent two years at the Air Force Weapons Lab. There I got into laser activities, a field entirely different from my graduate work. I came to Los Alamos in 1972 principally because the laser programs then being started at the Laboratory and the people here were stimulating, It is an exciting area to work in. A secondary consideration would have to be the New Mexico environment. My own personal way of working has been to change fields frequently, although always within physics. I started out at the Laboratory as a theorist in T Division and then became part of the fledgling isotope separation program and was Leader of the Laser Development Group until 1980. Then I took over my present job as Deputy Associate Director for Inertial Fusion. To me the main attraction of the Laboratory, in contrast to universities, is its ability to pull together the resources to do a large multidisciplinary program and move on it quickly.

JOHN WHEATLEY: I received my doctorate from the University of Pittsburgh in 1952 and came here just recently, after stints at the University of Illinois and the University of California, San Diego, because I saw the opportunity to do both the basic physics research that is my main line of work and also what I call fundamental technology. That combination is highly regarded here, while in my previous university careers I always felt I had to sneak my interest in technology in the back door. After all, instruction through basic research, not development of technology, is the principal function of a university. Also, I perceive a very substantial increase in my effective mass here because the Lab has many more people interested in my field, which is thermal physics and condensed-matter physics.

Challenges and Prospects

by Donald M. Kerr

On this occasion of the 40th anniversary of the founding of the Los Alamos Laboratory. I would like to shape in broad outline my hopes for the Laboratory in the next decade. Though some of what I will say may go beyond what might be labeled as realistic, we must have such high hopes, for they help us stretch our capabilities. I will also address some substantial obstacles that could, if not countered, negate our best attempts to help the nation solve some of its pressing problems.

My first hope is that Los Alamos scientists will play a prominent role in reshaping the defense posture of America through efforts along three lines—arms control, nuclear weapons, and advanced weapons concepts.

The people of this planet have no more important task than to subdue the spiraling arms race and to eliminate the fear that, by accident or by design, nations might eliminate large portions of life on this earth by engaging in a massive nuclear exchange. While science cannot solve the political problems that snarl arms control talks, improved technology in satellite surveillance, seismic detection, and information analysis can help decrease the possibility of agreement violations through surprise actions, clandestine activities, or new developments. Such technological assistance is not likely to be the key element in advancing attempts to curb the arms race but may be useful if political developments become favorable.

Our nation's efforts toward arms control must be made from a position of strength. And that strength depends on being at the forefront of all scientific areas likely to yield new military applications. In the area of nuclear weapons, Los Alamos can make the following specific contributions.

- Encourage the modernization, where appropriate, of nuclear warheads to provide the best safety and security features technology can offer.
- Assure the effectiveness of nuclear weapons over a wider range of operating conditions.
- Improve the protection of warheads against newly developed electronic countermeasures designed to defeat our weapons.
- Develop new means of making our weapons more effective

against hardened targets in the Soviet Union.

- Improve the techniques for defending our own strategic forces from a first strike.
- Determine the feasibility of newer weapons, including those involving particle beams and lasers.

Finally, Los Alamos can contribute to the nation's defense through efforts in what we call advanced weapons concepts. This Laboratory was created to meet what was viewed as the most critical defense issue facing the country in World War II—the possibility that our enemies were developing a weapon based on new science and new technology. It is vital that the critical military needs currently facing the nation be met in a similar fashion today.

One advanced weapons development would be the introduction of truly intelligent weapon systems to the battlefield. Such systems have been discussed and popularized, but the immensely difficult task of developing them, although possible, remains to be done. I have in mind a weapon system including multiple sensing techniques coordinated by sophisticated electronics and computing capabilities. The intelligent weapon system would be integrated into an overall battlefield posture involving land, sea, and air forces.

Ten years or so ago the prospects for artificial intelligence were oversold, and work in that area received a bad name. But significant developments over the past decade suggest that now is the time to initiate its application. Already a number of techniques for using computers as expert systems are in the early stages of application. For example, one computer manufacturing company is using a modest form of artificial intelligence to establish the appropriate configurations of computer systems for purchasers. A computer programmed with more than two thousand rules and fed the requirements of the purchaser determines the configuration of equipment that best meets those requirements. Another and perhaps the most widely noted example is the use of computers in medical diagnosis to help physicians make the complex judgments required of them when faced with multiple symptoms and test results. In over 95 per cent of the tests thus far, diagnoses made by the computer agree with those of expert physicians.

The eventual goal in a military context is a weapon system that can be sent into a battle situation to sense and analyze many complex, perhaps rapidly varying factors, such as terrain, environmental conditions, and the nature and movement of enemy forces and weapons. The system, controlled by artificial intelligence, would make the decision as to which of its weapons to deploy and in what manner they would best be utilized. Such a system may sound far-fetched to some, but the technology required has progressed to the point that it should be vigorously pursued.

A nation possessing an intelligent weapon system would have a great tactical and psychological advantage over its enemy. Furthermore. smart weapon systems equipped with today's advanced nonnuclear warheads could displace low-yield, short-range nuclear weapons and thus considerably reduce the tension associated with the posting of nuclear weapons close to an enemy's borders.

Research along these lines should be pursued, and Los Alamos, together with Livermore and Sandia, can make important contributions in the next ten years, if properly supported and freed of extensive program strings, milestones, and reporting requirements. Modest funding of a few million dollars per year to each of the weapon-related national laboratories would be a sufficient beginning.

There are many other exciting advanced weapons concepts; I will mention only a few. We have ideas for antiterrorist technology that could reduce the impact of threats in many areas. We see means for detecting and protecting against chemical and biological threats. And we see a possibility of developing microwave weapons, which could become very important as electronics becomes more and more integrated into the battlefield.

My second hope is that the Laboratory will make major contributions to solving a problem that has commanded great public attention—the problem of supplying the energy needs of the nation and the world. The Laboratory has devoted a substantial effort to energy programs during the past decade, and it is my hope that as these efforts reach maturity in the coming decade, they will bear technological fruit in the following forms.

- Safety and engineering advances that will make nuclear power a more acceptable approach when the world turns again to this energy source, as I believe it eventually will.
- Nuclear waste disposal techniques that will satisfy public concerns.
- Techniques for extracting fossil fuels from the earth that will provide greater efficiency and worker safety and cause less pollution and environmental damage.
- Practical fuel cells that will power many diverse activities, from transportation to materials production.
- Geothermal projects that will tap the heat of the earth's mantle to provide a clean and safe supply of heat and electricity.
- Advances in renewable energy technologies that will allow for decentralized energy supplies so necessary in rural America and in many developing nations.

Controlled fusion is a major area in which we have made and continue to make important contributions to the development of a new energy source for future use. Since the early 1950s Los Alamos has played a major role in the international development of magnetic confinement science and technology. This cooperative effort has led to such a high level of sophistication that demonstration of energy break-even, using the mainline tokamak approach, seems assured during this decade, The ability to confine reactor-grade plasmas for times close to those required for thermonuclear ignition is an enormous scientific accomplishment that could not have been achieved without the resources that national laboratories, universities, and industry brought to bear on this problem.

At the same time it is clear to me that the demonstration of scientific feasibility on the tokamak will not automatically assure its economic feasibility as a power-producing system. It is likely that proof of commercial feasibility will fall to a different fusion concept whose inherent confinement requirements reduce engineering complexity and therefore cost to the point where it can become a practical system for the nation to adopt, or perhaps commercial feasibility will fall to much more advanced tokamak systems yet to be developed.

I believe the work going on at Los Alamos will play a significant role in developing a power-producing fusion reactor. I am encouraged in this respect by recent successful developments in our Reversed-Field Pinch and Compact Toroid programs because the efficient confinement properties of these schemes provide the magnetic fusion program with a new possible end-product: the compact, high-power-density reactor. This new approach efficiently utilizes resistive copper magnets and therefore differs qualitatively from the conventional reactor models, based on superconducting magnets, in greatly reducing the size, mass, complexity, and cost of a reactor and the time required for reactor development. These alternative fusion concepts are at an earlier stage of scientific development than the tokamak. Their potential for resulting in a significantly better commercial product provides the rationale for support in a well-balanced and prudent national program. Ideally, in such a program the allocation of resources will permit the full potential of these alternative concepts to be realized so that their best reactor attributes can merge with the more mature development base for the mainline approach to produce an optimized fusion system.

Diverse funding of numerous approaches is the best means for overcoming the great technical challenges posed by controlled fusion. If such funding occurs, I believe that Los Alamos can develop fusion power systems that are smaller, cheaper, and more easily maintained. Such developments may enhance the willingness of society to adopt this form of technology.

My third hope concerns the application of the Laboratory's expertise in physics, chemistry, and engineering to the new challenges in the fields of biology and medicine. Two instruments of fundamental importance to biomedical research have been developed at Los Alamos. These are the liquid scintillation spectrometer, which makes possible simultaneous counting of different radioisotopes, and the flow cytophotometer, which allows rapid analysis and isolation of individual cells. The latter development resulted in the establishment at Los Alamos of the National Flow Cytometry Resource. Current activities give me confidence that the next decades will see developments of similar importance to biology and medicine.

For example, improvements in flow cytometry now allow rapid identification and separation of chromosomes. This capability, coupled with powerful recombinant DNA techniques, opens new approaches in cell biology and genetics. The chromosome rearrangements characteristic of tumor cells can now be closely scrutinized, and this information may provide insight into the origins and abnormal behavior of cancer cells. With similar techniques cultured plant cells may be manipulated to produce new crop varieties with desired genetic characteristics, such as disease resistance and environmental tolerance.

Another example is the development of noninvasive techniques for analyzing human functions with minimal discomfort to the patient. In one such technique a nuclear magnetic resonance coil is used to follow the course of metabolic processes from outside a patient's body. The coil detects important intermediate products of metabolism that have been labeled with a suitable magnetic isotope, such as carbon-13. The labeled materials are available from the Laboratory's Stable Isotope Production Facility, which pioneered in the field of stable isotopes for biomedical research.

The Laboratory is also developing advanced physical techniques for biological and medical applications. Examples include rapid, precise identification of microorganisms based on their scattering of circularly polarized light and detailed structural analysis of biological macromolecules based on neutron and x-ray spectroscopy.

Another venture into the realm of biology exploits our computing capability—the largest in the world—to compile and make available to the scientific community a library of genetic sequences. Los Alamos has recently been designated as the site of the national DNA sequence data bank. This data bank will contribute significantly to unraveling the mysteries of DNA.

The Laboratory has a major responsibility in developing secure alternative energy sources such as shale oil. Experimental shale retorts and advanced capabilities in cellular and genetic toxicology provide the opportunity to choose extraction and processing methods that produce the least harmful pollutants. This will involve using the advanced techniques described above to study the effect of pollutants on cells.

It is my hope that, with strong inputs from academia and industry, the advanced physical, theoretical, and computational capabilities of Los Alamos will contribute to a decade of imaginative and striking benefits in the areas of biomedical research, energy development, and environmental science.

My fourth hope is that the Laboratory will continue to involve an increasing number of scientists from universities and industry in its activities. We have already made great progress in this area by establishing three centers designed to reach aggressively beyond our borders: a branch of the Institute of Geophysics and Planetary Physics, the Center for Nonlinear Studies, and the Center for Materials Sciences,

In terms of new efforts, I see the following possibilities.

- That not one but two or three of the world's most Powerful computers will be available beyond the bounds of our security fences for use by collaborating scientists from other institutions.
- That more and more students and faculty will become familiar with our activities and facilities by choosing to pursue research at Los Alamos.
- That our staff will increasingly aid in the transfer of technical information to industry and to universities by sharing in joint exchange appointments.

It is, of course, impossible to mention all significant advances expected in a laboratory as diverse as Los Alamos. But one final hope is that we will be surprised by some unexpected development or discovery that derives from the exploration of new questions and new possibilities. The very nature of scientific research makes such surprises possible, and for this reason basic research is a fundamental element in our plans.

To realize the hopes that I have outlined, difficult scientific problems will have to be confronted, pursued, and conquered. But those efforts now face challenges beyond the inherent scientific difficulty.

A changed political and social climate challenges these hopes. Some voices now question the major mission of the Laboratory. They ask, "Why is the Laboratory still engaged in weapons work?" That question often comes from those who believe that the thousands of nuclear warheads now in our arsenal are more than adequate and that no more effort in this scientific area is needed, These people deserve a reply.

Three chief factors drive our continued efforts in weapons, I touched on two of these above but their importance leads me to reiterate. The first is the extent to which potential enemies of the United States are making technological advances that could jeopardize the defense posture of the United States. This issue led to the creation of the Manhattan Project during World War II, and it is still a valid concern in the present political climate. Our political leaders generally feel that their ability to influence world affairs is affected by the extent to which the United States maintains technological supremacy in the defense area.

The second factor is the need for solutions to technical problems that may inhibit accords on arms control. Any agreement on this subject rests heavily on the ability to determine that its provisions will be followed by each signatory, The inability to verify compliance has created stumbling blocks in past negotiations. The Laboratory must assist in developing new verification techniques, for they maybe a critical link in reaching the goal of arms control. The Laboratory will also be called upon to help policy makers understand the capabilities and limitations of current approaches to verification,

The third factor is the certain knowledge that the pursuit of science inevitably yields ideas for new technologies that have a wide variety of applications, including military ones. The choice to develop the new military applications is the nation's. But the nation cannot choose to stop the scientific effort that creates those applications without also stifling development in other human endeavors. Science is neither compartmentalized within itself nor isolated from its surroundings. New scientific ideas have a way of leaping traditional boundaries among fields of science and of creating vast and unforeseen changes in the economic and political fabric of society.

Another challenge facing the Laboratory is the idea of some that our research activities be transferred to academia and industry, You might ask, "What is the place of Los Alamos in the midst of the country's large and sprawling research community?" After all, research efforts at universities have grown substantially since World War H, and industry has also seen reason to invest in research and development.

I believe there is a clear place for Los Alamos and other national laboratories. That place goes beyond weapons work, which the government obviously must control directly, to other areas of research in which a strong national interest justifies the presence of a federally supported laboratory.

For example, many areas of research-a notable example being nuclear fusion—face such inherent difficulties that they will yield results only over a very long term, Industry will not be inclined nor financially able to enter such areas. Another example is the area of research on the protection of workers, the public, and the environment from technologies new or old. Here the profit motive of industry may bring into question their objective assessment.

National laboratories such as Los Alamos can address these issues, and, in fact, Los Alamos is extraordinarily well equipped to do so. Our scientific computing capabilities are unsurpassed. We have the experience of dealing with military agencies and understand their needs and procedures. We can work in a way sometimes referred to as vertical integration: that is, we can develop an idea for, say, an instrument all the way from conception to production engineering. Our activities range from undirected basic research to production engineering of devices that weigh tons. We can transform ideas or bits of Nature's secrets into products useful to mankind. Of the thousands of laboratories in the nation only a small handful match this Laboratory's capabilities.

The world is increasingly specialized, compartmentalized, separated into isolated parts. The concept of integrated teamwork bringing mathematicians, physicists, chemists, biologists, engineers, and economists together for a sustained effort is not a tradition at very many institutions. In fact, it seldom happens. It is difficult to bring about. In many places it is impossible. At Los Alamos it is the usual practice. It is the way we have conducted business from the beginning.

The third challenge facing the Laboratory in the next decade concerns the level of financial support for its activities, particularly for basic research. Funding reductions can harm our work in important ways, and basic research often suffers more harm than other areas because sponsors are inclined to view it as less important than work closely coupled with an approaching milestone.

In the mid 1970s Congress established a new budget process in recognition of the need to review federal economic policy and to reduce the federal deficit. The resulting tighter budgets and economic policies have affected virtually all the Laboratory's activities and present a most serious challenge. My hopes for Los Alamos cannot be realized unless increased funding is available. The requested increases are modest but essential and represent a valuable investment for the nation.

The Laboratory is being asked to make sure that its work in major programs connects directly to program objectives that will yield usable technological applications. This emphasis must not be overdone, and in some cases that line has already been passed. When investigations have reached the stage at which such requests are appropriate, the emphasis may help us do what we want to do—to show that our work can solve national problems and lead to benefits for the nation and the world.

But we must constantly guard against demands for immediate, practical benefits from science. When basic questions are still being explored, when answers are only beginning to appear, and when technological applications are only dimly perceived, then questions of practical benefit must be deferred. If we at the Laboratory do our job well, we will open new areas of science that eventually will yield benefits. The nation must allow competent scientists to explore those areas and to confront the difficulties that may take years to overcome, satisfied that this investment is worthwhile. Budgetary restraints must not be allowed to force out all but research that is immediately applicable, for that course would amount to eating the seed corn of future harvests.

Let me conclude with a final challenge—the desire of some that science should overcome the tangled web of politics and assure that all its results are used only in positive ways. Such a desire is natural, but it is too much to expect of any single sector of society.

At the end of World War II, those at Los Alamos learned with the rest of the world that technical developments were beyond the control of the small group of scientists who pleaded that the results of their work be used solely for peaceful purposes. That control rests with the broader institutions of society. Today we continue to pursue the unanswered questions of science in the belief that our efforts will enhance the peace and prosperity of the world. The ultimate hope of those of us-at Los Alamos is that the voices for peace will prevail in all decisions that affect the use of our endeavors. ■

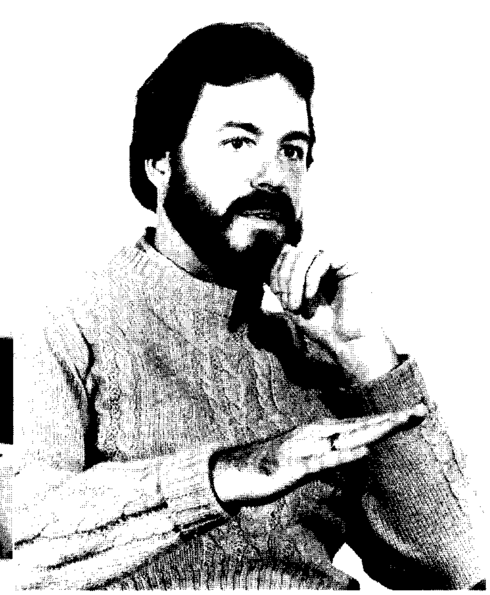
What's Happening Now...

What better way to learn about the state of the Laboratory—its present excitement and its future possibilities—than to talk with some of the outstanding scientists at Los Alamos. We chose ten who represent a wide spectrum of fields and asked them to share their personal views on the mission of the Laboratory, the current work, the management of research, and some pragmatic directions for the future.

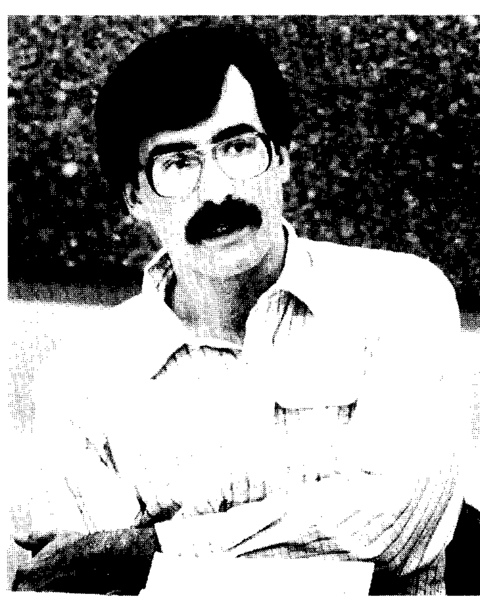
SCIENCE: *I know that many of you chose to come to Los Alamos for personal reasons and are enthusiastic about its setting, its people, and your own work here. But Los A lames has always been a*

Dan Baker

Stirling Colgate



Brian Crawford



Rocky Kolb

Jeremy Landt

mission-oriented Laboratory, and I wonder how you view that mission and your role in it?

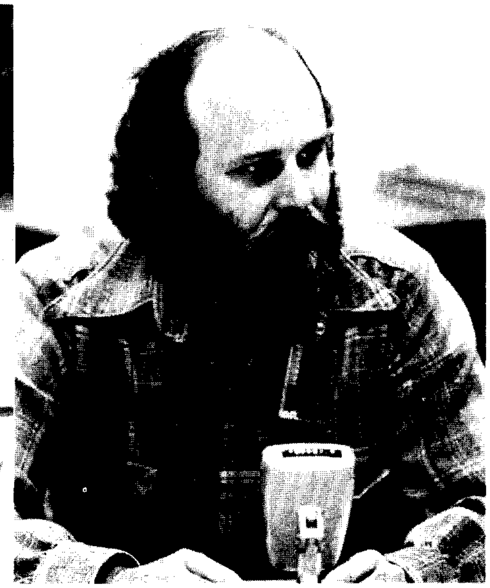
BAKER: Let me suggest a definition of the main mission of the Laboratory. Our mission is to provide input on all energy and national security issues that have a scientific or technological component. Is that general enough?

WHEATLEY: Yes. but I wonder whether the Laboratory's management has firmly in mind what technologies and ultimate applications we should be seeking.

HECKER: I personally feel that national security is our most important mission. Essentially, the country has entrusted to us and to Livermore their nuclear defense.

Sig Hecker

Steven Howe



Mac Hyman

Steve Rockwood

John Wheatley

Dan Baker on Space Science

The Vela satellite program to detect nuclear explosions in space has led scientists at Los Alamos to satellite exploration of the magnetosphere and of a wide variety of other space phenomena. Some of the instruments aboard such spacecraft have been designed to measure the interplanetary medium and planetary bow shocks, and we are doing theoretical studies in support of these observations. A related study is our work on cosmic particle acceleration. The information about energization of particles at interplanetary shocks may have applicability to shocks of much more cosmic proportions, such as those presumed to exist in supernova remnants.

We are also exploring the interplay between the solar wind [the hot, expanding corona of the sun] and the magnetic field of the earth. This interplay produces the magnetic structure we call the magnetosphere, the tenuous plasma region that makes up the uppermost part of the earth's atmosphere. We are doing computer modeling of the entire magnetosphere and, furthermore, are developing computer network links to many other institutions involved in similar work.

In a more practical vein we are using our advancing technology to do experiments in which we release chemical tracers into the ionsphere or even deeper into the magnetosphere to learn in what way these additives may modify the outer parts of the earth's environment.

Still another project is attempting to use an existing satelite in a different and innovative way. The International Sun-Earth Explorer 3 [ISEE-3] spacecraft has been orbiting at the L-I

LANDT: Certainly the Laboratory is aware of its obligation to help the country defend itself and to maintain a balance of technologies. Right now I am assigned to the Weapons Advanced Concepts Program Office, which was begun a year ago to try in a practical way to determine which technologies really make a difference for the national defense so that the country won't throw its money away on the wrong things. The Laboratory management is very interested in addressing this issue, and they have put dollars behind it and people to work on it.

ROCKWOOD: Today the government's method of doing business is very much applied and mission-oriented. Although basic research is also essential to our national security mission, it is often overlooked, and the national laboratories are handcuffed in this area by administrative limitations. People here have to be clever in extracting from their mission-oriented programs good basic results in science. I think Los Alamos has been rather successful at that.

WHEATLEY: Do you think mission orientation is a good thing? As a matter of principle?

ROCKWOOD: Moderation in all things.

BAKER: I think we must tight this trend toward applied work only, toward everything having an immediate payoff. A national laboratory should play as active a role in basic research as any laboratory. The country will suffer in the long run if we don't.

ROCKWOOD: Often the most exciting and fundamentally useful part of a program is not its stated objective but some unplanned spin-off. In the laser isotope separation program, spectroscopists working to explain the spectrum of the octahedral molecule UF_6 discovered that the octahedral symmetry group had originally been analyzed incorrectly and had been wrong in the literature for years. Even a very applied program may yield results of use to basic science.

BAKER: That's certainly been true in space physics. The Vela satellite program to detect nuclear explosions deep in space was a mission-oriented project, and we continue to have test and verification activities. To accomplish that practical goal we had to place instrumentation on the spacecraft to measure the environment. As a result, many properties of the magnetosphere were discovered.

Now the space physics groups are involved in a number of activities on collisionless shock waves, cosmic particle acceleration, the interplay between the solar wind and the earth's magnetic field, and the exploration by the International Sun-Earth Explorer 3 satellite of the night side of the earth.

SCIENCE: *How do you get funds for all these activities?*

BAKER: In a variety of ways. We have been able to obtain reimbursable funding from NASA [National Aeronautics and Space Administration] for some of our projects. But the continuing money from the weapons program gives us more stability than we could ever obtain from reimbursable funding alone. When we get our funding from the DOE [Department of Energy] or from the Laboratory, we are better able to make long-range plans. It's fortunate for us that the Europeans are also participating in many of our scientific satellite programs because the European Space Agency plans much further ahead than NASA does.

HYMAN: There are some problems with diversified funding. The Mathematical Modeling and Analysis Group in the Theoretical Division is almost completely basic research, and we also have been obtaining some support from outside the Laboratory. The largest block grant we have supports only one and one-half staff members. Because our funding comes in such little pieces, we are perpetual job hunters and odd jobbers—always knocking on a different door.

ROCKWOOD: The country hasn't learned how to fund basic science at all. Research doesn't integrate with time. Each administration

point on the sunward side of the earth for about four years. The L-1 point, the sun-earth Lagrangian point, can be thought of as an imaginary center of mass around which the satellite has been traveling in a large looping orbit. Now this satellite has been moved into the earth's distant magnetotail and is orbiting well downstream on the night side of the earth. It will be the first spacecraft to explore that region in space. To accomplish the move, the satellite's gas-jet thruster, which ordinarily performs minor station-keeping orbital adjustments, was used to move the craft in such a way that it encountered the moon's gravitational pull and got a lunar gravitational assist to kick it deep into the magnetotail. It is not in a stationary orbit, and thus the lunar encounters must occur every one to three months in order to keep the satellite deep in the magnetotail. Eventually another lunar push will occur, and ISEE-3 will go on to intercept a comet. This will be the first time that any spacecraft has gotten close to a cometary body.

Bob Farquhar, a very creative guy at NASA who seemingly can move any satellite anywhere you want using any other celestial object, helped with the ISEE-3 project and has also helped to plan what is called the International Solar-Polar Mission. Because we don't have enough energy in most launch vehicles to get significantly out of the ecliptic plane [the plane of the earth's orbit], we are sending a satellite out to Jupiter to get a large gravitational kick from that massive planet. The spacecraft will then move above the ecliptic plane and travel high over the sun's pole, another previously unexplored region. ●

comes in and has a new policy. Basic science suffers more from these oscillations than it would from a low level of sustained funding. And I believe Los Alamos suffers more from funding oscillations and changes in direction than other national laboratories. Our normal attrition rate is about 4 per cent per year. Any change in direction by more than that amount involves moving people around. People's skills are not always totally applicable to a different program, and those who are not absorbed by other parts of the Laboratory are not absorbed by the town at all. It is this very closed environment, which drastically constrains our flexibility, that I see as a major problem for the Laboratory. It always has been so.

Returning to the question of the funding of basic research, I feel that, although the government can't just pour out money and expect nothing in return except good intentions, the funding "pendulum" has swung too far toward applied activities.

WHEATLEY: Some of you would say that Los Alamos ought as a matter of principle to devote some fraction of its work to purely unqualified basic science, the sole motive being to understand things better and to develop knowledge or whatever-to have fun, really. I would like to suggest that perhaps that's not true. Perhaps it is our responsibility to articulate the possible relationship between our work and some appropriate mission of this Laboratory. I am not thinking of explicit applications, necessarily. Let me give you a personal example. I think that it is appropriate that my work in thermal and condensed-matter physics should feed into thermal technology. broadly defined, that is to say, into technologies that involve the concepts of energy, work, heat, temperature, and so on.

Right now I am working on heat engines. I had set myself a semipractical problem that no one in industry would define as practical of course—but it was. It had to do with producing cold very simply. I had an idea for doing that with acoustics, so I started playing around with the idea, developing it, and soon—meaning one year later—I found that what I was doing seemed to me to have very broad implications. Now I have put possible applications off to one side, and I am looking strictly at the basic science. at the fundamentals of it. I think I have identified what I regard as a new principle applying to heat engines in a very general sense. I do feel a responsibility ultimately to be able to draw a connection between the basic scientific work I do and some technology.

KOLB: I don't feel that way at all, There is a real necessity for nonmission. For fifteen years people have been looking at magnetic monopoles, intensively, just for pleasure, and for the past five or six years have been studying grand unified gauge theories—same motivation. Recently, Rubakov in Russia and Callan at Princeton have proposed that monopoles can catalyze proton decay, can just completely convert the rest mass of protons into energy. It will be another five years before it's worked out. Now something like that would have a tremendous payoff. would be comparable to Otto Hahn's discovery of fission. But it never could happen in a mission-oriented environment. No one told these people they should study monopole structure because it might have important applications. And no government agency has told me I should be studying them, either.

WHEATLEY: I'm not waiting to be told what I should do, either. For instance, I would feel perfectly tine studying spin-polarized hydrogen, a project in which 1 am very interested. Nor can I tell you what gadget that might be used in, but I do see that it is part of the foundation for thermal physics and that we ought to understand it.

KOLB: I don't choose research projects by wondering if they will have any impact on technology.

BAKER: Aren't you thinking of beam weapons systems using

monopoles?

KOLB: If I think about it, it is only after doing the basic science.

HOWE: Is it necessarily the basic researcher's responsibility to come UP with the utility of it? There are, perhaps. other people who are more interested in the engineering side, so they take the proton-monopole catalysis concept that Rocky mentioned and say. "Well, let's develop starship drives: let's design power reactors!"

SCIENCE: *Rocky, how do you choose your research projects? You 've said how you don 't choose them.*

KOLB: I don't know, actually, I don't know what I am going to be doing tomorrow or when I go back to my office. I read the literature and see what other people are doing. This communication is very important. I follow the direction the work is going.

HYMAN: You may recognize a problem as being important, but in the end the choice is subjective. A question gets under your skin, and you can't let loose until you understand it. That's the driving force behind science—the need to understand. As far as Rocky's responsibility to the Laboratory, that has become clear as he's talked, His obligation is to push back the frontiers of basic science—that's his job description. At the same time every scientist has a responsibility to the overall health of the Lab. Whenever you discover something that could be applied in a programmatic effort, you go down the hall, knock on doors, and make sure the right people know about what you have done.

KOLB: When I first read about Callan and Rubakov's work on monopole-catalyzed proton decay, I was at Aspen, and I said, "Well, I have to get back to Los Alamos and tell people about this." but then Stirling and I decided it couldn't work, so I didn't go knocking on doors.

WHEATLEY: Coming back to the missions of the Laboratory, I understand why we should be doing some basic science and much fundamental technology, that is. research on problems whose ultimate objectives are fully seen. However, my own view concerning applied work and hardware is that if you have a particular, well-defined job to do, the private sector would probably do it better.

HECKER: I would disagree, John. The weapons mission is a specific job, and we have done it very well.

WHEATLEY: The weapons case is rather special because of the national security problem. Suppose that you took the secrecy requirements away.

BAKER: In fact, private industry does secret work, builds all the components. We provide the overall science and technology. I don't think secrecy is the defining factor, The national laboratories are most effective doing both the theory and the design development of jobs that are high risk and from which an industry couldn't expect a profit in a short term. Fusion is another example.

HYMAN: Our exceptional facilities also give us an edge over industry. The two thousand scientists at Los Alamos comprise a pool

Sig Hecker on Materials Science

Materials are the *sine qua non* for new technology. At Los Alamos we have been in the business of processing new materials for technological needs from the very beginning. Now materials processing is becoming more sophisticated as we learn to exploit our understanding of materials on an atomic level. Our work on rapid solidification and ceramic processing exemplifies this trend.

So-called rapid-solidification-rate materials are made by cooling the liquid state very rapidly, on the order of a million degrees per second. The rapid solidification avoids equilibrium decomposition and consequently affords the opportunity to create materials with new and novel structures, For example, if you smash a liquid metal between an anvil and hammer or spin it against a cooled, rotating wheel, you can create a metallic glass, that is, an amorphous metal rather than a metal with the normal polycrystalline structure. Properties of metals depend critically on their crystal structure, or, more specifically, on the defects in the crystal structure. By creating an amorphous metal, we eliminate grain boundaries, which contain many defects and are therefore places where corrosion begins. Consequently, these metallic glasses have good corrosion resistance as well as high strength. Our rapid solidification work at Los Alamos has been applied mostly to processing actinides.

Our work in ceramics processing is aimed at a new class of structural materials for high-temperature environments, such as those involved in fuel processing and power generation. For example, a ceramic turbine might be used to achieve higher operating temperatures and higher efficiencies.

State-of-the-art work is being done in two areas: processing of dense ceramics without densification additives and growth of ceramic whiskers, The ceramics of greatest interest to us, silicon carbide and silicon nitride, must be made at relatively low temperatures to avoid decomposition. A densification additive forms a glass phase between the powder particles and essentially glues the particles together. Unfortunately, during high-temperature service, in a turbine for example, the glue turns glassy and

the ceramic loses strength. To eliminate the need for an additive, we have developed a technique for making an extremely fine, extremely reactive powder that shows great promise of densifying at low temperatures. We form the fine powder particles, which have diameters on the order of hundreds of angstroms, by a plasma-assisted chemical vapor-deposition process. In this process the constituents, such as silicon and carbon, are carried by appropriate gases and are reacted in a hot argon plasma. We are also using the Laboratory's expertise in shock loading to activate ceramic powder containing larger diameter particles. The idea is to produce a large concentration of defects on the surface of the particles before attempting to consolidate them.

Ceramic whiskers, a field in which we are the world leader, are long, single-crystal fibers of, for example silicon carbide or silicon nitride, with diameters that vary from less than a micron to maybe ten microns. These single crystals are grown by a process called the vapor-liquid-solid process. They are essentially defect free and have enormous strengths, from ten to fifty times that of structural steel. We are now trying to incorporate the whiskers into a composite material-a glass matrix, a ceramic matrix, or a glass-ceramic composite—to make high-temperature materials. Essentially, we are using processing science to control the strength and the ductility of materials on a microstructural level.

Another area that is not new, but extremely fascinating, is the actinides. In the last few years a marriage of condensed-matter physics, chemistry, and metallurgy has helped us to understand the intriguing electronic and magnetic properties of these elements and, in particular, how they determine the macroscopic properties of plutonium, uranium, and americium. For plutonium, especially, the only way to understand it is to understand the role of its bonding *f* electrons. For example, because the *f*-electron wave functions possess odd symmetry, bonding of these electrons favors unusual crystal structures with low symmetry. People in academic circles are now becoming very interested in the actinides because they offer new physics. ■

of knowledge found in only a very few places. Also we have five Crays and a complete set of shops.

WHEATLEY: We do have a complete set of shops, but it costs fifty-five dollars an hour to use them.

HYMAN: But they are at our disposal.

COLGATE: Just for a moment let me reduce the main missions and the main capability of this Laboratory to plain terms. Suppose we didn't have a Laboratory. Why would Congress, the politicians, want to start one? The only reason would be because they were scared: scared of losing the country—that's our national security mission—or scared of losing our way of life and our power—that's the energy mission. Fear for the future motivates the existence of this Laboratory. Politicians would never fund science from purely altruistic motives, and purely educational business would be in the universities where it belongs. But how do you make sure that a new idea doesn't come up to bite you from the rear, as Sputnik did? You have the most brilliant people around to think up all the new ideas that are possible before someone else thinks of them. So the basic capability of this Laboratory is its brilliant individual scientists. If someone wants you to come to the Laboratory, why do you accept? Because people here are doing the most exciting research in your field, and because you believe in your own ability,

ROCKWOOD: There's something I worry about, and I'd like to mention it here. At moments of international crisis, programs for the national laboratories are easily defined. But during periods of uncertainty about the future, and especially during periods of economic stress, the selection of programs is not so simply made. One of the strengths of Lo's Alamos internally is its great freedom of thought—freedom to disagree, to discuss openly with management the pros and cons of particular technical endeavors. It makes us stronger to have had these discussions and to look at all sides of a problem before going into it. But we should speak with only one voice to the external world. We don't need two, three, half a dozen people showing up in the same office in Washington, each with a different opinion as to which major programs the Laboratory should be pursuing.

SCIENCE: *While you more or less agree that the development of high technology for national security is the Los Alamos mission, the specific emphases and manner of carrying it out remain open to discussion. Perhaps we should turn now to some of the specific areas of research and development that are clearly important. Carson Mark has commented that many of the problems in technology development are materials problems. Sig, would you tell us what is being done at Los A lames in this area?*

HECKER: Our materials science effort demonstrates the exciting and productive relationship that exists between theory and experiment. It is one of the beauties of this Laboratory that metallurgists, physicists, and chemists work side by side. Our main interest in materials

processing, without question, has always come from the weapons program. Weapons designers, be they physicists or engineers, come to us with requests that to them seem exceedingly simple and to us almost impossible, at least at first glance. For example, the physicists wouldn't hestitate to ask us for structural air, that is, something with no density but enormous strength. Faced with sophisticated problems for years and years, we've learned how to tailor-make many special materials.

We have also done some basic research in materials science, and in the past few years we have begun to apply our understanding of materials on an atomic level to materials processing. One example is rapid-solidification-rate technology to make amorphous metals with high strength and good corrosion resistance, Another is ceramics processing; we are attempting to make materials for high-temperature environments, such as composites containing single-crystal ceramic whiskers.

LANDT: Electronics is another field that combines ideas and applications; it's partly software and partly hardware, and it's a crucial part of future technologies. I would like to put before you a statement by Dr. DeLauer, Undersecretary for the Department of Defense. Dr. DeLauer insists that electronics is the most critical of all technologies for the maintenance of peace, and he claims that "Further development of the electronics technology base of the United States is as important to defense today as the atomic bomb in World War II."* I think it's time the Laboratory took its electronics seriously.

BAKER: There are, however, a lot of good electronics firms.

LANDT: We are working on several projects that could make significant contributions in electronics—areas that private industry is not touching. These include high-speed electro-optic switches and thermionic integrated circuits that have important military as well as commerical potential. We are also developing high-power microwaves from lasers. This is research that could not be done without the exceptional computer and experimental facilities at Los Alamos.

SCIENCE: *Since we have mentioned speaking freely, I'd like to ask Steven whether there's anything he can tell us about weapons design work.*

HOWE: Most of what we do is classified, but I can say that we work to get better codes, better computational abilities to describe the processes in the weapon, to put in the things we do know so that the things we have to extrapolate can be better estimated. In the year I have been here we have come up with several interesting pursuits. One is in low-energy nuclear physics: there is a process that we think exists in the weapon but that we don't account for in the codes. This particular development is interesting because we have shared it with Livermore, and we have collaborated with them in getting it into the codes and making estimates. We also do secondary design work on weapons materials, attempting to understand basic processes. Generally we aim to satisfy the military requests and to come up with smaller, more efficient devices. We are continually looking at new

Jeremy Landt on Electronics

Our heavy reliance on the world of electronics has led Los Alamos into several fledgling projects that show great promise for the future. One is the development of the high-speed electro-optic switch, which can be used to probe integrated circuits with pulse widths of 50 picosecond or less. Understanding of semiconductor physics on these short time frames is essential for development of reliable, very high-speed integrated circuits for future weapons systems. The first generation of very high-speed integrated circuits is largely based on extrapolations of existing technology. To go beyond will require new technologies and understanding that industry does not have at present.

Another device under development is the thermionic integrated circuit, which is inherently hardened to radiation and EMP phenomena. Before research on this device began at Los Alamos an attempt to commercialize the technology failed because the basic physics was not understood. We could use this device to instrument nuclear and geothermal systems, as well as in military applications.

The area I find most exciting, however, is the broad area of high-power microwaves. We are working on novel generation mechanisms as well as novel applications. One new generation scheme involves the Helios laser, the "Laboratory's high-power carbon dioxide laser. Large numbers of hot electrons are generated in high-power laser targets. A carbon dioxide laser produces far more hot electrons than do lasers operating at shorter wavelengths. We are presently investigating ways of converting these electrons to high-power microwaves. The power levels achieved to date are very impressive and probably can be emproved much more. At present this research cannot be done anywhere else in the world. Los Alamos has both the computer codes to handle the flow of particles in electronagnetic fields and the experimental facilities to benchmark the codes. ■

*"Richard D. DeLauer, "The Force Multiplier," ***IEEE Spectrum,** *October 1982, p. 37.*

Brian Crawford on Life Sciences

Several exciting things are happening in life sciences. We are using laser-based flow cytometric methods to separate chromosomes from mammalian including human, genomes. DNA from these isolated chromosomes can be cloned by recombinant DNA methods, allowing studies of the basic structure and functional organization of the chromosome. Los Alamos is one of perhaps three labs with the requisite expertise in biophysics and molecular biology to perform this work, and recent NIH [National Institutes of Health] funding to establish a Flow Cytometry National Resource is fostering progress in this area.

We are also working on cellular oncogenes. These genes are thought to control the evolution of the normal cell toward malignant change. The isolation, that is, the cloning, of such genes by recombinant DNA methods and the reinsertion of these genes into normal cells, by a process known as DNA-mediated gene transfer, permit us to study how specific oncogene expression can result in cancerous change. We are also studying the role that gene rearrangement, which can result for example from chromosome damage, can play in the initiation and progression of cancer. This work relates to DOE concerns regarding the effects of both ionizing radiation and the by-products of fossil-fuel development and consumption.

Another exciting development is the establishment of an NIH-funded DNA sequence database in the Theoretical Division. Sequencing, or decoding, of the genetic code in cloned fragments of DNA is meaningful only if such information can be stored, retrieved readily, and analyzed. Just consider for a moment that each mammalian organism expresses on the order of fifteen thousand distinct genes in a cell—not to mention that each cell has DNA encoding for an amount of unexpressed information hat is several orders of magnitude greater. Software development for the analysis of the stored sequences will be pursued oncomitantly with this Herculean bookkeeping effort. ■

things and attempting to improve the codes both in X Division where we do theoretical weapons design and in T [Theoretical] Division. We do interesting work. and I find it kind of sad that we can't tell everybody about it. Clearly we could do better if we could talk to people.

BAKER: Do you find it difficult to get rewards from your work because you can't talk with more people about what you do, can't publish results?

HOWE: In some sense your ideas are rewards in themselves. If they work, you know you have made a gain, perhaps even contributed to unclassified scientific efforts like inertial confinement fusion, which is also being studied in our division.

SCIENCE: *Is it difficult to pick up information you need because your problems are classified?*

HYMAN: I really think it is. It is frustrating on all sides not to be able to express an interesting scientific question in the context where it arises. You notice the difference at national physics meetings between the typical scientist and those working only on classified problems, The ones working on unclassified problems can go to the blackboard and describe everything in minute detail, get immediate feedback, and also know that people will go home and continue thinking about the problem. When people first come to X and T Divisions. they continue to go to physics conventions as they did before. But if they work only on classified problems, often within the first few years their attendance drops off very fast. Some just stop attending national meetings and interacting with the outside world.

At the Center for Nonlinear Studies [CNLS] we are trying to encourage interactions between the classified and unclassified research areas by organizing mixed workshops. In these workshops the first two or three days are unclassified and uncleared university scientists are encouraged to attend and speak. On the last day classified questions related to national security are addressed, and the attendance is limited. The last such conference was a joint X-Division/CNLS workshop in February on interface instabilities.

A problem we have not been able to overcome is that numerical results generated by a classified code are classified-even when the physics model, the data tables, and the numerics used in the run are unclassified. This restriction greatly inhibits interactions with computational physicists outside the Laboratory.

SCIENCE: *How open is the communication between T and X divisions ?*

HOWE: We rely heavily on our communication with T Division people.

HYMAN: Mostly it's between people you've worked with for years or know from the coffee machine. And the interchange is more limited now that the two divisions have been physically separated. We are trying to get more joint seminars so that we can indeed hear what people doing unclassified research learn in the outside world and then relate it to our needs.

HECKER: It is a poor substitute to have to depend on T Division for your information.

HOWE: It doesn't really work.

SCIENCE: *Is an effort being made to change the situation?*

HYMAN: Yes, there's been a change in the attitude of management.

In T Division we've always been very strongly encouraged to publish at least one paper, if not more, a year and to present at least one, if not more, at a national meeting. Some of the same emphasis is now appearing in X Division.

HOWE: We are getting more new people straight out of universities, and I think those who are new are interested in the national meetings. Getting back to our relationship with T Division, I would like to see us, as designers, integrate better with the work in T Division. For example, we really don't have a well-defined effort to do nuclear physics type research in the weapons physics business. We do our job for the military. They say, "We want this beast," and so we take what the codes can give us, and we design the creature. The T Division staff doesn't have this limitation and their work in nuclear physics is relevant to what we do in weapons.

BAKER: I know that some people in X Division work enthusiastically with the space groups. They have a number of large computer codes that they like to test on a variety of systems to see just how well the codes predict behavior. The magnetosphere is a large plasma system with magnetic fields; they like to try to model that. We do such modeling, too, and like to compare the results of our different codes.

SCIENCE: *I want to ask you about the young people in the weapons program. Are they there because the problems are interesting or because they have some feeling of commitment to the development of new weapons?*

HOWE: Many of them are in there because they did their theses in areas used in weapons research. Weapons development is such a multidisciplinary field; everything in the world is involved in making this thing go. Chemistry, physics, nuclear engineering, hydrodynamics—almost any field you name is involved. I would say people's motivations vary.

HYMAN: Many people have come into the weapons field because at one time they recognized that controlling fusion is one of the most important unsolved physics problems of the century. Much of the knowledge and data needed to crack the controlled fusion problem is classified. Once in the system, people find the weapons-related problems equally or even more fascinating and rewarding.

HECKER: Because of the strictures of classification, people rarely choose to come to the Laboratory to do materials research for the weapons program. People come here to do research in other areas and then wind up working on weapons problems because they are so interesting. We do have a corps of extremely dedicated people who build prototype hardware, develop our local shots, and design Nevada Test Site shots. But the tight ring of security really stops the flow of ideas from the outside in. Our metallurgists working on plutonium have been so strictly limited that we have tried to give them a cross section of other work, but an enormous amount of materials expertise remains outside the reach of the Laboratory.

SCIENCE: *A new Center for Materials Science has been created at the Laboratory, as well as the Center for Nonlinear Studies and a branch of the Institute for Geophysics and Planetary Physics. Are these centers aimed at alleviating the communications problem?*

HECKER: Yes. Don Kerr has recognized the overall problem. The Center for Materials Science has brought us in close contact with first-class materials science people outside the Laboratory.

HYMAN: The Center for Nonlinear Studies has had a similar impact. We sponsored over three hundred visitors last year. Besides the week-long conference each year, we have a number of workshops in areas we've chosen to target. One target this year is understanding the creation, stability, and evolution of patterns, fronts, and interfaces. There will also be workshops on cellular automata, implicit methods of differential equations, fracture mechanics, science underground, synthetic metals, and biopolymers. And what is even better than solving immediate problems is bringing together from the Laboratory, industry, and universities people on a one-to-one basis—establishing relationships that can continue for many, many years.

BAKER: In contrast, the Institute for Geophysics and Planetary Physics is directed toward interactions with professors and their students. We are a resource of the University of California in particular, and we now have a number of their graduate students working here for a year or two.

KOLB: This type of interaction not only helps us; it brings in people who then discover what is going on in the Laboratory. Half the people taking part in this discussion had their first contact with the Laboratory either as graduate students or as postdocs. Both the graduate student program and the postdoc program are really excellent ways for the Laboratory to recruit good people. I strongly believe it would be to the long-term benefit of the Laboratory to enlarge these programs and the visitor program as well.

ROCKWOOD: We should also work closely with the universities to make both students and faculty aware of the directions in applied science and the particular types of people that we see we are going to need. We can give universities access to such facilities as LAMPF, Antares, and Helios as research laboratories for their students; in return they may become more familiar with this Laboratory and be more responsive to our future needs.

HYMAN: In line with this thinking I should point out that the Graduate Research Assistant program is probably the most effective and least expensive of all of our advertising. But it's under-utilized, and I'd like to see it used more.

CRAWFORD: The closer our contact with graduate students, the better off we are, I think. It's a way of advertising the incredible potential and diversity of this place—some of it realized and some still untouched. It's difficult to overstate the importance of the Laboratory's diverse capabilities. I think there's a real need to keep

Laboratory Support for Basic Research

The Laboratory has always recognized the need to support a wide variety of basic research, and for most of the Laboratory's history, that research was funded entirely by the weapons program. During the 1970s, however, budgetary constraints made it increasingly difficult to maintain the level of so-called Weapons supporting Research, and in 1975 concern about its steady decrease prompted Harold Agnew to found the New Research Initiatives program as a supplement. However, despite the Laboratory's growth and widened spectrum of activities, Weapons supporting Research funds continued to be t.he dominant means of Laboratory support for basic research.

In fiscal year 1982 Donald Kerr combined and expanded the Weapons Supporting Research and New Research Initiatives programs with establishment of the Institutional supporting Research and Development program. This new program incorporated the following principles, many of which required new and extensive plans on the part of everyone involved.

- The program should be Laboratory wide and should include a broad spectrum of research and development related to all Laboratory programs,
- Projects should be consistent with and in support of the Laboratory's basic missions.
- Funds should be distributed according to a fair scheme that encourages competitive proposals and ensures optimum investment of resources.
- Support should be derived proportionately from all Laboratory programs,
- Accountability of funds should be reasonable and consistent with normal practice.

The ISRD program has definitely improved the manner in which discretionary research funds are allocated and the status of funded projects is reviewed, Considerable freedom is exercised by the Laboratory's associate directors in organizing and evaluating projects under their directorates. As is usual with any new program, some shortcomings have been recognized and some evolution is expected. It is evident that, in spite of the healthy challenge of submitting competitive proposals, there have been too many proposals and they have, for the most part, been too long. Paperwork is being reduced, arid a system of triennial, rather than annual, review is being developed for some projects.

The accompanying table lists the distribution of ISRD funds among various broad categories in fiscal year 1982.

DISTRIBUTION OF ISRD FUNDS IN FISCAL YEAR 1982

Research Category	Allocated Percentage of Total Funds
Materials Science and Chemistry	32%
Program Development and Applied Technology (Energy and Defense)	25%
Mathematics, Techniques, and Computer Modeling	13%
Nuclear Physics and Nuclear Chemistry	1 l%
Medium and High-Energy Physics	8%
Plasma Physics and Astrophysics	4%
Earth and Space Sciences	4%
Life Sciences	3%

not just students, but the whole country, informed about what we're doing and can do. One important example in life science research is the new DNA sequence data base being established in the Theoretical Division and funded by the National Institutes of Health. This will be a comprehensive computer-based library of DNA sequences designed specifically as a resource for scientists around the world who are doing recombinant DNA research. Eventually we may be able to produce a computer-based, electronic journal that bypasses conventional publication. Scientists could submit their DNA sequence data for review and receive results in recombinant DNA research electronically.

SCIENCE: *How do new projects such as the DNA sequence library get started?*

HOWE: First someone has to have an idea and that usually happens quite informally. We sit around and talk and suddenly some guy comes up with a neat idea.

COLGATE: 'That's right. Some of us don't know one another very intimately. but sooner or later we will meet, I will bump into John and start talking about cryogenic systems for fractional charge separation using superfluid liquid helium as a charge separation drift chamber.

ROCKWOOD: Once the idea is hatched, you might try it out with what is called bootlegging. You do the experiment or the calculations at your own discretion, but generally with the knowledge of the group leader, division leader, or whoever else is involved. If the idea shows real promise you may be funded through Institutional Supporting Research and Development [ISRD] money. This is the Laboratory's discretionary fund, It has traditionally been used for basic research,

but more recently it has also been used to fund new applied programs, I, for one, believe the applied programs should receive an equal share of this money. This is our investment in the programs of the future, and, in the final analysis, only programs pay the Laboratory's bills.

HECKER: The fact that this Laboratory has the foresight to take a meaningful fraction of its total income and plow it back as discretionary research is fantastic. At many other places the discretionary research money is more like one per cent. We do have an enormous opportunity for internal research. Of course, there has been a lot of upheaval recently about having to write proposals every year for ISRD money.

COLGATE: I think proposals are a darn good idea. I never did have to do them at Livermore. Then at the university I ended up having to write twelve a year. They are never easy, but they are really worth it.

BAKER: They do help people who didn't know what they were doing to think about their work a little more, but on the other side of that coin I think management can really be an obstacle.

COLGATE: Yes, if proposals are not reviewed correctly, you end up with a mess. Most proposals are now judged by the Laboratory management and the Senior Fellows, but this does not always constitute peer review.

HECKER: I agree that we do need more accountability than we used to have. However, one simply cannot set up an environment to do good basic research if proposals are required on a yearly basis. Also, the people making the decisions have become farther and farther away from the people who really know what is going on. I'd like the authority and the responsibility for research programs to rest with the divisions. By all means have an advisory panel of outside peer experts to judge the quality of the research, and if the results aren't good, then fire the division management.

BAKER: I've found that the handing out of Institutional Supporting Research money is based too much on historical factors rather than on quality of research. There is no competition in the true sense, that is, based on demonstrated scientific competence.

HECKER: That problem has been addressed to some extent. Two years ago six working groups were set up to look at areas that were not well represented traditionally, and I know that materials science has been receiving more support recently.

COLGATE: Perhaps the ultimate mechanism is, once again, the individuals. To my mind the Lab is put together of people who have an absurd sense of ego; that is, they have the drive and the motivation to back their own original ideas.

HYMAN: It's true that most projects have started with individuals who were aware that something was about ready to break. They went out and wrote proposals; they got up on their soapboxes; they sold their ideas and started small. Sometimes the ideas fizzled out, but other times they turned into whole divisions.

BAKER: Jerry, what reception do you find to suggestions being made by the Weapons Advanced Concepts people?

LANDT: Very good in general, but there are some people who resist change and don't like to see things at the Laboratory change.

HOWE: I find in the weapons program that you can have a wonderful idea either in software or in hardware, and, fine, they will help you develop it and make the best calculations possible. But then they fail to implement it. Furthermore, we are being urged to develop our own codes rather than just to borrow from Livermore. And in fact we do have several new ones, but I find there is some resistance to changing several hundred thousand lines of a code and putting in the new stuff. The same kind of reluctance appears in the hardware; it takes several years to get a materials idea implemented.

KOLB: Is that a management problem?

Rocky Kolb on Cosmology

"Cosmos" is the Greek word meaning order, and the basic goal of cosmology is to understand the universe on the basis of physical law. By applying physics to what we see in the universe, we endeavor to understand the structure of galaxies and the origin and large-scale structure of the universe.

Within the past five years or so some very interesting and very bold particle physics theories have been hypothesized. They model the physics of incredibly small scales-down to Planck's scale, which is about 10^{-33} centimeter. These theories are extrapolations, but there is some physical basis to them and they imply certain things about the universe. For example, they predict proton decay and the existence of magnetic monopoles. If these predictions are correct, then we now have models of the structure of matter under unbelievably extreme conditions of density and temperature, and we are in a position to study the very, very early universe. By the early universe we used to mean 1 minute or 1 second after the big bang. Now we can talk about 10^{-33} or 10^{-38} or 10^{-40} second because we believe we have a model of the underlying physics with which to do the astrophysics and cosmology.

Some practical questions we might answer are how many magnetic monopoles are expected to be around, what are their properties, and how would one look for them. Another possible insight is understanding the asymmetry of the universe in baryons-that is, why there aren't an equal number of baryons and antibaryons. Unfortunately the big bang is not an experiment that you would want to-or could-duplicate.

Study of the early universe leaves an interesting unanswered question: why the universe is so old, If you look at the Einstein equations that describe the evolution of the universe, the only

time scale that appears is the Planck time, which is about 10^{-45} second. It is rather hard to understand why today, ten billion years, or 10^{60} Planck times, after the big bang, the universe hasn't either recollapsed or expanded to an extent that the gravitational attraction of the matter is irrelevant in the expansion. Today we cannot determine whether the universe will expand forever or eventually recontract, since the kinetic energy of expansion is almost equal and opposite to the gravitational potential energy. This seems to imply that in the initial expansion the kinetic energy balanced the gravitational energy to something like one part in 1056—essentially a zero-energy system. This conundrum has a possible explanation if the universe underwent a strong first-order phase transition. An active field now is phase transitions in the early universe. This is a true interdisciplinary field, bringing in particle physics, general relativity, and statistical mechanics,

Our investigations may also have a number of reciprocal implications for particle physics. It has become fashionable every time a particle physics model is proposed to look for the astrophysical impact of it, You try to see whether the new model does things to the universe that you can't allow. For example, does it lead to too much mass density in the universe? Another example is monopole-catalyzed proton decay, Colgate and I have pointed out that such decay would have a terrible environmental impact on neutron stars. The work we have done leads us to believe that either monopoles do not catalyze proton decay or that monopoles don't exist, which would really be a shame because their existence would have enormous practical implications. ■

HYMAN: It is somewhat a management problem in that the codes have been allowed to grow unstructured for so many years that they have become the unmanageable things they are.

HOWE: It may be an external problem—one caused by whoever is using the weapons.

CRAWFORD: The external response to new ideas probably varies greatly from agency to agency. The Office of Health and Environmental Research, which oversees much of the research in the Life Sciences Division, is quite receptive to new programs.

COLGATE: Other offices of the DOE are also receptive. For example, Rocky has had ISRD support for some time doing far-out research in cosmology relating to conditions in the early universe. But what's really relevant is that last year the Office of High Energy and Nuclear Physics saw tit to pick up part of his funding. Nothing ventured, nothing gained!

SCIENCE: *With regard to external support for new ideas, the Laboratory is encouraging more interactions with industry. How will this affect the Laboratory?*

ROCKWOOD: I would say that a closer union of this Lab and industry would be mutually beneficial. The best single thing that has happened is that the DOE may now allow patent rights to remain with a funding company. Private industry can now put some money into a national lab without losing all rights to patents that emerge from the work. For instance, an industrial organization that wants to get involved in a new venture requiring a group of plasma physicists wouldn't have to hire twenty of their own while they got started. Instead, they could hire our expertise in that area to help them get started—a healthy collaboration.

WHEATLEY: I really think that is right.

ROCKWOOD: I see us starting to make some progress. We have money coming from Westinghouse to help look for a method of enriching certain isotopes that they are interested in as a company. They would have refused to invest this money in us a year ago,

BAKER: The hot dry rock project is a related example. Money is coming from a variety of sources, such as the Japanese government and the German government, as well as our own government.

SCIENCE: *We hire the people and they fund them?*

ROCKWOOD: They hire our people, if you will. They contract to us to do a specific task that saves industry from building up a highly specialized group of people they don't need for the long term.

HYMAN: The kind of basic research a lot of us do is oriented toward the very large problem with very limited applications. Take the supercomputers. There just aren't that many supercomputers out there. Most vendors can't afford to support the effort needed to develop new algorithms and software that push these computers to their limits. Yet it is quite appropriate for us to do that here.

HOWE: I can forsee that industry funding might compete with basic research for a person's time. Since it is near-term support, you are going to have managers saying, "AH right, we want you guys to work on this project for Westinghouse, and you have to put aside your basic research for now."

ROCKWOOD: I think rather that industry will be wanting to use basic research that we have already completed. But I won't say that conflicts will never arise. They'll have to be worked out.

CRAWFORD: If we become closely allied with both universities and private industry, perhaps we will be able to function more as a research and development organization—taking ideas from university programs and assigning teams of researchers well qualified to test the feasibility of such ideas—with the goal of technology transfer to private industry.

SCIENCE: *Gentlemen, it seems that our relationship with industry may undergo a change. What other changes would you like to see*

happen in the future? I know I'd like to hear about the proposal for an underground laboratory.

KOLB: Los Alamos has a proposal to build such a laboratory at the Nevada Test Site. It would be operated as a user facility, like LAMPF, and would make possible an entire class of very sensitive elementary particle experiments that require shielding from the normal above-ground radiation levels.

Los Alamos is a good laboratory for this facility because, first of all, we have strong groups in theoretical particle physics and in astrophysics. The interdisciplinary work of the facility would require a broad base in many areas of physics. We would aim to learn about neutrino oscillation and determine neutrino masses, topics that would have a large impact on our understanding of galaxy formation. We would have a chance to detect proton decay, which would go a long way toward telling us how much we understand about the origin of baryon symmetry. We could also learn many things about cosmic-ray physics and the large-scale structure of the universe. And a facility like that would generate technology in building detectors and in doing state-of-the-art experiments.

HOWE: I would like to see us expand in the space utilization business. We have a great deal of expertise in basic physics research and materials sciences, but we don't have much of a program for utilizing space.

HECKER: At the expense of the Jet Propulsion Laboratory?

HOWE: JPL is mostly involved in planetary exploration, and NASA is doing hardware development. Perhaps Los Alamos should begin programs to utilize the shuttle, to utilize the space station if it gets built.

BAKER: Those things are being considered, but so far the effort is fragmented.

WHEATLEY: There currently is an interesting cooperative program between the Center for Nonlinear Studies and the Center for Materials Sciences, having to do with conductive polymers. Wouldn't it be good to have such a program between the Institute for Geophysics and Planetary Physics and the Center for Materials Science on materials processing problems for space? We talked with a fellow from NASA who is in charge of their program for materials processing in space. That is really interesting physics—and chemistry and metallurgy and what you would call materials science.

HOWE: That is an important point. Probably the Weapons Advanced Concepts people are looking at orbital devices, but if someone comes up with an idea for an experiment to go on the shuttle, we have no one in the Laboratory who could translate the idea into shuttle-compatible hardware, as far as I know. NASA would have to be contacted. There is no given laboratory in the country to interface with industry and provide shuttle compatibility.

CRAWFORD: I'd like to see the Materials Science and Technology and the Electronics divisions combine research in their areas with the space program to develop alloys, circuits, etc. in space stations. It would be an ideal opportunity for cooperation with the private sector, and it could foster the rebirth of the space programs. It could place us at the forefront of university-industry cooperation with national laboratories.

HOWE: I'd also like to see us involved in the defense angle. The military consults with the Laboratory on a lot of concepts now, and we should have the capability of consulting in the area of space utilization.

LANDT: Interchange takes place along a number of avenues, but there are no hard and fast rules.

BAKER: We clearly have many of our eggs in the space basket for communication and for intelligence gathering, and our reliance on space is likely to grow. It is certainly something the Laboratory is interested in.

HOWE: The Air Force recently created the Space Technology Center in Albuquerque. We could have a good interaction with that phase of the military, and it would be an ideal way for the Laboratory to get involved in the space program.

SCIENCE: ***Are there any other similar areas? How about computer science in terms of the future?***

HYMAN: The way that the inside of a computer works is going to change completely in the next few years, and unless we rethink how to write programs, we won't fully exploit the potential power of the new machines. Some people saw this years ago and asked that we prepare new algorithms ***before*** the machines arrived. Slowly the proposals went through the Laboratory and through Washington. Now, finally, we have a viable research group in the Computing Division developing new methods for machines not yet built.

There are two similar computer projects still at the proposal stage that come to mind. The first is a CAD/CAM [computer-aided design/computer-aided manufacturing] effort to model three-dimensional surfaces on the computer with a very interactive user interface. The second project is in artificial intelligence and would have many applications within the Laboratory, from providing a reliable friendly user interface for our complex computer network to applications in nuclear safeguards.

The proposal to form an artificial intelligence group at Los Alamos surfaced about a year ago, and by now it is well polished and dog-eared at the corners. A group of about thirty of our scientists meet regularly and sponsor classes and talks from visiting and Laboratory experts.

Just how speculative do you want me to be about future scientific computing?

SCIENCE: *Go ahead, speculate.*

HYMAN: All the major physics codes at this Laboratory have many similar components. At the lowest level, they use trigonometric functions—sines, cosines, and tangents. In the early days of computing, everyone had his own favorite procedure for these elementary functions, but gradually the better ones were included in the mathematics program library. In the '60s and '70s higher level routines for solving linear systems of equations, integrating ordinary differential equations, handling one-dimensional interpolation, and other moderately complicated procedures were developed and included in the computer library. But then in the late '70s the trend slowed down and in some cases stopped. Right now we have no appreciable effort developing the next generation of mathematics support software. If such a group existed, it would be writing even higher level routines: multidimensional interpolation and differentiation programs, grid generation and adaptive mesh routines that adjust the solution algorithm to the boundary of the problem and the structure of the solution, routines to help solve large systems of sparse nonlinear equations, and routines to incorporate the boundary conditions into a discrete approximation of the physics model,

For this new software to be successful, it must be compatible with existing techniques and be simple enough that in a trial run potential users can observe tangibly better results than with existing methods. The software packages that are most readily accepted are those that behave like the existing ones—only work better.

Industries and most universities that develop new software are too far removed from the production code programmers to interact with them and obtain the essential feedback. Also, the production codes are run on the most powerful computers available and those writing the software must have access to these machines. This means that we at the national computing centers should be writing the next generation of high-level mathematics support routines to be used in our production codes. At the same time we really should be getting together more with the scientists in industry and universities who are writing mathematics software. This means having a much more active visitor program in math software development and providing easy, long-distance access to our supercomputers.

CRAWFORD: I agree that we should forge ahead in our computer work, both the hardware and the software. Our national security will depend partly on our ability to lead the supercomputer field.

HYMAN: We need a coordinated effort like Japan's. Japan already dominates in applying robotics in industry. Through its Ministry of International Trade and Industry, it has identified other projects it plans to complete by 1990. One project is a high-speed computer whose capability is at least ten times that of the Cray-1. Another is a fifth-generation computer that will implement artificial intelligence—the number of inferences per second would be a hundred to a thousand times current technology. Losing our technological edge in these areas would have serious repercussions on both our economic and our national security.

CRAWFORD: I would like to insert another note of warning. Recombinant DNA techniques are ridiculously simple to master. The United States could suffer from foreign nations or even terrorist groups employing biological or chemical weapons. Our Laboratory is an ideal place—we have both physical isolation and classified research ability—to establish a defense program against such agents. Biological and chemical agents can and will be used by those with a cause, however ill conceived. Countermeasures like specific antitoxins are within reach of our present capability. The nation should move forward in preparing these defenses.

LANDT: To close this discussion, I would like to spend a minute or two talking about future defense. Historically this Lab has developed the nuclear side, but now we should try to get people to think about the other side, the nonnuclear. There is an antinuclear movement in this country and the world. Advances in electronics are going to permit some conventional munitions to have the same military impact as nuclear ones, and we should take advantage of that. These are some of the things the Weapons Advanced Concepts people are thinking about.

ROCKWOOD: I also believe the Laboratory should be expanding into nonnuclear weapons for defense. It appears that the nuclear age has, if you will, made the world "safe" for conventional warfare. Conflicts such as the kind in Vietnam, the Falkland Islands, and the Middle East seem those most likely to occur, and the ever-increasing role of high-technology weapons in those conflicts is a matter of which we must be cognizant. We are a nation that aspires to defend itself not by massive uses of people, but as much as possible by the use of high technology—and that means us here at Los Alamos. ■

The Participants

DAN BAKER: I got my Ph.D. at the University of Iowa with Jim Van Allen in 1974 and then went to Caltech as a Research Fellow in the Physics Division, While there I collaborated over a period of a couple of years with people from Los Alamos. In 1977 I came to Los Alamos for a job interview and was impressed with the interests and abilities of the people I encountered. I decided to join a group in the Physics Division involved in high-altitude physics, where I then worked for two or three years on satellite instrumentation and data interpretation. Since October of '81 I've been Leader of the Space Plasma Physics Group in the Earth and Space Sciences Division, which, I might add, is better known simply as Heaven and Earth Division.

STIRLING COLGATE: I came to Los Alamos primarily because the then Director of the Laboratory, Harold Agnew, and the then Leader of the Theoretical Division, Peter Carruthers, persuaded me to come. I had been a staff physicist at Lawrence Livermore Laboratory for twelve years and then President of New Mexico Tech for ten years. I realized that the type of research I knew best would utilize the facilities of a major national laboratory. My work in inertial fusion continues, and the ability to do astrophysics, atmospheric research, and tectonic engineering in an environment where my advice is respected and my research work is encouraged is a privilege beyond measure. In addition, becoming recognized as a theoretical physicist after initially being an engineer in the Merchant Marine and then being an experimental physicist for many years is a very great privilege, indeed. Explosions turn me on—from firecrackers to testing nuclear bombs at Eniwetok, from using the Lab's codes to calculate supernova explosions to preventing volcanic ones. Our universe started with an explosion, is tilled with explosions, and by far the most extraordinary and singular one is the explosion of intelligent life.

BRIAN CRAWFORD: I was actively recruited by the Laboratory while I was completing work for my Ph.D. at Johns Hopkins University. The Genetics Group of the Life Sciences Division needed someone to investigate the basic mechanisms by which ionizing radiation, chemicals, or other agents cause gene mutation and/or malignant transformation in cells. I had the specific skills required because my thesis had involved study of the genetic mechanisms of chemical carcinogenesis. I was encouraged to apply for one of the Laboratory's Oppenheimer Fellowships, which I received in time to begin work in the summer of 1981. Since I came, I have been applying recombinant DNA methods to research on the genetic events underlying carcinogenesis. What attracts me to this Lab are its advanced facilities and, above all, its cooperative atmosphere—theoreticians are working closely with biophysicists and biochemists in very sophisticated studies.

SIG HECKER: I grew up in Austria but moved to Cleveland when I was thirteen. Indeed, I had never been west of Toledo until I came here as a summer graduate student in 1965. My visit was brought about by a gentleman from the Laboratory's recruiting office who showed me a brochure containing lovely photos of New Mexico mountains. Once here I liked the marriage of basic science and applied technology at the Laboratory. After receiving my Ph.D. from Case Institute of Technology, now Case Western Reserve University, I returned to Los Alamos as a postdoc in 1968, attracted by the excellent funding and the chance to do basic research in metal deformation. In 1973 I came as a staff member after three years in the Physics Department of General Motors. I've worked ever since in materials science, principally in plutonium metallurgy and in actinides, although I've worked on a number of projects related to the space power and basic energy programs. Two years ago I joined the Division Office of what is now the Materials Science and Technology Division.

STEVEN HOWE: I'm another of those students who keep turning up. I started coming here as a summer student in 1975 and did that for the next two years. Then in January '78 I came to do my thesis research at the Weapons Neutron Research Facility at LAMPF. After receiving my degree from Kansas State University, I spent a year at Kernforschung Zentrum in Karlsruhe and then returned as a staff member in September '81. I'm in the Thermonuclear Applications Group in the Applied Theoretical Physics Division.

JAMES (MAC) HYMAN: I was indirectly introduced to Livermore and Los Alamos at the same time. I was interviewed for my graduate fellowship, a Hertz Fellowship, by someone from Livermore, and he asked, "What are you doing this summer?" I worked that summer at Livermore, and it was the first time I saw mathematicians and physicists working in close coordination with experimentalists. It was just great—except the temperature was 115 degrees. My boss at Livermore had been here during the war, and he said, "Where you really want to go next is Los Alamos." So I did, and it evolved into a full-time job after I got my degree from the Courant Institute. I work on numerical methods and software for large systems of differential equations, equations that model the physics experiments. It's partly physics, partly computer science, and mostly mathematics.

EDWARD (ROCKY) KOLB: I received my Ph.D. at the University of Texas in '78. I interviewed here for a postdoc position, but I went to Caltech instead. Then I came here as an Oppenheimer Fellow rather than going to a university, because here I could spend 100 per cent of my time doing research rather than teaching and sitting on committees. I was attracted by the people I would have a chance to work with. It was really the people who brought me here. I did my Ph.D. in elementary particle theory, and now I'm into cosmology and astrophysics, high-energy astrophysics. I'm in the Theoretical Astrophysics Group and I work closely with the Elementary Particles and Field Theory Group, an overlap that's possible here for someone not in a traditional discipline. At universities people seem more locked into compartments: there's one person in nuclear physics, one person in atomic physics, and so forth, and it's not easy to move into new fields. Here at Los Alamos you can move quickly into exciting fields as they open up.

JEREMY LANDT: The country in the western part of my home state of South Dakota is very much like the country here, so perhaps that was a factor in my initial attraction to Los Alamos. I came here in 1967 as a summer graduate student and liked the facilities and the people. When I completed my research work at Stanford, there weren't too many jobs available at Los Alamos in the areas I had studied—radiopropagation, electromagnetic theory, and that kind of thing. But there were at Livermore, so I spent a few very enjoyable

years there. But I got tired of all the people and the hassle, and when something opened up here, I applied and came back in 1975. Except for the past year, my stint here has been spent in the Electronics Division. I have worked on electronic identification systems, EMP calculations, application of radar and other electronic techniques to mapping underground fractures for the hot dry rock project, plus a little nuclear magnetic resonance work, so I have dabbled in this and that. At present I'm working in the Weapons Advanced Concepts Program Office. We're supposed to be looking at wonderful new things; we're finding lots of wonderful old things that other people have thought of.

STEVE ROCKWOOD: After finishing my doctorate at Caltech in 1969, I went into the Air Force as my obligation to the country during the Vietnam era and spent two years at the Air Force Weapons Lab. There I got into laser activities, a field entirely different from my graduate work. I came to Los Alamos in 1972 principally because the laser programs then being started at the Laboratory and the people here were stimulating, It is an exciting area to work in. A secondary consideration would have to be the New Mexico environment. My own personal way of working has been to change fields frequently, although always within physics. I started out at the Laboratory as a theorist in T Division and then became part of the fledgling isotope separation program and was Leader of the Laser Development Group until 1980. Then I took over my present job as Deputy Associate Director for Inertial Fusion. To me the main attraction of the Laboratory, in contrast to universities, is its ability to pull together the resources to do a large multidisciplinary program and move on it quickly.

JOHN WHEATLEY: I received my doctorate from the University of Pittsburgh in 1952 and came here just recently, after stints at the University of Illinois and the University of California, San Diego, because I saw the opportunity to do both the basic physics research that is my main line of work and also what I call fundamental technology. That combination is highly regarded here, while in my previous university careers I always felt I had to sneak my interest in technology in the back door. After all, instruction through basic research, not development of technology, is the principal function of a university. Also, I perceive a very substantial increase in my effective mass here because the Lab has many more people interested in my field, which is thermal physics and condensed-matter physics.

LANDT: Certainly the Laboratory is aware of its obligation to help the country defend itself and to maintain a balance of technologies. Right now I am assigned to the Weapons Advanced Concepts Program Office, which was begun a year ago to try in a practical way to determine which technologies really make a difference for the national defense so that the country won't throw its money away on the wrong things. The Laboratory management is very interested in addressing this issue, and they have put dollars behind it and people to work on it.

ROCKWOOD: Today the government's method of doing business is very much applied and mission-oriented. Although basic research is also essential to our national security mission, it is often overlooked, and the national laboratories are handcuffed in this area by administrative limitations. People here have to be clever in extracting from their mission-oriented programs good basic results in science. I think Los Alamos has been rather successful at that.

WHEATLEY: Do you think mission orientation is a good thing? As a matter of principle?

ROCKWOOD: Moderation in all things.

BAKER: I think we must tight this trend toward applied work only, toward everything having an immediate payoff. A national laboratory should play as active a role in basic research as any laboratory. The country will suffer in the long run if we don't.

ROCKWOOD: Often the most exciting and fundamentally useful part of a program is not its stated objective but some unplanned spin-off. In the laser isotope separation program, spectroscopists working to explain the spectrum of the octahedral molecule UF_6 discovered that the octahedral symmetry group had originally been analyzed incorrectly and had been wrong in the literature for years. Even a very applied program may yield results of use to basic science.

BAKER: That's certainly been true in space physics. The Vela satellite program to detect nuclear explosions deep in space was a mission-oriented project, and we continue to have test and verification activities. To accomplish that practical goal we had to place instrumentation on the spacecraft to measure the environment. As a result, many properties of the magnetosphere were discovered.

Now the space physics groups are involved in a number of activities on collisionless shock waves, cosmic particle acceleration, the interplay between the solar wind and the earth's magnetic field, and the exploration by the International Sun-Earth Explorer 3 satellite of the night side of the earth.

SCIENCE: *How do you get funds for all these activities?*

BAKER: In a variety of ways. We have been able to obtain reimbursable funding from NASA [National Aeronautics and Space Administration] for some of our projects. But the continuing money from the weapons program gives us more stability than we could ever obtain from reimbursable funding alone. When we get our funding from the DOE [Department of Energy] or from the Laboratory, we are better able to make long-range plans. It's fortunate for us that the Europeans are also participating in many of our scientific satellite programs because the European Space Agency plans much further ahead than NASA does.

HYMAN: There are some problems with diversified funding. The Mathematical Modeling and Analysis Group in the Theoretical Division is almost completely basic research, and we also have been obtaining some support from outside the Laboratory. The largest block grant we have supports only one and one-half staff members. Because our funding comes in such little pieces, we are perpetual job hunters and odd jobbers—always knocking on a different door.

ROCKWOOD: The country hasn't learned how to fund basic science at all. Research doesn't integrate with time. Each administration

Dan Baker on Space Science

The Vela satellite program to detect nuclear explosions in space has led scientists at Los Alamos to satellite exploration of the magnetosphere and of a wide variety of other space phenomena. Some of the instruments aboard such spacecraft have been designed to measure the interplanetary medium and planetary bow shocks, and we are doing theoretical studies in support of these observations. A related study is our work on cosmic particle acceleration. The information about energization of particles at interplanetary shocks may have applicability to shocks of much more cosmic proportions, such as those presumed to exist in supernova remnants.

We are also exploring the interplay between the solar wind [the hot, expanding corona of the sun] and the magnetic field of the earth. This interplay produces the magnetic structure we call the magnetosphere, the tenuous plasma region that makes up the uppermost part of the earth's atmosphere. We are doing computer modeling of the entire magnetosphere and, furthermore, are developing computer network links to many other institutions involved in similar work.

In a more practical vein we are using our advancing technology to do experiments in which we release chemical tracers into the ionsphere or even deeper into the magnetosphere to learn in what way these additives may modify the outer parts of the earth's environment.

Still another project is attempting to use an existing satelite in a different and innovative way. The International Sun-Earth Explorer 3 [ISEE-3] spacecraft has been orbiting at the L-I

point on the sunward side of the earth for about four years. The L-1 point, the sun-earth Lagrangian point, can be thought of as an imaginary center of mass around which the satellite has been traveling in a large looping orbit. Now this satellite has been moved into the earth's distant magnetotail and is orbiting well downstream on the night side of the earth. It will be the first spacecraft to explore that region in space. To accomplish the move, the satellite's gas-jet thruster, which ordinarily performs minor station-keeping orbital adjustments, was used to move the craft in such a way that it encountered the moon's gravitational pull and got a lunar gravitational assist to kick it deep into the magnetotail. It is not in a stationary orbit, and thus the lunar encounters must occur every one to three months in order to keep the satellite deep in the magnetotail. Eventually another lunar push will occur, and ISEE-3 will go on to intercept a comet. This will be the first time that any spacecraft has gotten close to a cometary body.

Bob Farquhar, a very creative guy at NASA who seemingly can move any satellite anywhere you want using any other celestial object, helped with the ISEE-3 project and has also helped to plan what is called the International Solar-Polar Mission. Because we don't have enough energy in most launch vehicles to get significantly out of the ecliptic plane [the plane of the earth's orbit], we are sending a satellite out to Jupiter to get a large gravitational kick from that massive planet. The spacecraft will then move above the ecliptic plane and travel high over the sun's pole, another previously unexplored region. ●

comes in and has a new policy. Basic science suffers more from these oscillations than it would from a low level of sustained funding. And I believe Los Alamos suffers more from funding oscillations and changes in direction than other national laboratories. Our normal attrition rate is about 4 per cent per year. Any change in direction by more than that amount involves moving people around. People's skills are not always totally applicable to a different program, and those who are not absorbed by other parts of the Laboratory are not absorbed by the town at all. It is this very closed environment, which drastically constrains our flexibility, that I see as a major problem for the Laboratory. It always has been so.

Returning to the question of the funding of basic research, I feel that, although the government can't just pour out money and expect nothing in return except good intentions, the funding "pendulum" has swung too far toward applied activities.

WHEATLEY: Some of you would say that Los Alamos ought as a matter of principle to devote some fraction of its work to purely unqualified basic science, the sole motive being to understand things better and to develop knowledge or whatever-to have fun, really. I would like to suggest that perhaps that's not true. Perhaps it is our responsibility to articulate the possible relationship between our work and some appropriate mission of this Laboratory. I am not thinking of explicit applications, necessarily. Let me give you a personal example. I think that it is appropriate that my work in thermal and condensed-matter physics should feed into thermal technology. broadly defined, that is to say, into technologies that involve the concepts of energy, work, heat, temperature, and so on.

Right now I am working on heat engines. I had set myself a semipractical problem that no one in industry would define as practical of course—but it was. It had to do with producing cold very simply. I had an idea for doing that with acoustics, so I started playing around with the idea, developing it, and soon—meaning one year later—I found that what I was doing seemed to me to have very broad implications. Now I have put possible applications off to one side, and I am looking strictly at the basic science. at the fundamentals of it. I think I have identified what I regard as a new principle applying to heat engines in a very general sense. I do feel a responsibility ultimately to be able to draw a connection between the basic scientific work I do and some technology.

KOLB: I don't feel that way at all, There is a real necessity for nonmission. For fifteen years people have been looking at magnetic monopoles, intensively, just for pleasure, and for the past five or six years have been studying grand unified gauge theories—same motivation. Recently, Rubakov in Russia and Callan at Princeton have proposed that monopoles can catalyze proton decay, can just completely convert the rest mass of protons into energy. It will be another five years before it's worked out. Now something like that would have a tremendous payoff. would be comparable to Otto Hahn's discovery of fission. But it never could happen in a mission-oriented environment. No one told these people they should study monopole structure because it might have important applications. And no government agency has told me I should be studying them, either.

WHEATLEY: I'm not waiting to be told what I should do, either. For instance, I would feel perfectly tine studying spin-polarized hydrogen, a project in which 1 am very interested. Nor can I tell you what gadget that might be used in, but I do see that it is part of the foundation for thermal physics and that we ought to understand it.

KOLB: I don't choose research projects by wondering if they will have any impact on technology.

BAKER: Aren't you thinking of beam weapons systems using

monopoles?

KOLB: If I think about it, it is only after doing the basic science.

HOWE: Is it necessarily the basic researcher's responsibility to come UP with the utility of it? There are, perhaps. other people who are more interested in the engineering side, so they take the proton-monopole catalysis concept that Rocky mentioned and say. "Well, let's develop starship drives: let's design power reactors!"

SCIENCE: *Rocky, how do you choose your research projects? You 've said how you don 't choose them.*

KOLB: I don't know, actually, I don't know what I am going to be doing tomorrow or when I go back to my office. I read the literature and see what other people are doing. This communication is very important. I follow the direction the work is going.

HYMAN: You may recognize a problem as being important, but in the end the choice is subjective. A question gets under your skin, and you can't let loose until you understand it. That's the driving force behind science—the need to understand. As far as Rocky's responsibility to the Laboratory, that has become clear as he's talked, His obligation is to push back the frontiers of basic science—that's his job description. At the same time every scientist has a responsibility to the overall health of the Lab. Whenever you discover something that could be applied in a programmatic effort, you go down the hall, knock on doors, and make sure the right people know about what you have done.

KOLB: When I first read about Callan and Rubakov's work on monopole-catalyzed proton decay, I was at Aspen, and I said, "Well, I have to get back to Los Alamos and tell people about this." but then Stirling and I decided it couldn't work, so I didn't go knocking on doors.

WHEATLEY: Coming back to the missions of the Laboratory, I understand why we should be doing some basic science and much fundamental technology, that is. research on problems whose ultimate objectives are fully seen. However, my own view concerning applied work and hardware is that if you have a particular, well-defined job to do, the private sector would probably do it better.

HECKER: I would disagree, John. The weapons mission is a specific job, and we have done it very well.

WHEATLEY: The weapons case is rather special because of the national security problem. Suppose that you took the secrecy requirements away.

BAKER: In fact, private industry does secret work, builds all the components. We provide the overall science and technology. I don't think secrecy is the defining factor, The national laboratories are most effective doing both the theory and the design development of jobs that are high risk and from which an industry couldn't expect a profit in a short term. Fusion is another example.

HYMAN: Our exceptional facilities also give us an edge over industry. The two thousand scientists at Los Alamos comprise a pool

Sig Hecker on Materials Science

Materials are the *sine qua non* for new technology. At Los Alamos we have been in the business of processing new materials for technological needs from the very beginning. Now materials processing is becoming more sophisticated as we learn to exploit our understanding of materials on an atomic level. Our work on rapid solidification and ceramic processing exemplifies this trend.

So-called rapid-solidification-rate materials are made by cooling the liquid state very rapidly, on the order of a million degrees per second. The rapid solidification avoids equilibrium decomposition and consequently affords the opportunity to create materials with new and novel structures, For example, if you smash a liquid metal between an anvil and hammer or spin it against a cooled, rotating wheel, you can create a metallic glass, that is, an amorphous metal rather than a metal with the normal polycrystalline structure. Properties of metals depend critically on their crystal structure, or, more specifically, on the defects in the crystal structure. By creating an amorphous metal, we eliminate grain boundaries, which contain many defects and are therefore places where corrosion begins. Consequently, these metallic glasses have good corrosion resistance as well as high strength. Our rapid solidification work at Los Alamos has been applied mostly to processing actinides.

Our work in ceramics processing is aimed at a new class of structural materials for high-temperature environments, such as those involved in fuel processing and power generation. For example, a ceramic turbine might be used to achieve higher operating temperatures and higher efficiencies.

State-of-the-art work is being done in two areas: processing of dense ceramics without densification additives and growth of ceramic whiskers, The ceramics of greatest interest to us, silicon carbide and silicon nitride, must be made at relatively low temperatures to avoid decomposition. A densification additive forms a glass phase between the powder particles and essentially glues the particles together. Unfortunately, during high-temperature service, in a turbine for example, the glue turns glassy and

the ceramic loses strength. To eliminate the need for an additive, we have developed a technique for making an extremely fine, extremely reactive powder that shows great promise of densifying at low temperatures. We form the fine powder particles, which have diameters on the order of hundreds of angstroms, by a plasma-assisted chemical vapor-deposition process. In this process the constituents, such as silicon and carbon, are carried by appropriate gases and are reacted in a hot argon plasma. We are also using the Laboratory's expertise in shock loading to activate ceramic powder containing larger diameter particles. The idea is to produce a large concentration of defects on the surface of the particles before attempting to consolidate them.

Ceramic whiskers, a field in which we are the world leader, are long, single-crystal fibers of, for example silicon carbide or silicon nitride, with diameters that vary from less than a micron to maybe ten microns. These single crystals are grown by a process called the vapor-liquid-solid process. They are essentially defect free and have enormous strengths, from ten to fifty times that of structural steel. We are now trying to incorporate the whiskers into a composite material-a glass matrix, a ceramic matrix, or a glass-ceramic composite—to make high-temperature materials. Essentially, we are using processing science to control the strength and the ductility of materials on a microstructural level.

Another area that is not new, but extremely fascinating, is the actinides. In the last few years a marriage of condensed-matter physics, chemistry, and metallurgy has helped us to understand the intriguing electronic and magnetic properties of these elements and, in particular, how they determine the macroscopic properties of plutonium, uranium, and americium. For plutonium, especially, the only way to understand it is to understand the role of its bonding *f* electrons. For example, because the *f*-electron wave functions possess odd symmetry, bonding of these electrons favors unusual crystal structures with low symmetry. People in academic circles are now becoming very interested in the actinides because they offer new physics. ■

of knowledge found in only a very few places. Also we have five Crays and a complete set of shops.

WHEATLEY: We do have a complete set of shops, but it costs fifty-five dollars an hour to use them.

HYMAN: But they are at our disposal.

COLGATE: Just for a moment let me reduce the main missions and the main capability of this Laboratory to plain terms. Suppose we didn't have a Laboratory. Why would Congress, the politicians, want to start one? The only reason would be because they were scared: scared of losing the country—that's our national security mission—or scared of losing our way of life and our power—that's the energy mission. Fear for the future motivates the existence of this Laboratory. Politicians would never fund science from purely altruistic motives, and purely educational business would be in the universities where it belongs. But how do you make sure that a new idea doesn't come up to bite you from the rear, as Sputnik did? You have the most brilliant people around to think up all the new ideas that are possible before someone else thinks of them. So the basic capability of this Laboratory is its brilliant individual scientists. If someone wants you to come to the Laboratory, why do you accept? Because people here are doing the most exciting research in your field, and because you believe in your own ability,

ROCKWOOD: There's something I worry about, and I'd like to mention it here. At moments of international crisis, programs for the national laboratories are easily defined. But during periods of uncertainty about the future, and especially during periods of economic stress, the selection of programs is not so simply made. One of the strengths of Lo's Alamos internally is its great freedom of thought—freedom to disagree, to discuss openly with management the pros and cons of particular technical endeavors. It makes us stronger to have had these discussions and to look at all sides of a problem before going into it. But we should speak with only one voice to the external world. We don't need two, three, half a dozen people showing up in the same office in Washington, each with a different opinion as to which major programs the Laboratory should be pursuing.

SCIENCE: *While you more or less agree that the development of high technology for national security is the Los Alamos mission, the specific emphases and manner of carrying it out remain open to discussion. Perhaps we should turn now to some of the specific areas of research and development that are clearly important. Carson Mark has commented that many of the problems in technology development are materials problems. Sig, would you tell us what is being done at Los A lames in this area?*

HECKER: Our materials science effort demonstrates the exciting and productive relationship that exists between theory and experiment. It is one of the beauties of this Laboratory that metallurgists, physicists, and chemists work side by side. Our main interest in materials

processing, without question, has always come from the weapons program. Weapons designers, be they physicists or engineers, come to us with requests that to them seem exceedingly simple and to us almost impossible, at least at first glance. For example, the physicists wouldn't hestitate to ask us for structural air, that is, something with no density but enormous strength. Faced with sophisticated problems for years and years, we've learned how to tailor-make many special materials.

We have also done some basic research in materials science, and in the past few years we have begun to apply our understanding of materials on an atomic level to materials processing. One example is rapid-solidification-rate technology to make amorphous metals with high strength and good corrosion resistance, Another is ceramics processing; we are attempting to make materials for high-temperature environments, such as composites containing single-crystal ceramic whiskers.

LANDT: Electronics is another field that combines ideas and applications; it's partly software and partly hardware, and it's a crucial part of future technologies. I would like to put before you a statement by Dr. DeLauer, Undersecretary for the Department of Defense. Dr. DeLauer insists that electronics is the most critical of all technologies for the maintenance of peace, and he claims that "Further development of the electronics technology base of the United States is as important to defense today as the atomic bomb in World War II."* I think it's time the Laboratory took its electronics seriously.

BAKER: There are, however, a lot of good electronics firms.

LANDT: We are working on several projects that could make significant contributions in electronics—areas that private industry is not touching. These include high-speed electro-optic switches and thermionic integrated circuits that have important military as well as commerical potential. We are also developing high-power microwaves from lasers. This is research that could not be done without the exceptional computer and experimental facilities at Los Alamos.

SCIENCE: *Since we have mentioned speaking freely, I'd like to ask Steven whether there's anything he can tell us about weapons design work.*

HOWE: Most of what we do is classified, but I can say that we work to get better codes, better computational abilities to describe the processes in the weapon, to put in the things we do know so that the things we have to extrapolate can be better estimated. In the year I have been here we have come up with several interesting pursuits. One is in low-energy nuclear physics: there is a process that we think exists in the weapon but that we don't account for in the codes. This particular development is interesting because we have shared it with Livermore, and we have collaborated with them in getting it into the codes and making estimates. We also do secondary design work on weapons materials, attempting to understand basic processes. Generally we aim to satisfy the military requests and to come up with smaller, more efficient devices. We are continually looking at new

Jeremy Landt on Electronics

Our heavy reliance on the world of electronics has led Los Alamos into several fledgling projects that show great promise for the future. One is the development of the high-speed electro-optic switch, which can be used to probe integrated circuits with pulse widths of 50 picosecond or less. Understanding of semiconductor physics on these short time frames is essential for development of reliable, very high-speed integrated circuits for future weapons systems. The first generation of very high-speed integrated circuits is largely based on extrapolations of existing technology. To go beyond will require new technologies and understanding that industry does not have at present.

Another device under development is the thermionic integrated circuit, which is inherently hardened to radiation and EMP phenomena. Before research on this device began at Los Alamos an attempt to commercialize the technology failed because the basic physics was not understood. We could use this device to instrument nuclear and geothermal systems, as well as in military applications.

The area I find most exciting, however, is the broad area of high-power microwaves. We are working on novel generation mechanisms as well as novel applications. One new generation scheme involves the Helios laser, the "Laboratory's high-power carbon dioxide laser. Large numbers of hot electrons are generated in high-power laser targets. A carbon dioxide laser produces far more hot electrons than do lasers operating at shorter wavelengths. We are presently investigating ways of converting these electrons to high-power microwaves. The power levels achieved to date are very impressive and probably can be emproved much more. At present this research cannot be done anywhere else in the world. Los Alamos has both the computer codes to handle the flow of particles in electronagnetic fields and the experimental facilities to benchmark the codes. ■

"Richard D. DeLauer, "The Force Multiplier, " **IEEE Spectrum,** *October 1982, p. 37.*

Brian Crawford on Life Sciences

Several exciting things are happening in life sciences. We are using laser-based flow cytometric methods to separate chromosomes from mammalian including human, genomes. DNA from these isolated chromosomes can be cloned by recombinant DNA methods, allowing studies of the basic structure and functional organization of the chromosome. Los Alamos is one of perhaps three labs with the requisite expertise in biophysics and molecular biology to perform this work, and recent NIH [National Institutes of Health] funding to establish a Flow Cytometry National Resource is fostering progress in this area.

We are also working on cellular oncogenes. These genes are thought to control the evolution of the normal cell toward malignant change. The isolation, that is, the cloning, of such genes by recombinant DNA methods and the reinsertion of these genes into normal cells, by a process known as DNA-mediated gene transfer, permit us to study how specific oncogene expression can result in cancerous change. We are also studying the role that gene rearrangement, which can result for example from chromosome damage, can play in the initiation and progression of cancer. This work relates to DOE concerns regarding the effects of both ionizing radiation and the by-products of fossil-fuel development and consumption.

Another exciting development is the establishment of an NIH-funded DNA sequence database in the Theoretical Division. Sequencing, or decoding, of the genetic code in cloned fragments of DNA is meaningful only if such information can be stored, retrieved readily, and analyzed. Just consider for a moment that each mammalian organism expresses on the order of fifteen thousand distinct genes in a cell—not to mention that each cell has DNA encoding for an amount of unexpressed information hat is several orders of magnitude greater. Software development for the analysis of the stored sequences will be pursued oncomitantly with this Herculean bookkeeping effort. ■

things and attempting to improve the codes both in X Division where we do theoretical weapons design and in T [Theoretical] Division. We do interesting work. and I find it kind of sad that we can't tell everybody about it. Clearly we could do better if we could talk to people.

BAKER: Do you find it difficult to get rewards from your work because you can't talk with more people about what you do, can't publish results?

HOWE: In some sense your ideas are rewards in themselves. If they work, you know you have made a gain, perhaps even contributed to unclassified scientific efforts like inertial confinement fusion, which is also being studied in our division.

SCIENCE: *Is it difficult to pick up information you need because your problems are classified?*

HYMAN: I really think it is. It is frustrating on all sides not to be able to express an interesting scientific question in the context where it arises. You notice the difference at national physics meetings between the typical scientist and those working only on classified problems, The ones working on unclassified problems can go to the blackboard and describe everything in minute detail, get immediate feedback, and also know that people will go home and continue thinking about the problem. When people first come to X and T Divisions. they continue to go to physics conventions as they did before. But if they work only on classified problems, often within the first few years their attendance drops off very fast. Some just stop attending national meetings and interacting with the outside world.

At the Center for Nonlinear Studies [CNLS] we are trying to encourage interactions between the classified and unclassified research areas by organizing mixed workshops. In these workshops the first two or three days are unclassified and uncleared university scientists are encouraged to attend and speak. On the last day classified questions related to national security are addressed, and the attendance is limited. The last such conference was a joint X-Division/CNLS workshop in February on interface instabilities.

A problem we have not been able to overcome is that numerical results generated by a classified code are classified-even when the physics model, the data tables, and the numerics used in the run are unclassified. This restriction greatly inhibits interactions with computational physicists outside the Laboratory.

SCIENCE: *How open is the communication between T and X divisions ?*

HOWE: We rely heavily on our communication with T Division people.

HYMAN: Mostly it's between people you've worked with for years or know from the coffee machine. And the interchange is more limited now that the two divisions have been physically separated. We are trying to get more joint seminars so that we can indeed hear what people doing unclassified research learn in the outside world and then relate it to our needs.

HECKER: It is a poor substitute to have to depend on T Division for your information.

HOWE: It doesn't really work.

SCIENCE: *Is an effort being made to change the situation?*

HYMAN: Yes, there's been a change in the attitude of management.

Laboratory Support for Basic Research

The Laboratory has always recognized the need to support a wide variety of basic research, and for most of the Laboratory's history, that research was funded entirely by the weapons program. During the 1970s, however, budgetary constraints made it increasingly difficult to maintain the level of so-called Weapons supporting Research, and in 1975 concern about its steady decrease prompted Harold Agnew to found the New Research Initiatives program as a supplement. However, despite the Laboratory's growth and widened spectrum of activities, Weapons supporting Research funds continued to be t.he dominant means of Laboratory support for basic research.

In fiscal year 1982 Donald Kerr combined and expanded the Weapons Supporting Research and New Research Initiatives programs with establishment of the Institutional supporting Research and Development program. This new program incorporated the following principles, many of which required new and extensive plans on the part of everyone involved.

- The program should be Laboratory wide and should include a broad spectrum of research and development related to all Laboratory programs,
- Projects should be consistent with and in support of the Laboratory's basic missions.
- Funds should be distributed according to a fair scheme that encourages competitive proposals and ensures optimum investment of resources.
- Support should be derived proportionately from all Laboratory programs,
- Accountability of funds should be reasonable and consistent with normal practice.

DISTRIBUTION OF ISRD FUNDS IN FISCAL YEAR 1982

Research Category	Allocated Percentage of Total Funds
Materials Science and Chemistry	32%
Program Development and Applied Technology (Energy and Defense)	25%
Mathematics, Techniques, and Computer Modeling	13%
Nuclear Physics and Nuclear Chemistry	11%
Medium and High-Energy Physics	8%
Plasma Physics and Astrophysics	4%
Earth and Space Sciences	4%
Life Sciences	3%

The ISRD program has definitely improved the manner in which discretionary research funds are allocated and the status of funded projects is reviewed, Considerable freedom is exercised by the Laboratory's associate directors in organizing and evaluating projects under their directorates. As is usual with any new program, some shortcomings have been recognized and some evolution is expected. It is evident that, in spite of the healthy challenge of submitting competitive proposals, there have been too many proposals and they have, for the most part, been too long. Paperwork is being reduced, arid a system of triennial, rather than annual, review is being developed for some projects.

The accompanying table lists the distribution of ISRD funds among various broad categories in fiscal year 1982.

not just students, but the whole country, informed about what we're doing and can do. One important example in life science research is the new DNA sequence data base being established in the Theoretical Division and funded by the National Institutes of Health. This will be a comprehensive computer-based library of DNA sequences designed specifically as a resource for scientists around the world who are doing recombinant DNA research. Eventually we may be able to produce a computer-based, electronic journal that bypasses conventional publication. Scientists could submit their DNA sequence data for review and receive results in recombinant DNA research electronically.

SCIENCE: *How do new projects such as the DNA sequence library get started?*

HOWE: First someone has to have an idea and that usually happens quite informally. We sit around and talk and suddenly some guy comes up with a neat idea.

COLGATE: 'That's right. Some of us don't know one another very intimately. but sooner or later we will meet, I will bump into John and start talking about cryogenic systems for fractional charge separation using superfluid liquid helium as a charge separation drift chamber.

ROCKWOOD: Once the idea is hatched, you might try it out with what is called bootlegging. You do the experiment or the calculations at your own discretion, but generally with the knowledge of the group leader, division leader, or whoever else is involved. If the idea shows real promise you may be funded through Institutional Supporting Research and Development [ISRD] money. This is the Laboratory's discretionary fund, It has traditionally been used for basic research,

Rocky Kolb on Cosmology

"Cosmos" is the Greek word meaning order, and the basic goal of cosmology is to understand the universe on the basis of physical law. By applying physics to what we see in the universe, we endeavor to understand the structure of galaxies and the origin and large-scale structure of the universe.

Within the past five years or so some very interesting and very bold particle physics theories have been hypothesized. They model the physics of incredibly small scales-down to Planck's scale, which is about 10^{-33} centimeter. These theories are extrapolations, but there is some physical basis to them and they imply certain things about the universe. For example, they predict proton decay and the existence of magnetic monopoles. If these predictions are correct, then we now have models of the structure of matter under unbelievably extreme conditions of density and temperature, and we are in a position to study the very, very early universe. By the early universe we used to mean 1 minute or 1 second after the big bang. Now we can talk about 10^{-33} or 10^{-38} or 10^{-40} second because we believe we have a model of the underlying physics with which to do the astrophysics and cosmology.

Some practical questions we might answer are how many magnetic monopoles are expected to be around, what are their properties, and how would one look for them. Another possible insight is understanding the asymmetry of the universe in baryons-that is, why there aren't an equal number of baryons and antibaryons. Unfortunately the big bang is not an experiment that you would want to-or could-duplicate.

Study of the early universe leaves an interesting unanswered question: why the universe is so old, If you look at the Einstein equations that describe the evolution of the universe, the only

but more recently it has also been used to fund new applied programs, I, for one, believe the applied programs should receive an equal share of this money. This is our investment in the programs of the future, and, in the final analysis, only programs pay the Laboratory's bills.

HECKER: The fact that this Laboratory has the foresight to take a meaningful fraction of its total income and plow it back as discretionary research is fantastic. At many other places the discretionary research money is more like one per cent. We do have an enormous opportunity for internal research. Of course, there has been a lot of upheaval recently about having to write proposals every year for ISRD money.

COLGATE: I think proposals are a darn good idea. I never did have to do them at Livermore. Then at the university I ended up having to write twelve a year. They are never easy, but they are really worth it.

BAKER: They do help people who didn't know what they were doing to think about their work a little more, but on the other side of that coin I think management can really be an obstacle.

COLGATE: Yes, if proposals are not reviewed correctly, you end up with a mess. Most proposals are now judged by the Laboratory management and the Senior Fellows, but this does not always constitute peer review.

HECKER: I agree that we do need more accountability than we used to have. However, one simply cannot set up an environment to do good basic research if proposals are required on a yearly basis. Also, the people making the decisions have become farther and farther away from the people who really know what is going on. I'd like the authority and the responsibility for research programs to rest with the divisions. By all means have an advisory panel of outside peer experts to judge the quality of the research, and if the results aren't good, then fire the division management.

BAKER: I've found that the handing out of Institutional Supporting Research money is based too much on historical factors rather than on quality of research. There is no competition in the true sense, that is, based on demonstrated scientific competence.

HECKER: That problem has been addressed to some extent. Two years ago six working groups were set up to look at areas that were not well represented traditionally, and I know that materials science has been receiving more support recently.

COLGATE: Perhaps the ultimate mechanism is, once again, the individuals. To my mind the Lab is put together of people who have an absurd sense of ego; that is, they have the drive and the motivation to back their own original ideas.

HYMAN: It's true that most projects have started with individuals who were aware that something was about ready to break. They went out and wrote proposals; they got up on their soapboxes; they sold their ideas and started small. Sometimes the ideas fizzled out, but other times they turned into whole divisions.

BAKER: Jerry, what reception do you find to suggestions being made by the Weapons Advanced Concepts people?

LANDT: Very good in general, but there are some people who resist change and don't like to see things at the Laboratory change.

HOWE: I find in the weapons program that you can have a wonderful idea either in software or in hardware, and, fine, they will help you develop it and make the best calculations possible. But then they fail to implement it. Furthermore, we are being urged to develop our own codes rather than just to borrow from Livermore. And in fact we do have several new ones, but I find there is some resistance to changing several hundred thousand lines of a code and putting in the new stuff. The same kind of reluctance appears in the hardware; it takes several years to get a materials idea implemented.

KOLB: Is that a management problem?

time scale that appears is the Planck time, which is about 10^{-45} second. It is rather hard to understand why today, ten billion years, or 10^{60} Planck times, after the big bang, the universe hasn't either recollapsed or expanded to an extent that the gravitational attraction of the matter is irrelevant in the expansion. Today we cannot determine whether the universe will expand forever or eventually recontract, since the kinetic energy of expansion is almost equal and opposite to the gravitational potential energy. This seems to imply that in the initial expansion the kinetic energy balanced the gravitational energy to something like one part in 1056—essentially a zero-energy system. This conundrum has a possible explanation if the universe underwent a strong first-order phase transition. An active field now is phase transitions in the early universe. This is a true interdisciplinary field, bringing in particle physics, general relativity, and statistical mechanics,

Our investigations may also have a number of reciprocal implications for particle physics. It has become fashionable every time a particle physics model is proposed to look for the astrophysical impact of it, You try to see whether the new model does things to the universe that you can't allow. For example, does it lead to too much mass density in the universe? Another example is monopole-catalyzed proton decay, Colgate and I have pointed out that such decay would have a terrible environmental impact on neutron stars. The work we have done leads us to believe that either monopoles do not catalyze proton decay or that monopoles don't exist, which would really be a shame because their existence would have enormous practical implications. ■

HYMAN: It is somewhat a management problem in that the codes have been allowed to grow unstructured for so many years that they have become the unmanageable things they are.

HOWE: It may be an external problem—one caused by whoever is using the weapons.

CRAWFORD: The external response to new ideas probably varies greatly from agency to agency. The Office of Health and Environmental Research, which oversees much of the research in the Life Sciences Division, is quite receptive to new programs.

COLGATE: Other offices of the DOE are also receptive. For example, Rocky has had ISRD support for some time doing far-out research in cosmology relating to conditions in the early universe. But what's really relevant is that last year the Office of High Energy and Nuclear Physics saw tit to pick up part of his funding. Nothing ventured, nothing gained!

SCIENCE: *With regard to external support for new ideas, the Laboratory is encouraging more interactions with industry. How will this affect the Laboratory?*

ROCKWOOD: I would say that a closer union of this Lab and industry would be mutually beneficial. The best single thing that has happened is that the DOE may now allow patent rights to remain with a funding company. Private industry can now put some money into a national lab without losing all rights to patents that emerge from the work. For instance, an industrial organization that wants to get involved in a new venture requiring a group of plasma physicists wouldn't have to hire twenty of their own while they got started. Instead, they could hire our expertise in that area to help them get started—a healthy collaboration.

WHEATLEY: I really think that is right.

ROCKWOOD: I see us starting to make some progress. We have money coming from Westinghouse to help look for a method of enriching certain isotopes that they are interested in as a company. They would have refused to invest this money in us a year ago,

BAKER: The hot dry rock project is a related example. Money is coming from a variety of sources, such as the Japanese government and the German government, as well as our own government.

SCIENCE: *We hire the people and they fund them?*

ROCKWOOD: They hire our people, if you will. They contract to us to do a specific task that saves industry from building up a highly specialized group of people they don't need for the long term.

HYMAN: The kind of basic research a lot of us do is oriented toward the very large problem with very limited applications. Take the supercomputers. There just aren't that many supercomputers out there. Most vendors can't afford to support the effort needed to develop new algorithms and software that push these computers to their limits. Yet it is quite appropriate for us to do that here.

HOWE: I can forsee that industry funding might compete with basic research for a person's time. Since it is near-term support, you are going to have managers saying, "AH right, we want you guys to work on this project for Westinghouse, and you have to put aside your basic research for now."

ROCKWOOD: I think rather that industry will be wanting to use basic research that we have already completed. But I won't say that conflicts will never arise. They'll have to be worked out.

CRAWFORD: If we become closely allied with both universities and private industry, perhaps we will be able to function more as a research and development organization—taking ideas from university programs and assigning teams of researchers well qualified to test the feasibility of such ideas—with the goal of technology transfer to private industry.

SCIENCE: *Gentlemen, it seems that our relationship with industry may undergo a change. What other changes would you like to see*

ing, everyone had his own favorite procedure for these elementary functions, but gradually the better ones were included in the mathematics program library. In the ’60s and ’70s higher level routines for solving linear systems of equations, integrating ordinary differential equations, handling one-dimensional interpolation, and other moderately complicated procedures were developed and included in the computer library. But then in the late ’70s the trend slowed down and in some cases stopped. Right now we have no appreciable effort developing the next generation of mathematics support software. If such a group existed, it would be writing even higher level routines: multidimensional interpolation and differentiation programs, grid generation and adaptive mesh routines that adjust the solution algorithm to the boundary of the problem and the structure of the solution, routines to help solve large systems of sparse nonlinear equations, and routines to incorporate the boundary conditions into a discrete approximation of the physics model,

For this new software to be successful, it must be compatible with existing techniques and be simple enough that in a trial run potential users can observe tangibly better results than with existing methods. The software packages that are most readily accepted are those that behave like the existing ones—only work better.

Industries and most universities that develop new software are too far removed from the production code programmers to interact with them and obtain the essential feedback. Also, the production codes are run on the most powerful computers available and those writing the software must have access to these machines. This means that we at the national computing centers should be writing the next generation of high-level mathematics support routines to be used in our production codes. At the same time we really should be getting together more with the scientists in industry and universities who are writing mathematics software. This means having a much more active visitor program in math software development and providing easy, long-distance access to our supercomputers.

CRAWFORD: I agree that we should forge ahead in our computer work, both the hardware and the software. Our national security will depend partly on our ability to lead the supercomputer field.

HYMAN: We need a coordinated effort like Japan’s. Japan already dominates in applying robotics in industry. Through its Ministry of International Trade and Industry, it has identified other projects it plans to complete by 1990. One project is a high-speed computer whose capability is at least ten times that of the Cray-1. Another is a fifth-generation computer that will implement artificial intelligence—the number of inferences per second would be a hundred to a thousand times current technology. Losing our technological edge in these areas would have serious repercussions on both our economic and our national security.

CRAWFORD: I would like to insert another note of warning. Recombinant DNA techniques are ridiculously simple to master. The United States could suffer from foreign nations or even terrorist groups employing biological or chemical weapons. Our Laboratory is an ideal place—we have both physical isolation and classified research ability—to establish a defense program against such agents. Biological and chemical agents can and will be used by those with a cause, however ill conceived. Countermeasures like specific antitoxins are within reach of our present capability. The nation should move forward in preparing these defenses.

LANDT: To close this discussion, I would like to spend a minute or two talking about future defense. Historically this Lab has developed the nuclear side, but now we should try to get people to think about the other side, the nonnuclear. There is an antinuclear movement in this country and the world. Advances in electronics are going to permit some conventional munitions to have the same military impact as nuclear ones, and we should take advantage of that. These are some of the things the Weapons Advanced Concepts people are thinking about.

ROCKWOOD: I also believe the Laboratory should be expanding into nonnuclear weapons for defense. It appears that the nuclear age has, if you will, made the world “safe” for conventional warfare. Conflicts such as the kind in Vietnam, the Falkland Islands, and the Middle East seem those most likely to occur, and the ever-increasing role of high-technology weapons in those conflicts is a matter of which we must be cognizant. We are a nation that aspires to defend itself not by massive uses of people, but as much as possible by the use of high technology—and that means us here at Los Alamos. ■

The Participants

DAN BAKER: I got my Ph.D. at the University of Iowa with Jim Van Allen in 1974 and then went to Caltech as a Research Fellow in the Physics Division, While there I collaborated over a period of a couple of years with people from Los Alamos. In 1977 I came to Los Alamos for a job interview and was impressed with the interests and abilities of the people I encountered. I decided to join a group in the Physics Division involved in high-altitude physics, where I then worked for two or three years on satellite instrumentation and data interpretation. Since October of ‘81 I’ve been Leader of the Space Plasma Physics Group in the Earth and Space Sciences Division, which, I might add, is better known simply as Heaven and Earth Division.

STIRLING COLGATE: I came to Los Alamos primarily because the then Director of the Laboratory, Harold Agnew, and the then Leader of the Theoretical Division, Peter Carruthers, persuaded me to come. I had been a staff physicist at Lawrence Livermore Laboratory for twelve years and then President of New Mexico Tech for ten years. I realized that the type of research I knew best would utilize the facilities of a major national laboratory. My work in inertial fusion continues, and the ability to do astrophysics, atmospheric research, and tectonic engineering in an environment where my advice is respected and my research work is encouraged is a privilege beyond measure. In addition, becoming recognized as a theoretical physicist after initially being an engineer in the Merchant Marine and then being an experimental physicist for many years is a very great privilege, indeed. Explosions turn me on—from firecrackers to testing nuclear bombs at Eniwetok, from using the Lab's codes to calculate supernova explosions to preventing volcanic ones. Our universe started with an explosion, is tilled with explosions, and by far the most extraordinary and singular one is the explosion of intelligent life.

BRIAN CRAWFORD: I was actively recruited by the Laboratory while I was completing work for my Ph.D. at Johns Hopkins University. The Genetics Group of the Life Sciences Division needed someone to investigate the basic mechanisms by which ionizing radiation, chemicals, or other agents cause gene mutation and/or malignant transformation in cells. I had the specific skills required because my thesis had involved study of the genetic mechanisms of chemical carcinogenesis. I was encouraged to apply for one of the Laboratory's Oppenheimer Fellowships, which I received in time to begin work in the summer of 1981. Since I came, I have been applying recombinant DNA methods to research on the genetic events underlying carcinogenesis. What attracts me to this Lab are its advanced facilities and, above all, its cooperative atmosphere—theoreticians are working closely with biophysicists and biochemists in very sophisticated studies.

SIG HECKER: I grew up in Austria but moved to Cleveland when I was thirteen. Indeed, I had never been west of Toledo until I came here as a summer graduate student in 1965. My visit was brought about by a gentleman from the Laboratory's recruiting office who showed me a brochure containing lovely photos of New Mexico mountains. Once here I liked the marriage of basic science and applied technology at the Laboratory. After receiving my Ph.D. from Case Institute of Technology, now Case Western Reserve University, I returned to Los Alamos as a postdoc in 1968, attracted by the excellent funding and the chance to do basic research in metal deformation. In 1973 I came as a staff member after three years in the Physics Department of General Motors. I've worked ever since in materials science, principally in plutonium metallurgy and in actinides, although I've worked on a number of projects related to the space power and basic energy programs. Two years ago I joined the Division Office of what is now the Materials Science and Technology Division.

STEVEN HOWE: I'm another of those students who keep turning up. I started coming here as a summer student in 1975 and did that for the next two years. Then in January '78 I came to do my thesis research at the Weapons Neutron Research Facility at LAMPF. After receiving my degree from Kansas State University, I spent a year at Kernforschung Zentrum in Karlsruhe and then returned as a staff member in September '81. I'm in the Thermonuclear Applications Group in the Applied Theoretical Physics Division.

JAMES (MAC) HYMAN: I was indirectly introduced to Livermore and Los Alamos at the same time. I was interviewed for my graduate fellowship, a Hertz Fellowship, by someone from Livermore, and he asked, "What are you doing this summer?" I worked that summer at Livermore, and it was the first time I saw mathematicians and physicists working in close coordination with experimentalists. It was just great—except the temperature was 115 degrees. My boss at Livermore had been here during the war, and he said, "Where you really want to go next is Los Alamos." So I did, and it evolved into a full-time job after I got my degree from the Courant Institute. I work on numerical methods and software for large systems of differential equations, equations that model the physics experiments. It's partly physics, partly computer science, and mostly mathematics.

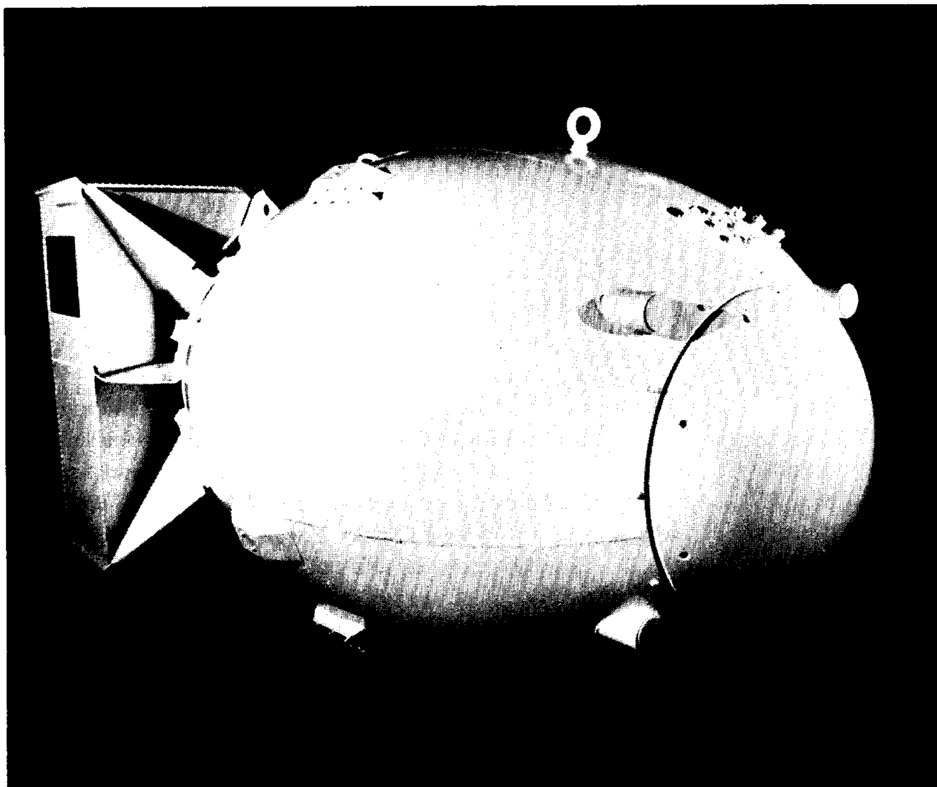
EDWARD (ROCKY) KOLB: I received my Ph.D. at the University of Texas in '78. I interviewed here for a postdoc position, but I went to Caltech instead. Then I came here as an Oppenheimer Fellow rather than going to a university, because here I could spend 100 per cent of my time doing research rather than teaching and sitting on committees. I was attracted by the people I would have a chance to work with. It was really the people who brought me here. I did my Ph.D. in elementary particle theory, and now I'm into cosmology and astrophysics, high-energy astrophysics. I'm in the Theoretical Astrophysics Group and I work closely with the Elementary Particles and Field Theory Group, an overlap that's possible here for someone not in a traditional discipline. At universities people seem more locked into compartments: there's one person in nuclear physics, one person in atomic physics, and so forth, and it's not easy to move into new fields. Here at Los Alamos you can move quickly into exciting fields as they open up.

JEREMY LANDT: The country in the western part of my home state of South Dakota is very much like the country here, so perhaps that was a factor in my initial attraction to Los Alamos. I came here in 1967 as a summer graduate student and liked the facilities and the people. When I completed my research work at Stanford, there weren't too many jobs available at Los Alamos in the areas I had studied—radiopropagation, electromagnetic theory, and that kind of thing. But there were at Livermore, so I spent a few very enjoyable

years there. But I got tired of all the people and the hassle, and when something opened up here, I applied and came back in 1975. Except for the past year, my stint here has been spent in the Electronics Division. I have worked on electronic identification systems, EMP calculations, application of radar and other electronic techniques to mapping underground fractures for the hot dry rock project, plus a little nuclear magnetic resonance work, so I have dabbled in this and that. At present I'm working in the Weapons Advanced Concepts Program Office. We're supposed to be looking at wonderful new things; we're finding lots of wonderful old things that other people have thought of.

STEVE ROCKWOOD: After finishing my doctorate at Caltech in 1969, I went into the Air Force as my obligation to the country during the Vietnam era and spent two years at the Air Force Weapons Lab. There I got into laser activities, a field entirely different from my graduate work. I came to Los Alamos in 1972 principally because the laser programs then being started at the Laboratory and the people here were stimulating, It is an exciting area to work in. A secondary consideration would have to be the New Mexico environment. My own personal way of working has been to change fields frequently, although always within physics. I started out at the Laboratory as a theorist in T Division and then became part of the fledgling isotope separation program and was Leader of the Laser Development Group until 1980. Then I took over my present job as Deputy Associate Director for Inertial Fusion. To me the main attraction of the Laboratory, in contrast to universities, is its ability to pull together the resources to do a large multidisciplinary program and move on it quickly.

JOHN WHEATLEY: I received my doctorate from the University of Pittsburgh in 1952 and came here just recently, after stints at the University of Illinois and the University of California, San Diego, because I saw the opportunity to do both the basic physics research that is my main line of work and also what I call fundamental technology. That combination is highly regarded here, while in my previous university careers I always felt I had to sneak my interest in technology in the back door. After all, instruction through basic research, not development of technology, is the principal function of a university. Also, I perceive a very substantial increase in my effective mass here because the Lab has many more people interested in my field, which is thermal physics and condensed-matter physics.



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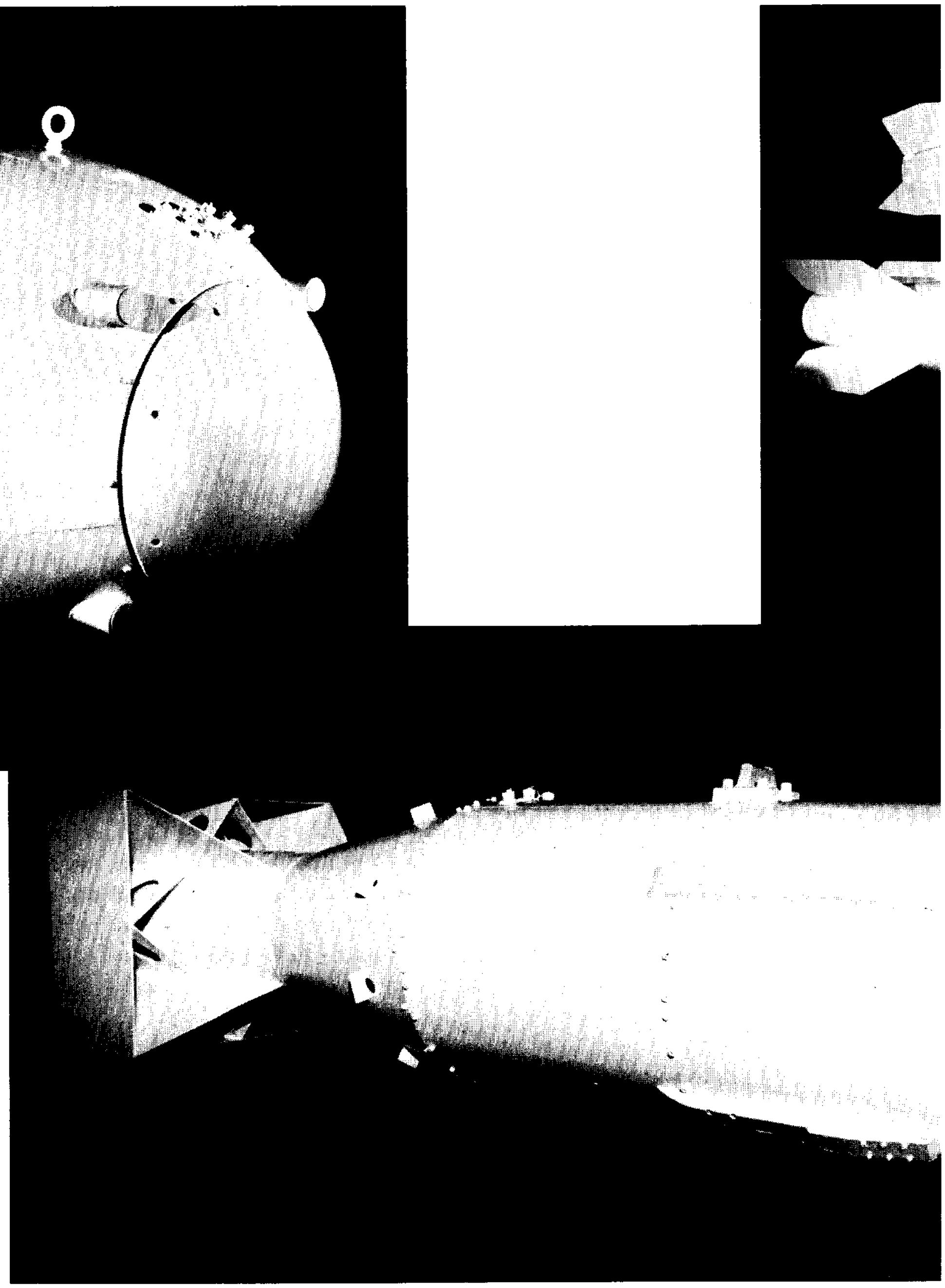

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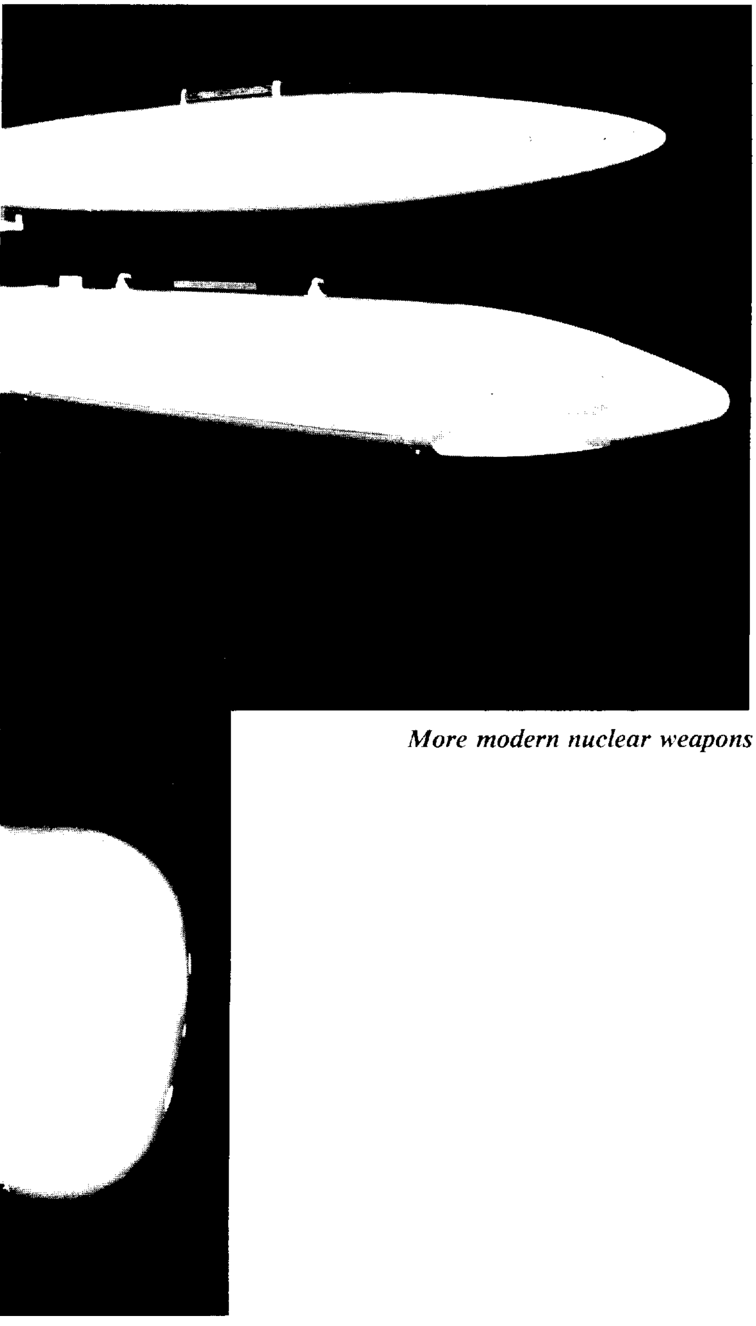


More modern nuclear weapons

Program

Overview

by C. Paul Robinson

The major mission of Los Alamos National Laboratory continues to be research and development of nuclear weapons. Having developed the first fission bomb as well as the first fusion bomb, the Laboratory continues as a center of excellence in the nation's most important defense science.

The fortieth anniversary of the Laboratory is synonymous with that of the nuclear weapons program, This anniversary is a significant event since normally a scientist's career—from receiving a Ph.D. at age 25 to retirement at 65—also spans forty years. Thus, with few exceptions the original pioneers in nuclear weapons have all completed their scientific careers and the weapons program is staffed by a new cast. Although the original sense of urgency has paled. the importance of the work has not, and today's weapons scientists and engineers are no less dedicated or purposeful than those of the first generation.

Over the past forty years the task of recruiting talented staff and maintaining a high sense of mission has not always been easy. The strict controls over nuclear secrets continue to be a fact of life. Very little substantive information has been declassified concerning the detailed physics and hydrodynamics of nuclear weapons. much less any of the design principles. Thus. the scientists and engineers who specialize in nuclear weapons research and development spend their entire careers working within a '-closed" technical society with little recognition of their work beyond that of close associates.

In society at large, opinions of the importance of weapons work have generally reflected the changing attitudes regarding the value of a strong national defense, attitudes that vary from pride in our nation's strength to the fear of war, particularly a nuclear war. In general, the relative isolation of Los Alamos has helped insulate the Laboratory and its weapons staff from the sometimes mercurial sentiments of the outside world. The importance of long-term stability in the weapons program cannot be overemphasized, both in allowing extremely complex problems to be attacked over a span of many, years and in providing a training ground for new scientists in complex subjects, some so difficult that only after a decade of work beyond the Ph.D. is one prepared to undertake original research.

The Laboratory has consistently enjoyed high respect and continued strong support from the political leaders of the nation in both Democratic and Republican administrations and from the Congress as a whole. The next few years should present an important test of national support as we see a grass-roots movement emerging to protest continued research and development of nuclear weapons. Most of' us within the nuclear weapons community welcome this inquiry by the public into the philosophy of nuclear defense matters and believe our role and stature will, in the long run. be strengthened by this attention. We believe the fundamental role of advanced technology in defending a democratic society must be re-examined and understood by each generation of Americans.

Major Research and Development Themes

in attempting to provide a snapshot of the nuclear weapons program on the occasion of its fortieth anniversary, it is useful to reflect on some of the important research and development themes of the last several years. This undertaking is difficult because of our tight security restrictions. (The only declassifications during the last few years involved concepts developed more than 25 years earlier.) However, discussion of a few themes should allow some insight into the overall scope of the effort.

Physics issues being investigated include two scientific mysteries dating from the early 1950s: "boost" physics and radiation flow. "Boost" refers to the process whereby thermonuclear reactions are used as a source of neutrons for inducing fissions at a much higher rate than can be achieved with neutrons from fission chain reactions alone. Achieving conditions for thermonuclear burn requires creating enormous temperatures (higher than within stars) simultaneously with enormous pressures. Since this process has no analogy within other realms of science, essentially all of the physics—including new mathematical methods—had to be created to provide theoretical understanding and design techniques. The science of nuclear weapon phenomena has always been a driving factor for larger and faster computers,* and boost physics will continue to require major advances in computing hardware, as well as in physical theory, before a complete description from first principles will be possible. This statement still is accurate in spite of the fact that today Los Alamos has what is believed to be the largest scientific computing center in the world.

The first megaton-yield explosives (hydrogen bombs) were based on the application of x rays produced by a primary nuclear device to compress and ignite a physically distinct secondary nuclear assembly. The process by which the time-varying radiation source is coupled to the secondary is referred to as radiation transport. Again, radiation fields of such magnitudes or characteristics had not been encountered in any other field of science, so the physical theories and methods had to be created, Similarly, full mathematical description is limited by the computing power of today's best scientific computers.

One of the most important research efforts to be initiated in the last decade is the inertial fusion program. Inertial fusion seeks to utilize laser radiation or ion beams to compress and heat fusion fuels to achieve thermonuclear reactions in the laboratory. Although directed toward different objectives, inertial fusion experiments embody significant aspects of radiation-transport physics. However, since the scale is significantly reduced, the physical characteristics of the processes and the resulting "micro-explosions" do not precisely replicate the phenomena exhibited in nuclear explosions. Nevertheless, the similarities have brought new approaches and techniques, and indeed new scientists, to explore these complex phenomena. The similarities have also brought strict control of the important inertial fusion information, and the major efforts in this field are carried out as classified projects within the weapon laboratories (Los Alamos, Livermore, and Sandia).

Classification restrictions allow little to be said about the modern practice of nuclear weapon design beyond the observation that, even after 40 years of quality research and development, improvements in both yield-to-weight and yield-to-volume ratios continue to be made.

The primary function of nuclear weapons is to provide strategic deterrence. Their ability to destroy essentially any blast-sensitive area target should deter all-out aggression by any adversary. In recent times efforts have also been devoted to developing nuclear weapons for tactical or military theater use in order to deter invasion by superior forces. Such uses would employ, if necessary, small nuclear warheads with outputs tailored to destroy the invading force with little collateral damage to nearby population. Los Alamos has played a major role in developing such munitions and continues to explore the potential for such weapon innovations. Another interest is exploration of possible uses of nuclear explosives as energy generators to power other specialized weapons. Energetic photons from a small nuclear explosion were used by Los Alamos scientists nine years ago to generate a short burst of laser light. Such exploratory research activities exemplify the frontier technologies being studied and evaluated within the nuclear weapons program today.

The Laboratory's engineering development activities have concentrated in recent years on improving the safety and security of nuclear weapons. Our most important design aim is to ensure that no nuclear explosion could ever occur in any conceivable accident. An enormous reservoir of human ingenuity and activity has been expended to meet this goal. In the autumn of 1980, a Los Alamos nuclear weapon, the Titan missile warhead, was involved in an accidental explosion of the rocket's fuel. Although the warhead itself was caught up in the explosion blast and was hurled, along with hundreds of tons of steel and concrete, more than 200 yards from the missile silo, the warhead survived essentially intact. The many safety features incorporated in the design served well. Neither that accident nor any other accident involving U.S. nuclear weapons has resulted in a nuclear explosion.

Los Alamos pioneered the development and use of insensitive high explosives in nuclear weapons. These unique chemical explosives provide greatly improved safety against even the spread of the radioactive materials in accidents ranging from airplane crashes and fuel tires to intentional firing of a rifle into the high explosive.

Other significant recent engineering developments include the "ruggedizing" of the devices to withstand extreme environments. An example of the hardness that can be achieved is a new earth-penetrating nuclear warhead that can survive ground impact and significant penetration without adverse consequences to its explosive capacity.

All advanced technology results from a blending of theory and experiment, and for nuclear weapons underground tests are the essential experimental ingredient of our research and development programs. We have adapted quite well to carrying out all of these

The first sizable computer, the ENIAC developed in the late '40s and early '50s and now displayed at the Smithsonian Institution, was used for nuclear weapon calculaions.

experiments deep underground in an isolated desert area in southern Nevada. Besides proof-testing new designs or alterations to previous designs, the Nevada tests sample and measure the unique conditions produced within a nuclear explosion. Very sophisticated instruments have been developed for such measurements. Detectors must register the physical phenomena and transmit the data to the surface before being enveloped in the nuclear fireball. Most of these diagnostic techniques have eventually found application in a variety of other scientific measurements.

Major Weaponization Programs

In recent years Los Alamos has been responsible for designing all new strategic warheads that have entered the nation's stockpile.

The W76 is the principal warhead for submarine-launched ballistic missiles, each carrying a number of independently targeted warheads. These warheads are carried on Poseidon and Trident submarines and represent one of our most important retaliatory nuclear forces.

The W78 is deployed on Minuteman III land-based strategic missiles.

The W80, a common warhead for both air- and sea-launched cruise missiles, represents a major development in strategic weaponry. These weapon systems do not pose a first-strike threat against potential adversaries but do guarantee that specific targets could be precisely attacked in a retaliatory strike. Cruise-missile weapon systems represent the best of America's advanced electronic guidance and targeting capabilities, augmented by exceptionally compact nuclear explosive charges. These systems should provide a major share of our deterrent capability for a decade or more.

In addition to these strategic systems, other Los Alamos weapons under development include warheads for the Navy's air-defense missile, the Standard Missile 2, and for the Army's Pershing II intermediate-range missile and significantly improved versions of the Air Force's and Navy's air-carried bombs.

Supporting Activities

A variety of other defense activities have arisen over the years, Most of these, such as providing concepts for protecting our re-entry vehicles, relate to the nuclear weapons mission. Fundamental questions, such as the degree of protection needed to survive high fluxes of neutrons or gamma rays, require both new theoretical and experimental techniques. Recently we have considered other threats, including laser and particle beam fluxes as well as electromagnetic pulses. These studies required the development of energy sources and damage measurement instrumentation, along with elaborate mathematical simulation models. This effort exemplifies a continuing characteristic of Los Alamos research efforts-that concentrated efforts can yield significant improvements in the state of the art on either side of an issue. Understanding the limiting features of hardening a strategic system against attack inevitably leads to improved concepts for the best methods to attack enemy systems.

Verification activities, particularly in regard to nuclear test treaties, have diversified and expanded. The Limited Test Ban Treaty of 1963, which precludes nuclear tests in the atmosphere, led to research into detection of nuclear explosions there or deep in space. Satellite-based instruments, along with ionospheric probes and detectors, have provided valuable data on the natural background conditions in space while continuously monitoring for nuclear explosions. In addition, we have developed seismic detection systems to provide higher accuracy in determining the yield of underground tests, which are limited under the threshold test ban treaty (observed although unratified) to less than 150 kilotons.

Two emergency operations teams have been formed to respond to accidents involving U.S. nuclear weapons or to threats of improvised nuclear devices. The value of highly skilled personnel on the scene during such crises cannot be overestimated. The teams comprise volunteers whose normal work assignments range from weapon design theorists and engineers to explosives experts.

Although the Laboratory has always devoted some effort to the development of nonnuclear weapons, we have expanded these activities in response to ever-widening threats. The subjects of improved lightweight armors and armor Penetrators have substantially benefited from our research and innovation. Laser and electromagnetic wave generators have been conceived and developed into prototypical devices. Similarly, we have utilized our expertise in particle accelerators to build prototypes of neutral particle beam weapons.

A recent innovation, which appears to have significant potential for a variety of defense missions, combines the scientific capabilities in accelerator and quantum optics technologies to create large free-electron laser systems.

Small but quite important defense-related projects now under way include defensive approaches against chemical and biological warfare attacks, particularly rapid detection and protection. We also have begun to explore technological responses to deal with terrorist or subnational threats.

Finally, we have undertaken a concentrated effort to identify the most serious technological threats we may encounter in the future. Thus, we are again emphasizing the most important long-range mission of the Laboratory: to determine how science and technology can best be employed to defend the country,

Although any projections regarding the course of weapons research and development during the next 40 years would be fraught with difficulty, I believe it safe to assert that warfare is likely to be far more chaotic in the future than in the past. We cannot rely on any single technology, not even our nuclear strength, to provide an absolute defense. We must continue to probe wide areas of science, both to prevent technological surprise by an enemy and to create new strengths for ourselves. ■

Nuclear Data

The Numbers Needed to Design the Bombs

by Ben C. Diven, John H. Manley, and Richard F. Taschek

The Los Alamos Laboratory was established in 1943 to investigate whether nuclear weapons were feasible and, if so, to design and fabricate them as soon as possible. It was obvious that this task demanded many new nuclear data. Even the basic fission processes were very poorly known, and most of the interactions of neutrons with nuclei of potential weapon materials were unexplored.

It was also clear that obtaining the necessary nuclear data required accelerators. Because building accelerators would be time-consuming, even if they duplicated ones already in existence, several accelerators at other institutions in the United States were simply dismantled, shipped to Los Alamos, and installed in hastily constructed buildings. A 0.6-million-volt Cockcroft-Walton accelerator came from the University of Illinois. Two Van de Graaff accelerators (2.5 and 4 million volts) came from the University of Wisconsin. And a cyclotron that could produce deuterons with energies up to 11 million electron volts came from Harvard. These machines had been used for effective nuclear physics research at their home bases but now were destined for studies specifically needed for the design of a nuclear weapon, under conditions where the effort could be better coordinated. In a single community day-to-day discussions of physical concepts and experimental methods would no doubt stimulate and speed up the learning process.

To learn about the data that needed to be gathered and the difficulties of doing so, we interviewed three scientists who participated from the earliest days. They clearly had enjoyed the challenges and the rewards.

SCIENCE: *Among the first and most important jobs at Los Alamos was the hurried transport of accelerators to the site. Why were accelerators needed?*

MANLEY: Accelerators could be used as sources of fast neutrons. Before Los Alamos the fission process had been well studied for slow, or thermal, neutrons because thermal fission was the basis for the reactors that would produce plutonium for the bomb. But in an explosive chain reaction in a nuclear weapon, a bunch of neutrons would come out—boom—from uranium or plutonium with much higher energies, almost a million times higher, than typical thermal energies. These so-called fast neutrons would not be moderated, or slowed down, by graphite as they were in a production reactor but instead would bounce around in a big mass of uranium or plutonium and cause various reactions. At the start of the bomb project, we didn't know how effective fast neutrons would be in producing new fissions. We needed to measure the fast fission cross section and other fast-neutron processes, and the only way to produce fast neutrons for these experiments was with accelerators.

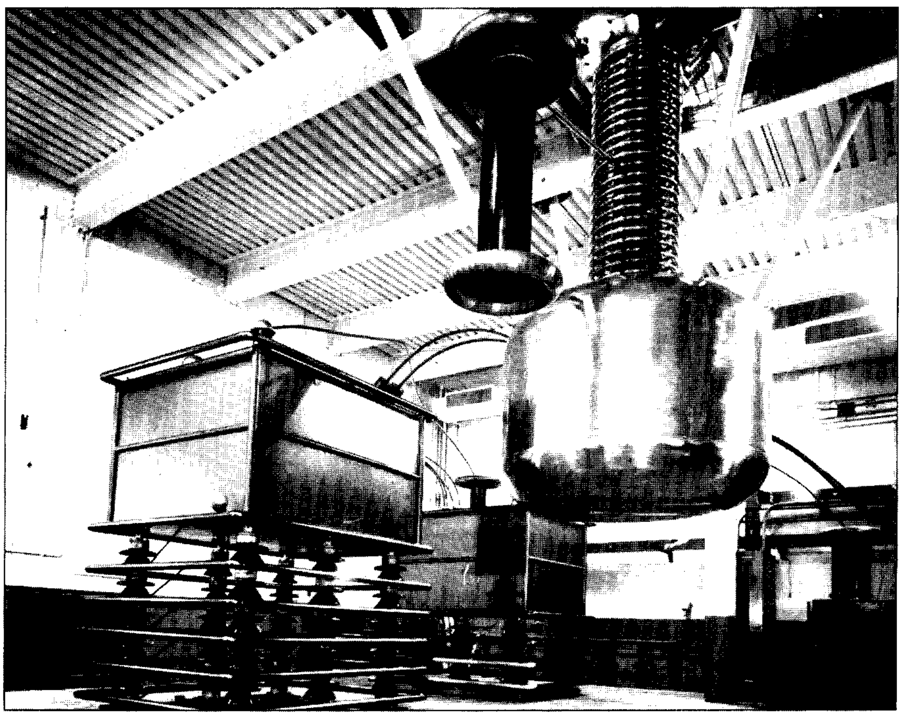
TASCHEK: Most neutrons emerge from the fission process with energies between 0.1 and 3 MeV [million electron volts]. But until about 1942 there were no neutron sources at those energies except for Cockcroft-Walton accelerators of the kind that John worked with at Illinois. That machine was used to bombard deuterons with deuterons [$D + D \rightarrow {}^3He + n$]. Incident deuterons with energies of 0.4 MeV produced reasonably monoenergetic 2.5-MeV neutrons. Then at Wisconsin, where I was prior to coming to Los Alamos, neutrons with a range of energies were produced by bombarding lithium with protons accelerated in a Van de Graaff accelerator [$p + {}^7Li \rightarrow {}^7Be + n$].

MANLEY: We needed neutrons covering as much of the relevant energy range as possible, and we needed them in a hurry. So we just moved the university accelerators to Los Alamos as a matter of convenience. Wisconsin, which was on government contract, supplied the two Van de Graaffs. They produced monoenergetic neutrons whose energy could be varied from a few tenths of an MeV to 1.8 MeV. We went to Harvard and convinced them to let us have their cyclotron. Bob Wilson. being an old cyclotron man and having his project on isotope separation closed down at Princeton, was the logical one to run it. The cyclotron produced an intense neutron source over a big smear of energies. But with a moderator it became a good source of thermal neutrons. Finally we just swiped my old Cockcroft-Walton that was built at Illinois.

I was the one in charge of getting all those damned machines up to Los Alamos in the spring of 1943, and that was work. We had to load them from boxcars into trucks, travel up the old road to Los Alamos, install them. and so on. I remember we couldn't get the Wilson Transport Company on the job very fast. They did give us a driver and a little pickup truck, which couldn't carry much. We had packed the Cockcroft-Waken acceleration tube in a hurry simply by running a long bolt through all the sections and clamping them together with wood. That was in the back of the pickup truck waving around. I had fidgets coming up here. Then for several months we worked to put it all back together again. It was a mess at the beginning. The wiring wasn't all in, and here we were trying to get things hooked up. We worked three shifts a day, and by July every one of those accelerators was operational—a real record.

TASCHEK: Accelerators were very primitive in those days. We didn't ask for the Princeton cyclotron because it really was put together with sealing wax and string. When the magnetic field was turned on. the

The Illinois Cockcroft-WaMon



The Cockcroft-Waken accelerator requisitioned for Project Y had been developed by John Manley and his coworkers at the University of Illinois in the late 1930s. It was an improved version of the first such accelerator, which was built in 1931-32 by Cockcroft and Walton at the Cavendish Laboratory in Cambridge, England. As plans for establishing the Los Alamos Laboratory developed, Manley, now a member of the Metallurgical Laboratory at the University of Chicago, persuaded a young Bachelor of that group, Harold Agnew. to take on the job of overseeing the moving of the Illinois machine to Los Alamos and its installation in Z Building. It was to serve there as a source of neutrons, which were produced by bombarding deuterons with accelerated deuterons. The accelerator was installed in the basement area of the building so that its vertical acceleration tube could provide a beam on a target at the ground-level area. As expected, the reduced atmospheric pressure at the approximately 7000-foot elevation of Los Alamos decreased the voltage attainable with the machine by a moderate 25 per cent from its design voltage, that is, to about 450 kilovolts. Above that voltage electrical sparking occurred from the exposed elements of the high-voltage equipment. This photograph of the Cockcroft-Walton shows a condenser bank on the left and on the right the high-voltage electrode with the acceleration tube extending vertically upward.

vacuum would break. The Harvard machine was the only reasonably well-designed cyclotron, so it was simply pre-empted—and at a ridiculously cheap price. The Short Tank Van de Graaff from Wisconsin was also a pretty poor specimen when it came here. It had been designed by the graduate students and was redesigned and rebuilt here under the direction of one of them, Joe McKibben.

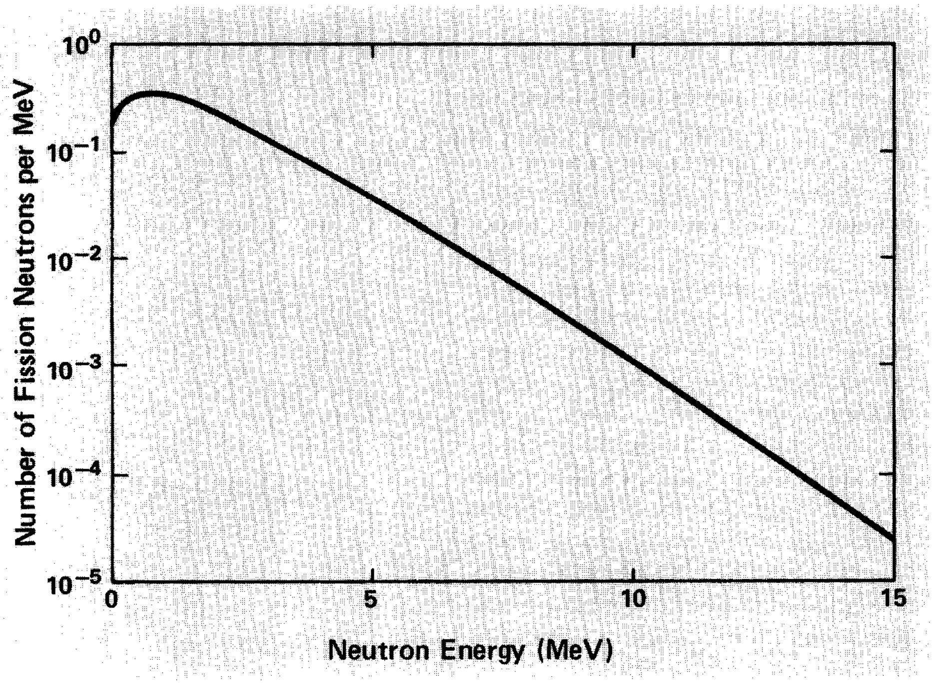
Then we faced the technically difficult job of producing monoenergetic neutrons from the proton-lithium reaction. First we had to get monoenergetic protons out of the Van de Graaff. Then we needed a method for making very thin lithium targets so that the neutrons produced in the reaction would not be scattered as they left the target.

We had people working on making the neutron sources better and trying to generate new sources. Other people were working on how to measure the neutron flux [number of neutrons emitted per second], and some people of course were actually measuring the quantities of interest—of which there were a great number.

DIVEN: During the first year at Los Alamos the nuclear data work occupied the attention of a substantial fraction of the staff. It was an extremely important effort.

SCIENCE: ***What were the crucial questions that had to be answered by nuclear data measurements?***

DIVEN: When we first came to Los Alamos, it was very poorly known how much uranium-235 or plutonium would be required to make a bomb because their critical masses for fast neutrons were unknown. The most important quantities to determine were the fission cross sections for uranium-235 and plutonium and the average number of neutrons emitted per fission. We also needed to know the fraction of fission neutrons that gets captured and does not take part in the chain reaction. We were going to try to decrease the amount of fissile material in the bomb by surrounding it with a so-called tamper that would reflect neutrons and prevent their escape from the nuclear core. So we had to measure the scattering properties of a huge number of materials in order to guess which would work best for this purpose.



Accelerators were brought to Los Alamos to provide neutrons with energies similar to those of the majority of neutrons produced by fission of uranium-235 or plutonium-239. Shown here is the spectrum of neutrons emerging from the fission of uranium-235; the fission neutron spectrum of plutonium-239 is similar.

MANLEY: We had to know the elastic scattering, the inelastic scattering, and the capture cross sections for every single element we wanted to try as a tamper.

DIVEN: And we needed to know these cross sections as a function of neutron energy. A bomb contains a big mass of fissile material, and any one neutron can undergo many reactions as it bounces around in the nuclear core. It can scatter elastically or inelastically, it can be captured, or it might cause a fission. And every time it does one of these things its energy changes. It isn't enough to know a cross section at some particular energy. We needed to measure accurately the energy spectrum of fission neutrons and to measure the various cross sections over this whole spectrum. Making this enormous number of measurements in a short time was a staggering problem.

SCIENCE: ***Did the nuclear data work begin at Los Alamos?***

TASCHEK: It started before at various universities and other institutions, and then the same people came here to continue it. For example, the need for a tamper was known very early, and people at Wisconsin working with the Short Tank, the small Van de Graaff, were trying various heavy elements like tungsten and gold. They had a rather impressive supply of gold there for that purpose.

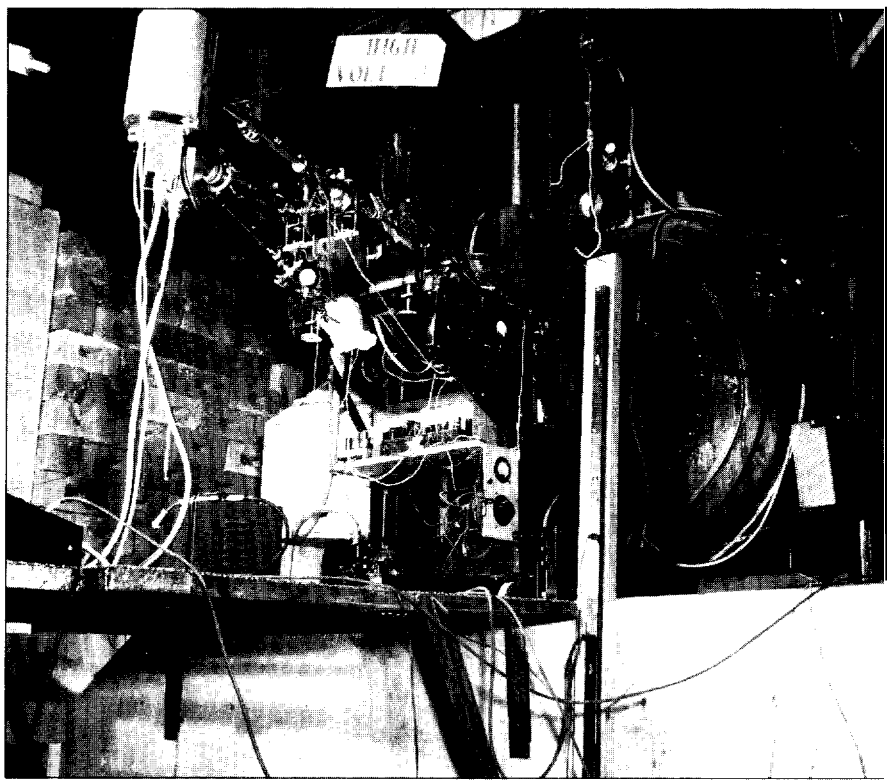
MANLEY: The very first experiment done at Los Alamos was to answer the question of just how soon, relative to the fission itself, the so-called prompt neutrons are emitted. It was a go/no-go experiment—if the neutrons didn't come out soon enough, we couldn't have an explosive chain reaction. The presence of some delayed neutrons is what makes control of a reactor possible, but you don't want to control a bomb—you want it to go bang. Delays of a hundredth of a microsecond would have meant the end of the project. Some people here did a very cute experiment that could detect time delays of a billionth of a second. None were detected, so we were OK.

DIVEN: The experiment was really elegant because you didn't have to know the efficiency of the fast-neutron counters. you didn't have to know how much uranium-235 was in the target, and you didn't have to know the incident neutron flux. All you had to do was irradiate a uranium target with neutrons to induce fission and count the number of fast neutrons with a gas or vacuum between the uranium target and a fast-neutron counter. If neutron emission was delayed relative to fission. the neutron count would be less with gas between the target and the counter than with vacuum because. by slowing the fission fragments, the gas causes neutron emission to take place farther from the counter. Since the velocity of a fission fragment is about 10^9 centimeters per second in vacuum, a distance between target and counter of a few centimeters gave a pretty good time scale. Within the limits of the experiment. no difference in count rates was detected. So an upper limit of 10^{-9} second was established for the delay in prompt neutron emission.

MANLEY: That experiment was fairly easy to do because all we wanted was an upper limit. But as soon as we wanted absolute numbers for fission cross sections, we ran into serious difficulties. I remember tearing my hair out because we couldn't be sure how much uranium-235 was on the target foils. The assays were very difficult, and the results wandered all over the place. It wasn't even easy to determine bow much total uranium we had.

TASCHEK: The fission cross-section experi-

The Wisconsin Long Tank



The Van de Graaff accelerator known as the Long Tank was the latest of a series built during the late 1930s by Ray Herb and numerous graduate students at the University of Wisconsin. Both the Long Tank and the Short Tank, a lower voltage, higher current Wisconsin machine that was the product mainly of Joseph McKibben, came to Los Alamos in the spring of 1943 along with much of the Wisconsin research group. The Long Tank was probably the best tool of that period for precision research on nuclear reactions, and after becoming operational again in June 1943, it became the workhorse for the Laboratory's investigations of neutron interactions with fissile materials and other bomb materials, Monoenergetic neutrons were produced by bombarding a lithium-7 target with accelerated protons, The energy of these monoenergetic neutrons could be varied between a few tens of keV and almost 2 MeV a major fraction of the interesting part of the fission neutron spectrum. With accelerated deuterons, a different range of neutron energies could be reached but with more difficulty and rather bad backgrounds. This photograph of the Long Tank shows the neutron-producing target and an experimental target (foreground) and the pressure vessel (right background).

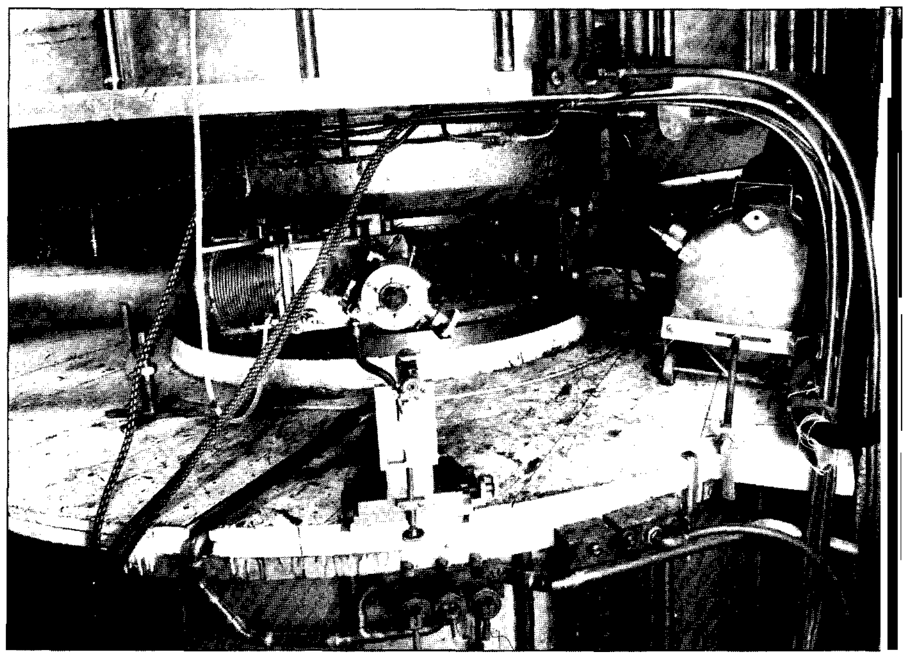
ments were done by coating platinum foils with a thin layer of uranium, maybe 10^{-5} centimeter thick, bombarding the foils with neutrons of a certain energy, and detecting the fission fragments with ionization chambers. By counting the fission fragments for different neutron energies we were able to make relative measurements of the cross section. For measurements of the absolute cross section we needed to know the neutron flux of the source. That problem plagued us for the next twenty years or so and still does a little bit.

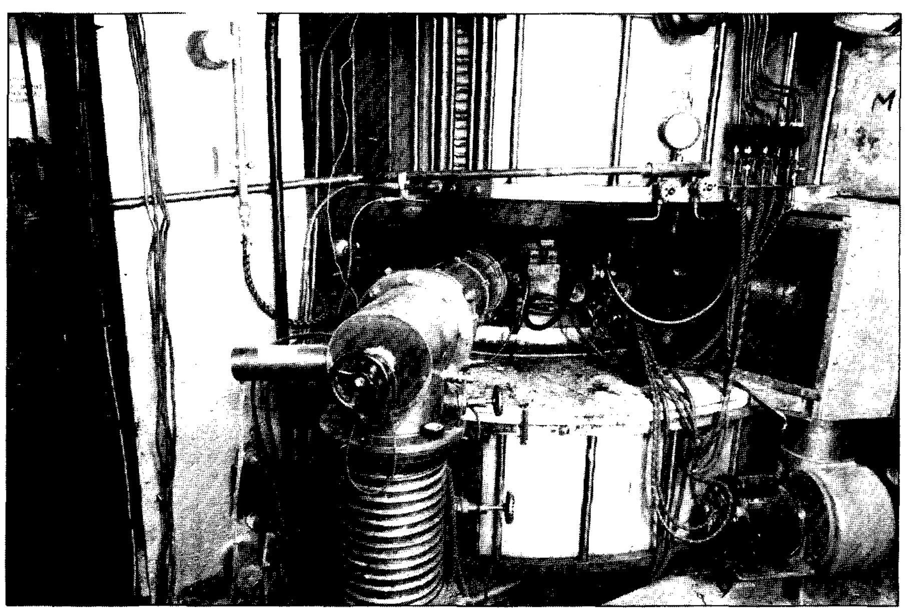
We also had great difficulty measuring $\bar{\nu}$ [the average number of neutrons emitted per fission] as a function of energy. That quantity could be measured fairly well and quite easily for thermal neutrons, but it was hard to measure for fast neutrons because so many neutrons—on the order of 108 or 10^9—must go through the sample before one fission takes place. In other words. the signal-to-noise ratio is very, very low. It was a long time before we could measure $\bar{\nu}$ for fast neutrons. During the war we simply assumed that $\bar{\nu}$ was a little bigger for fast neutrons than it was for thermal neutrons,

The neutron-capture cross sections were also very difficult to measure and cross sections for the emission of two neutrons weren't being measured at all except in a few cases where one of the final fission products is a radioactive nuclide. It took about twenty years before we could make systematic measurements of all the cross sections involved, The measurements John participated in during the war, that is, the angular distributions of inelastically and elastically scattered neutrons, were also very, very difficult, Not until the '60s did we begin to get some fairly decent measurements. Most of our wartime difficulties arose from lack of appropriate techniques and, most important, suitable electronics. From today's standpoint. electronics was at the cave-man level during the war.

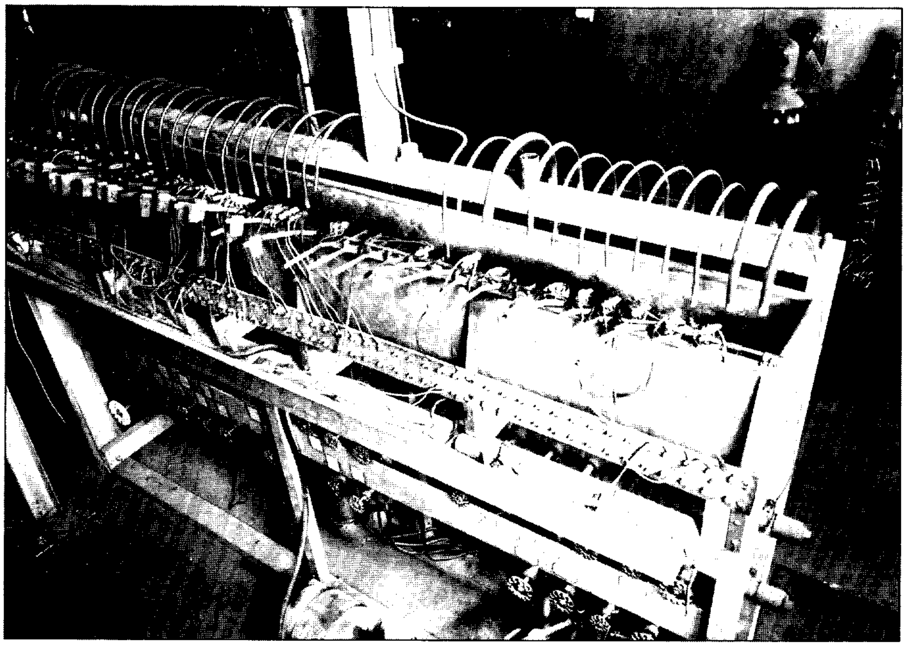
MANLEY: All these measurements were aimed at determining critical mass and ex-

The Harvard Cyclotron





The cyclotron commandeered for use at Los Alamos belonged to Harvard University and had been built there during the 1930s. It produced 7-MeV protons and 11-MeV deuterons with a maximum beam current of 100 microampere. The two largest pieces of its magnet each weighed 18 tons, and the magnet's total weight was 70 tons. In preparation for



transport to Los Alamos, the cyclotron was disassembled and packed under the supervision of a group from Princeton University that later became the Laboratory's cyclotron group. Most parts of the cyclotron arrived at the site in early April 1943, By that time remarkable progress had been made toward turning the Los Alamos Ranch School into Project Y: most utilities were in, roads were built, and a special building for the cyclotron was complete. The magnet was reassembled by riggers using only jacks, rollers, and timber. Within about two months, albeit after many hours of overtime, the cyclotron produced its first deuterons for the war effort. These deuterons were used to generate neutrons by interacting with a beryllium target. In 1954 the cyclotron was relocated and rebuilt as a variable-energy machine, It was last operated in August 1974 and has since been dismantled.

The top left photograph shows the side of the cyclotron from which the deuteron beam was extracted. A bending magnet then steered the beam to a scattering chamber. The telescope in the middle was used to monitor the position of the dee feeler, which was very critical to the cyclotron's operation. To the right is the deflector capacitor. The bottom left photograph shows the other side of the cyclotron. From left to right are the diffusion pump, the ion-source control with water lines and cables attached, and behind the copper screen the two pyrex dee bells that support the dees. The top right photograph shows an unlikely contraption that assured proper flow of cooling water to various parts of the cyclotron. Return water flowed through the pipes into the little buckets, which had lead weights positioned along their "handles" and small holes in their bottoms. Mercury switches that monitored the flow of input water were mounted at the pivot points between the buckets and their handles. The set points of these switches were controlled by adjusting the sizes of the holes and the lead weights.

plosive yield. There were two main paths to arrive at the critical mass. One might be called the Edisonian approach—you just amass enough material and see if it works. But at the start we didn't have enough fissile material for this approach, so instead we tried to measure all the nuclear constants and all the cross sections that go into making a bomb critical and then summed up all the measurements to predict the bomb's behavior. This is the differential method.

TASCHEK: As more enriched uranium arrived at Los Alamos, we began to do an integral experiment known as a neutron-multiplication experiment. We added more and more enriched uranium to a uranium assembly with a neutron source at the center and measured how the number of neutrons multiplied with each addition. By extrapolating these measurements, we could determine the mass that would be needed to make the bomb go critical.

DIVEN: Finally we had enough material to achieve prompt criticality in an experiment called tickling the dragon's tail. We had a near-critical assembly of uranium hydride with a hole though the middle of it. We would shoot a small slug of uranium hydride through the hole, and for an instant there was enough material to make the assembly prompt critical. It was pretty exciting.

For some time there wasn't enough plutonium for any kind of integral measurement, so there was very heavy emphasis on differential measurements for that fissile material. The tamper materials were available, however, and John was doing integral measurements on them. Then the first significant quantities of plutonium began coming from Oak Ridge. At a fraction of a gram you could begin to measure some multiple effects. As more arrived, we were able to amass larger and larger quantities and get closer to what a real bomb would be like.

MANLEY: I was here when the first significant amounts of plutonium were delivered. Dick Baker fabricated some into a little sphere, and my group had to make neutron-multiplication measurements on it. That little sphere was so impressive to hold because it was warm from all the alpha activity. It was really quite a thrill to see this manmade element—it hadn't yet been discovered in nature—and to measure its neutron output,

DIVEN: By the time we had enough plutonium to make a bomb, nobody was interested in getting more differential data because it had been decided how to make the bomb. Everybody then began to work on how to diagnose what the bomb did. As a matter of fact, we ***had*** to stop making new differential measurements because that much plutonium didn't sit around in the lab with people petting it! It went right into making the Trinity device.

TASCHEK: In the last year the most important measurements were probably integral measurements. But the differential cross sections were used right up to the time of Trinity because they were needed for the first yield calculations from the Feynman and the Bethe-Feynman formulas. They were also used in calculations to check theory against the integral experiments. But of course no integral experiment short of detonating an actual weapon could include the implosion dynamics.

DIVEN: We did use the differential measurements to calculate the implosion, but in many respects the implosion device was a static device. The neutron generation time didn't change significantly over the many generations of neutrons produced before the bomb exploded. The thing we didn't know was the density of plutonium at different radii from the center during the implosion.

I should emphasize, however, that, as soon as the war was over, the differential measurements were once again the most important because they are the fundamental measurements. And for ten years or so after the war, a large effort was devoted to developing a reliable nuclear data base for weapon design.

TASCHEK: That's right. Nuclear data are needed because there is a basic technological difference between making a bomb and making, say, a steam engine. You can make anything from a little toy steam engine to a great big locomotive engine, and you can test it without completely destroying the engine. But a bomb does get completely destroyed in a full-scale test. So theory and computer simulation are very important in its design. And this is the main reason that computer development was worked on so hard at this Laboratory—to investigate the mechanics of implosion and to utilize all those complex nuclear data.

MANLEY: Apart from questions about critical mass, we had another big worry, and that was pre-initiation. If too many neutrons are around before the assembly of the critical mass is complete, you will get a fizzle. You want the neutrons to start the chain reaction at the moment the fissile material is in its most compact, or reactive, configuration.

DIVEN: At first the worry was that the alpha particles spontaneously emitted by plutonium and uranium would react with light-element impurities to make neutrons, and these neutrons would then initiate fission and produce a fizzle, Segre wanted plutonium for the gun design that was pure enough to eliminate this source of neutrons. But when the plutonium from the Oak Ridge reactor arrived, he discovered a contaminant—plutonium-240-that was undergoing spontaneous *fission.* It came as a big surprise.

MANLEY: We had gotten word from France about the spontaneous fission of polonium, although it wasn't definitive. That was the reason why Segre started doing spontaneous fission measurements.

TASCHEK: The discovery of spontaneous fission in plutonium-240 was really a blow to the bomb project because it meant that we couldn't use the gun design. Seth Neddermeyer's experiments with implosions really paid off then because the presence of plutonium-240 was not a problem with the implosion method of assembly. There wasn't enough time to build a plant to separate out the plutonium-240 for the gun device, so we went ahead with an expanded effort on the implosion work. As a result, the Los Alamos staff almost doubled.

SCIENCE: ***Can we talk a little bit more about the development of detectors and electronics for the nuclear data measurements?***

MANLEY: We mentioned that electronics was primitive. We had to design amplifiers and timing equipment to pick up appropriate signals from the particle detectors, which were usually ionization chambers. Then we made scalers to count the electronically recorded signals.

DIVEN: We had a large fraction of the very bright people working on electronics during the war because it was so important. We made enormous improvements in electronics.

MANLEY: I should emphasize that these developments were not the result of physicists and electronics people getting together. Rather, many of the good electronics people were the good physicists.

DIVEN: As for detectors, some of the detectors used then are still used in almost exactly the same way. Ion chambers aren't significantly different now than they were at the end of the war. During the war Geiger counters, proportional counters, and ion chambers were the work horses. What was needed most was better electronics to record the output of the detectors. Also we had to arrange the Geiger counters or proportional counters in some kind of geometry that would let us do what we wanted to do. For example, the long counters were designed to detect neutrons with uniform sensitivity over a wide energy range.

TASCHEK: Initially we used ion collection—the old academic tradition—for most measurements. But ion collection was slow, and in addition the detectors were so sensitive to vibrational noise that they had to be suspended very carefully so they wouldn't vibrate during the long collection times. One improvement that combined electronics design and insight was the collection of electrons rather than ions. Since electrons move much faster than ions, the counting rates were higher, the collection times were shorter, a good share of the vibrational noise problem was eliminated, and the signal-to-background ratio was improved. I did one of the first fast-neutron measurements on plutonium, and with ion collection the measurement was almost impossible because the alpha background of plutonium, which has a relatively short half-life, was so vast. On the other hand, with electron collection the counting rate was a thousand times faster, and the measurement was sort of a lead-pipe cinch. The electron collection idea came from Rossi and Segre.

DIVEN: When a charged particle enters a gas-filled ionization chamber it produces some ions and free electrons in the gap between two charged parallel plates. The electrons are attracted to the positive electrode and the ions move the other way. However, in most gases the electron attaches itself instantly to a gas molecule and forms a negative ion. The positive and negative ions drift slowly apart, taking about a millisecond to go some distance. Since electrons with their much smaller mass would move more rapidly across the chamber, Rossi searched for gases in which the electrons would remain free. Among those he found, there was a huge variation in the speed with which the electrons would move. Eventually Rossi found that in argon electrons moved roughly a thousand times faster than the ions, so counts could be registered a thousand times faster.

TASCHEK: The gas became impure very fast, but a recirculating system was developed that kept the system working.

SCIENCE: ***To return to the experimental work itself what nuclear data measurements were crucial to the development of thermonuclear weapons?***

TASCHEK: The most crucial was the measurement of the cross section for fusing deuterium and tritium. The original idea for a thermonuclear weapon was based on using the energy released in fusing two deuterons $[\mathrm{D} + \mathrm{D} \rightarrow {}^3\mathrm{He} + n]$. But then tritium was seen at the Berkeley cyclotron in some highly irradiated targets, and Bethe persuaded the Purdue group to measure the DT fusion cross section $[\mathrm{D} + \mathrm{T} \rightarrow {}^4\mathrm{He} + n]$. They accelerated tritium, which probably came out of the accelerator as HT or something like that. Neither Bethe nor anybody else anticipated such a big cross section for the DT reaction. But the Purdue group didn't have enough energy resolution to really understand their results. Then the work on the DT cross section was transferred to Los Alamos, In 1944 or thereabouts Bretscher and his group measured the DT and DD cross sections again. At that time Los Alamos had the world monopoly on tritium, and Bretscher's group had enough to make a target from water enriched to 25 or 50 per cent in tritium. The water was frozen onto a plate and bombarded with deuterons. They measured quite a piece of the DT cross section as a function of deuteron energy, and although the energy resolution in the low-energy region of interest was not all that good, they were able to determine that the DT cross section was higher than the DD cross section by a factor of 10 or more. That was the most important breakthrough for thermonuclear weapons.

MANLEY: It is amazing how early that work started. In the summer of '42, which was before the Purdue group was established, all the theorists, including Bethe, Teller, and so on, were together under Oppenheimer at Berkeley. In May of '43 Oppenheimer was put in charge of the Rapid Rupture Project, a delightful code name for fast fission. That group in Berkeley was giving theoretical direction to all the contracts connected with bomb development, and I was chasing around the country trying to see that the contracts got done, the experimental measurements got done, and so on. Whether the direction for the DT work at Purdue came directly from Bethe or Teller or by way of Oppenheimer and me doesn't matter.

TASCHEK: As far as Schreiber, who was in charge of the Purdue project, was concerned, his channel was through Bethe. The only surprising thing was that Bethe didn't predict the large cross section that was found.

DIVEN: It's interesting that the first laboratory building finished at Los Alamos was the cryogenics building to make liquid deuterium for a hydrogen bomb. By the time the building was finished, it was realized that hopes for developing a hydrogen bomb in the time available were futile, and so the building was used as a warehouse.

MANLEY: We might add that no one knew how to make a fusion bomb until 1951.

TASCHEK: After the big push for the H-bomb started in 1950, Jim Tuck and his group remeasured the DD, DT, and D^{3}He fusion cross sections. Since heating the material to thermonuclear temperatures would be very difficult, it was important to have accurate measurements of the low-energy region. The cross section varies extremely rapidly below deuteron energies of 150 keV, and the results of previous measurements were in disagreement. Tuck used very thin gas-cell targets to minimize uncertainties introduced by energy losses of the incident deuterons in the target material and was able to achieve what are still considered the definitive measurements of the DD and DT cross sections.

SCIENCE: ***What were some other important or surprising nuclear measurements done at Los Alamos?***

MANLEY: Measurement of $\bar{\nu}$ for fast neutrons. That wasn't done anyplace else.

TASCHEK: Another important first at Los Alamos was observation of the width of the neutron resonances in uranium-235. The fact that these resonances were so narrow in energy and therefore long-lived was initially surprising to the theoreticians. They expected any resonant structure to be very wide.

DIVEN: One surprise was the amount of tritium produced from lithium-7 $[{}^7\mathrm{Li} + n \rightarrow n' + \mathrm{T} + {}^4\mathrm{He}]$. Only after we had unexpectedly large yields from the first solid-fuel thermonuclear devices because of this reaction did we measure its cross section accurately.

TASCHEK: In a more philosophic vein we developed a systematic approach for going from first principles to the development of a complex device. The necessary steps between science and technology were worked out and in the last thirty years have been applied to many other technologies. Inventions such as Edison's electric light have a scientific basis behind them, but they were made by playing around in the lab. Now most things are too complicated for that to take place.

MANLEY: The fast-neutron measurements made at Los Alamos on almost any isotope in the natural world made a big impact in the outside literature.

TASCHEK: That's right. Our fast-neutron work dominated all other similar work for at least ten years. This work was important as pure science and it also formed a large part of a solid quantitative basis for weapon design.

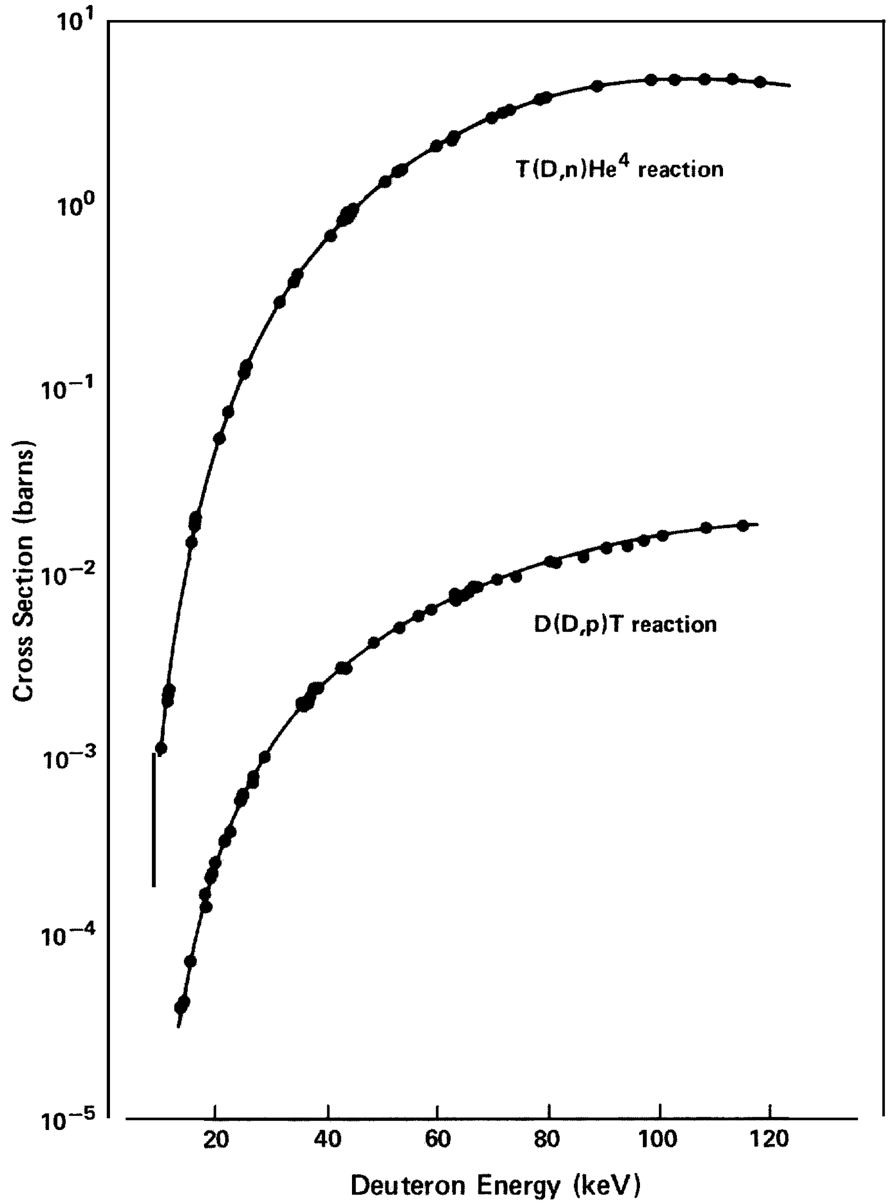
DIVEN: That work was also directly applicable to fast reactors. Probably for twenty years after the war most of the fast reactor data involving fast neutrons came out of Los Alamos.

TASCHEK: And those data were used in thermal reactor work as well because, depending on how a reactor is designed, how much moderator is used, and so on, a good fraction of the fission in a thermal reactor is fast-neutron fission.

I'd like to point out that prior to the Los Alamos work most measurements in both charged-particle physics and neutron physics were just relative measurements. People didn't bother to measure anything very accurately. They got a counting rate, but they didn't know the cross sections very well as a function of energy. Data like that can't be put into a design.

MANLEY: Calibration of the sources was the key to getting reliable numbers. We set up a special small lab just for that purpose.

DIVEN: The systematic approach to fast-



Definitive measurements were made at Los Alamos in the early 1950s of the cross sections for two fusion reactions that might form the basis of a thermonuclear weapon. From W. R. Arnold, J. A. Phillips, G.A. Sawyer, E. J. Stovall, Jr., and J. L. Tuck, Los Alamos Scientific Laboratory reports LA-1479 and LA-1480 (January 1953).

neutron data also had an important influence on postwar theory. For example, the statistical models of nuclear reactions were developed as a result of that work.

SCIENCE: ***How has the relationship between theory and experiment evolved over the years?***

TASCHEK: Before the war nuclear theory was really crude. I went back and looked at the first review papers of Bethe and Bacher. They contain an awful lot, but a lot is missing too. The situation is quite changed around now: theory can explain everything that experiment can do plus a little more. Nowadays you are likely to believe the theory.

DIVEN: In some cases relevant to weapon phenomena, you have to believe the calculated cross sections because the isotopes present are so short-lived that they disintegrate before you can collect them to do the experiments.

TASCHEK: However, the detail of the calculations is often still not adequate to the design problem. For instance, we are still measuring the uranium-235 fission cross section, and we can measure it to an accuracy of about 2 per cent. Theory won't predict it that well. Another example is the DT cross section, which is a simple problem from the theoretical point of view, but its absolute value still cannot be calculated as well as it can be measured.

MANLEY: Dick and Ben are giving answers to the question in which the word "theory" relates to models of a nucleus that help us understand or predict results of experiments on particular nuclei. There is also "theory" that predicts the behavior of a system of interacting nuclei, such as in a nuclear reactor or bomb. With enough experimental information on cross sections, etc., one can do quite well in making "theoretical" calculations of system behavior without "nuclear theory." Examples are critical masses, bomb efficiencies, reactor neutronics, and the like. These calculations more than nuclear theory occupied efforts here for many years and were the major reason for the important developments at Los Alamos in computers that have resulted in very sophisticated nuclear weapon design calculations,

TASCHEK: One experiment we have not talked about yet and might be good to end on was the Trinity experiment,

MANLEY: Yes. One of the most valuable pieces of data from the war years was the generation time measured at Trinity—the alpha experiment. Alpha is a measure of how the neutron population increases with time. It is closely related to bomb efficiency.

DIVEN: The number of neutrons produced as a function of time is eat, where a is a constant if the density and size of the energy-producing region don't change significantly during the explosion. Alpha is still one of the most important diagnostics for all of our tests. If you want the simplest possible test, you measure nothing but the yield—the total bomb energy—and alpha; these parameters will tell you the most about how well or how poorly the bomb worked.

TASCHEK: Many other nuclear experiments were set up at Trinity to do diagnostics, that is, to diagnose the causes if the yield was not anywhere near the theoretical expectation.

MANLEY: That was the purpose of the Trinity experiment after all. We didn't know what the yield was going to be, so we had to prepare for everything from zero to twenty kilotons and to give the answers for why it was any one of those figures from zero to what it was.

TASCHEK: We measured many things that had not really been looked at adequately. Prompt gamma rays were measured in a uniquely definitive way for the first time at Trinity.

MANLEY: In terms of comprehensive data collection, the Trinity experiment was one of the most amazing field experiments ever. Every measurement, as far as I know, was significant in one way or another. It was probably the only field experiment where you had only one shot at it. And that is still one of the problems at Nevada. There is a lot riding on each individual shot. You can't go back the next day and tweak things up and try again like you can in the laboratory. It is too expensive.

It must be intriguing to listen to us talk with such obvious enjoyment about these things that were really a hell of a lot of work. ■

Early Reactors

From Fermi's Water Boiler to Novel Power Prototypes

by Merle E. Bunker

In the urgent wartime period of the Manhattan Project, research equipment was being hurriedly commandeered for Los Alamos from universities and other laboratories. This equipment was essential for obtaining data vital to the design of the first atomic bomb. A nuclear reactor, for example, was needed for checking critical-mass calculations and for measuring fission cross sections and neutron capture and scattering cross sections of various materials, particularly those under consideration as moderators and reflectors. But a reactor was not an item that could simply be requisitioned from some other laboratory.

Enrico Fermi advocated construction at Los Alamos of what was to become the world's third reactor,* the first homogeneous liquid-fuel reactor, and the first reactor to be fueled by uranium enriched in uranium-235. Eventually three versions were built, all based on the same concept. For security purposes these reactors were given the code name "Water Boilers." The name was appropriate because in the higher power versions the fuel solution appeared to boil as hydrogen and oxygen bubbles were formed through decomposition of the water solvent by the energetic fission products.

The first Water Boiler was assembled late in 1943, under the direction of D. W. Kerst, in a building that still exists in Los Alamos Canyon. Fuel for the reactor consumed the country's total supply of enriched uranium (14 percent uranium-235). To help protect this invaluable material, two machine-gun posts were located at the site.

The reactor (Fig. 1) was called LOPO (for low power) because its power output was virtually zero. This feature simplified its design and construction and eliminated the need for shielding. The liquid fuel, an aqueous solution of enriched uranyl sulfate, was contained in a l-foot-diameter stainless-steel spherical shell surrounded by neutron-reflecting blocks of beryllium oxide on a graphite base. Neutron-absorbing safety and control rods passed through channels in the reflector. Soon to become known as soup, the fuel solution was pumped into the containment shell from a conical storage basin. Since the reactor was intended for low-power operation, no provision for cooling was required.

Many illustrious scientists were involved in the design, construction, and early operation of LOPO, including Richard Feynman, Bruno Rossi, Frederic de Hoffmann, Marshall Holloway, Gerhart Friedlander, Herbert Anderson, and Enrico Fermi. According to R. E. Schreiber, the Laboratory's deputy director for many years, the Water Boiler was Fermi's plaything. "He would work on weapon physics problems in the morning and then spend his afternoons down at the reactor. He always analyzed the data as it was being collected. He was very insistent on this point and would stop an experiment if he did not feel that the results made sense." On the day that LOPO achieved criticality, in May 1944 after one final addition of enriched uranium, Fermi was at the controls.

LOPO served the purposes for which it had been intended: determination of the critical mass of a simple fuel configuration and testing of a new reactor concept. The critical mass, for the geometry used, was found to be the exceptionally low value of 565.5 grams of uranium-235. After these measurements and a series of reactivity studies, LOPO was dismantled to make way for a second Water Boiler that could be operated at power levels up to 5.5 kilowatts and thus provide the strong source of neutrons the Laboratory needed for cross-section measurements and other studies. Named HYPO (for high power), this version (Fig. 2) was built under the direction of L. D. P. King and R. E. Schreiber. The soup was changed to a solution of uranyl nitrate, and cooling coils were installed within the fuel vessel. In addition, a "Glory Hole" through

The first two were Fermi's "pile" at Chicago's Stagg Field and the X-10 graphite reactor at Oak Ridge.

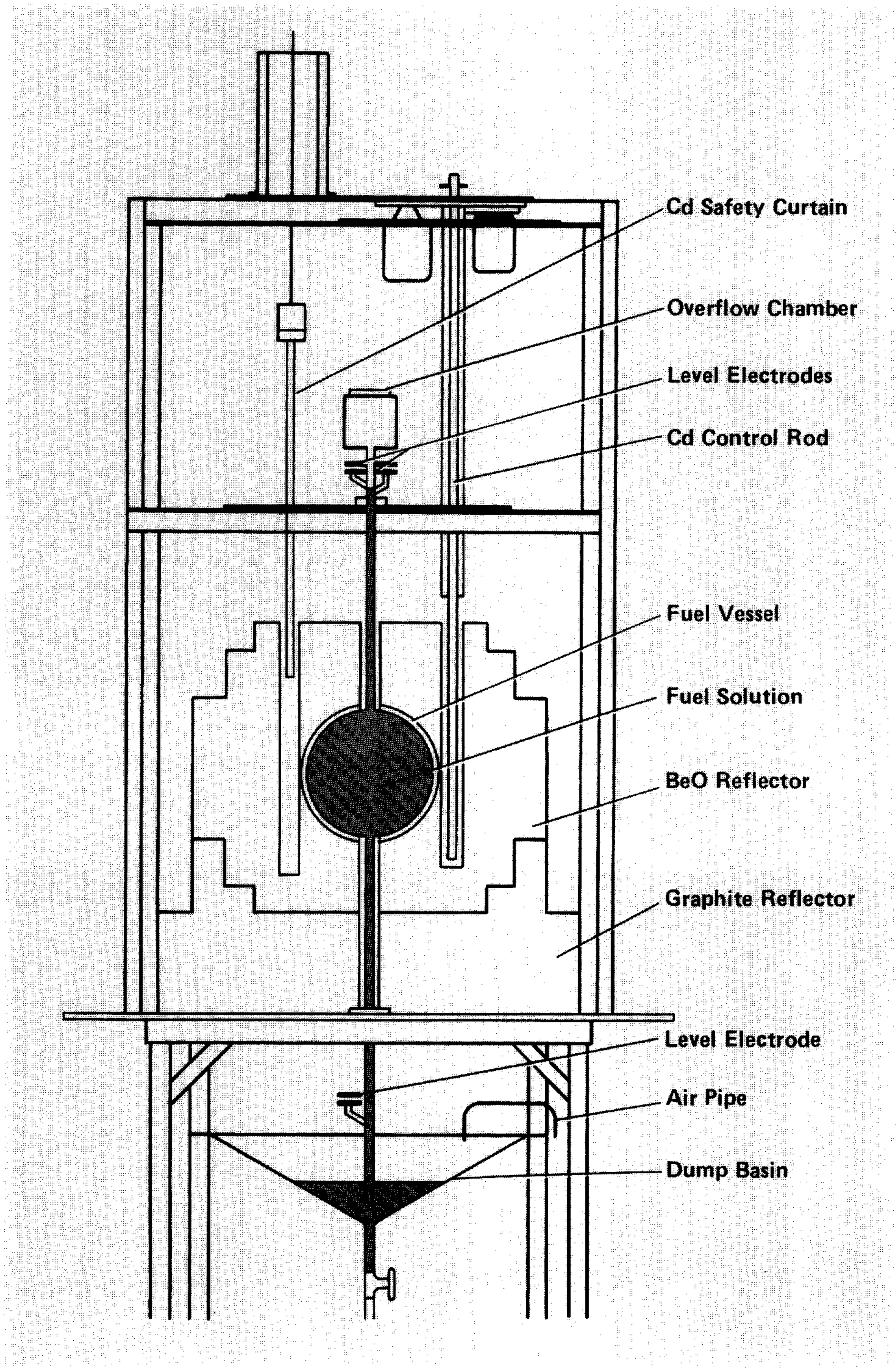


Fig. 1. Cross section of LOPO. This assembly was located inside a cubical fiberboard enclosure, 12 feet on a side, that was temperature-controlled to a fraction of a degree. Such a precaution was deemed necessary since no information existed about the temperature dependence of the reactivity.

the spherical container allowed samples to be placed in the most intense neutron flux. A massive concrete shield was built to surround the core and the large graphite thermal column that radiated from it. The reactor became operative in December 1944. Many of the key neutron measurements needed in the design of the early atomic bombs were made with HYPO.

By 1950 higher neutron fluxes were desirable, as well as more research facilities. Consequently, extensive modifications were made to permit operation at power levels up to 35 kilowatts and production of neutron fluxes above 10^{12} neutrons per square centimeter per second. This version of the Water Boiler was, of course, named SUPO (Fig. 3).

Completed in March 1951, the conversion from HYPO to SUPO included the following modifications.

- Three 20-foot-long stainless-steel cooling coils were installed in the l-foot-diameter spherical fuel vessel for greater heat-removal capacity (Fig. 4).
- The enrichment of the uranyl nitrate soup was increased from 14 to 88.7 percent uranium-235.
- The beryllium oxide portion of the reflector was replaced with graphite to permit a more rapid and complete shutdown of the reactor.
- A gas recombination system was connected to the fuel vessel to eliminate the explosive hazard posed by the radiolytic hydrogen and oxygen evolved during power operation. The system included a chamber containing platinized alumina, which catalyzed recombination of the exhaust gases at a temperature of about 440 degrees Celsius. The water formed was then returned to the fuel vessel. Incredibly, the original catalyst chamber performed satisfactorily for 23 years.

SUPO was operated almost daily until its deactivation in 1974. Its neutrons were used for many measurements important to the weapon program. As an example, for nearly 20 years the most accurate values for weapon yields were obtained by a radiochemical method that involved comparison of the responses of two fission counters placed in one of SUPO'S thermal columns. One counter detected the fissions in a standard amount of uranium-235, and the other the fissions in a small sample of the bomb debris. This measurement, coupled with an assay of fission products in the bomb debris, revealed what fraction of the original uranium-235 had fissioned and, hence, the bomb yield. Also, with a series of uranium-235 foils in its Glory Hole, SUPO could provide a beam of neutrons having an almost pure fission-spectrum energy distribution. This beam was used for an important series of weapon-related cross-section measurements. In addition, fundamental studies of the fission process, involving advanced time-of-flight techniques, were conducted at the reactor for many years.

During the 1950s the Water Boiler was used by the Laboratory's Health Division in pioneer research on effects of neutron, beta, and gamma radiation on live animals, including mice, rats, rabbits, and monkeys. Effects studied included life shortening, loss of reproductive power, and the development of cataracts, various forms of cancer, and blood disorders. Also, evidence for genetic effects was sought in hundreds of mice studied over many generations. Aside from their basic scientific value, these data provided major guidance in setting radiation-exposure limits for humans.

Experiments on the transient behavior of SUPO were also carried out in the early '50s. The reactivity of the reactor was rapidly increased by ejecting a neutron absorber from the core region in about 0.1 second. It was found that immediately following the reactivity increase, a sizeable reactivity decrease occurred. The reactivity decrease was due primarily to increased production of radiolytic hydrogen and oxygen, which caused a decrease in the density and, hence, in the neutron-moderating ability of the soup. This built-in safety feature of water-boiler reactors had, in fact, already been demonstrated in an unplanned excursion during the assembly of SUPO. The excursion occurred when a staff member was testing the piston-like mechanisms that cushion the fall of dropped control rods. At one point he lifted two control rods simultaneously, and the reactor went supercritical. The ensuing excursion lasted only a fraction of a second. Radiation alarms were activated as some soup was pushed to the top of the reactor through pressure-sensing tubes, but fortunately the reactor was not damaged and the staff member received only a modest

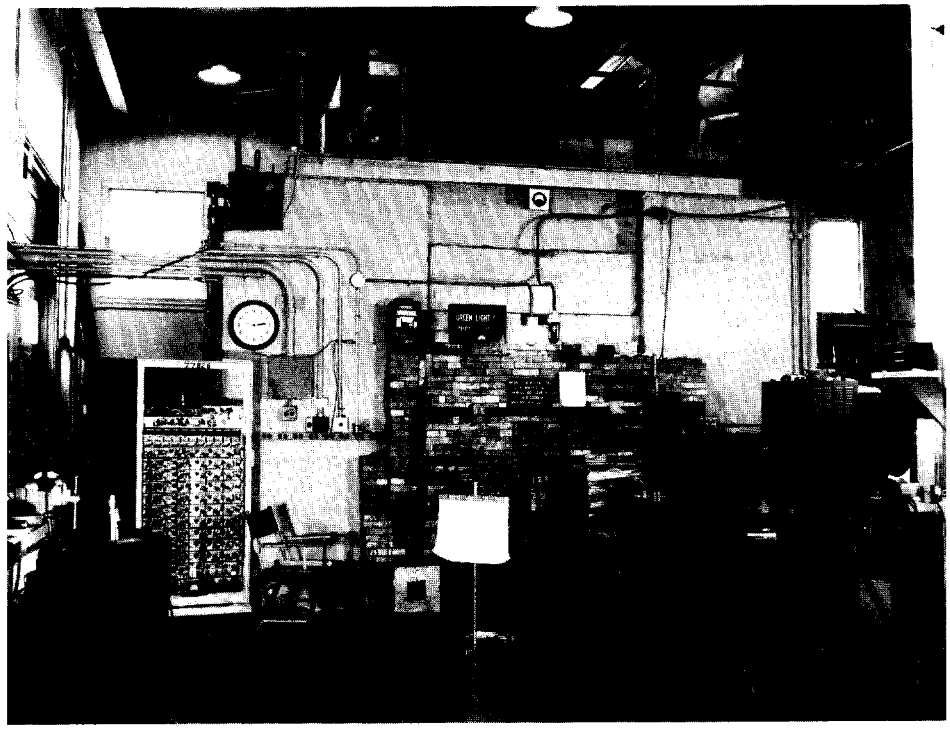


Fig. 2. South face of HYPO, 1948. The plywood boxes on top of the reactor are filled with boron-paraffin for neutron shielding. The device below the clock is the first 100-channel pulse-height analyzer built at Los Alamos.

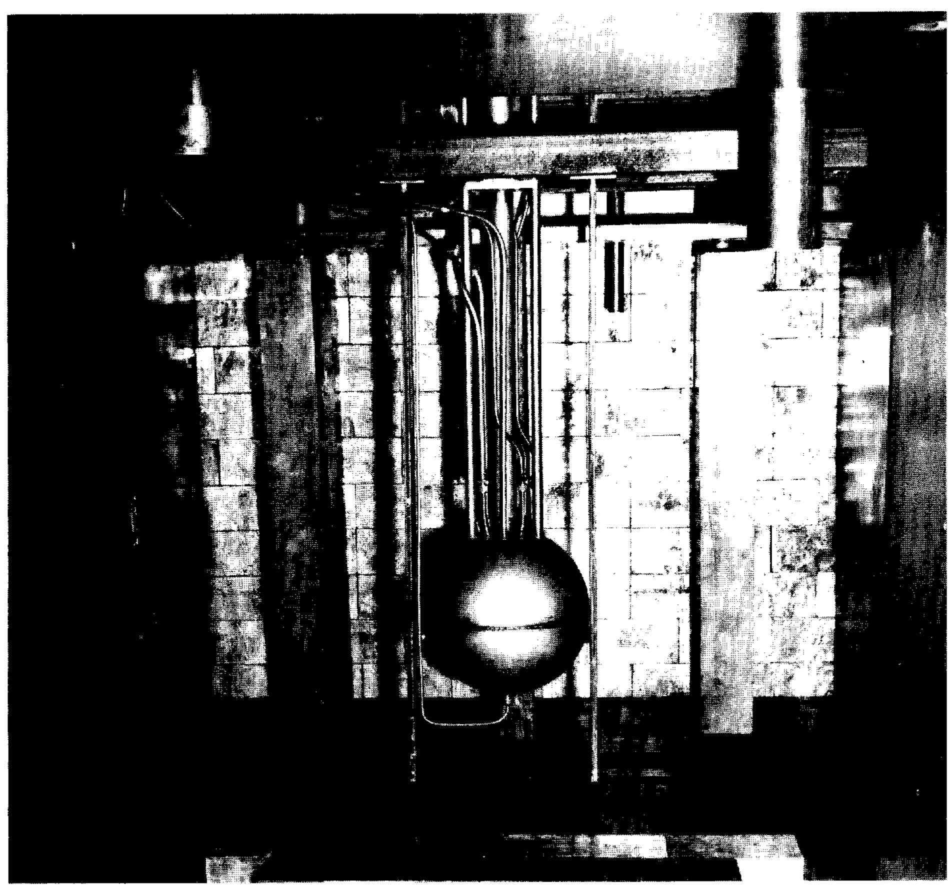


Fig. 3. The SUPO sphere prior to installation of the surrounding graphite reflector. The plates tangent to the sphere are control-rod sheaths. The tube leaving the bottom of the sphere is for addition or removal of fuel solution.

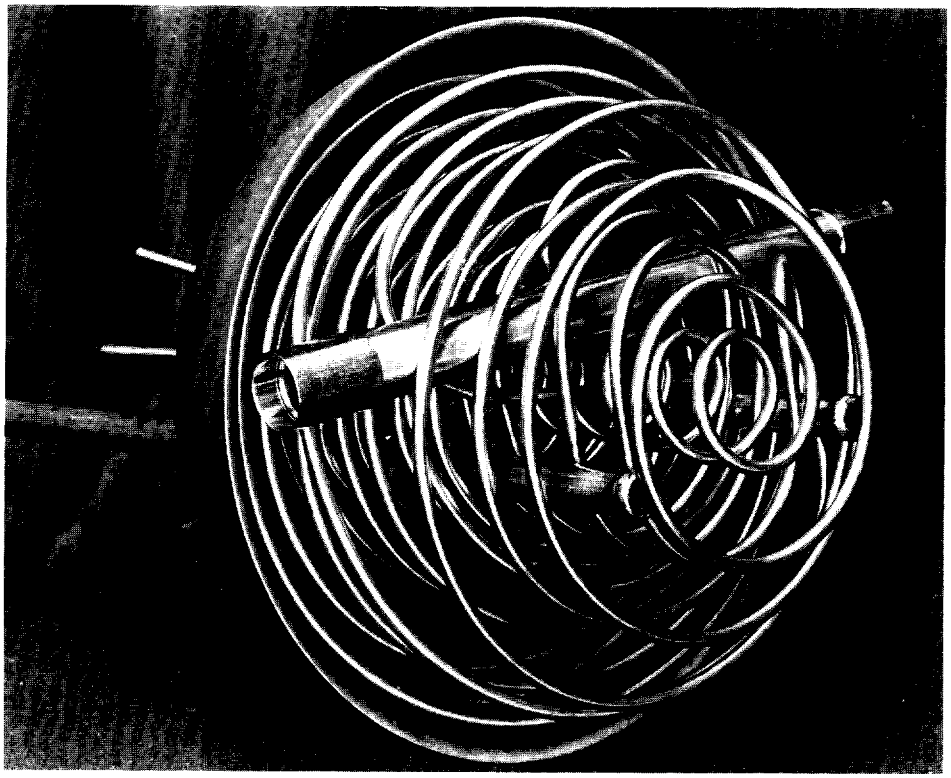


Fig. 4. Arrangement of cooling coils within the SUPO sphere. The pair of re-entrant tubes are control-rod sleeves, and the larger tube is the sleeve for the so-called Glory Hole.

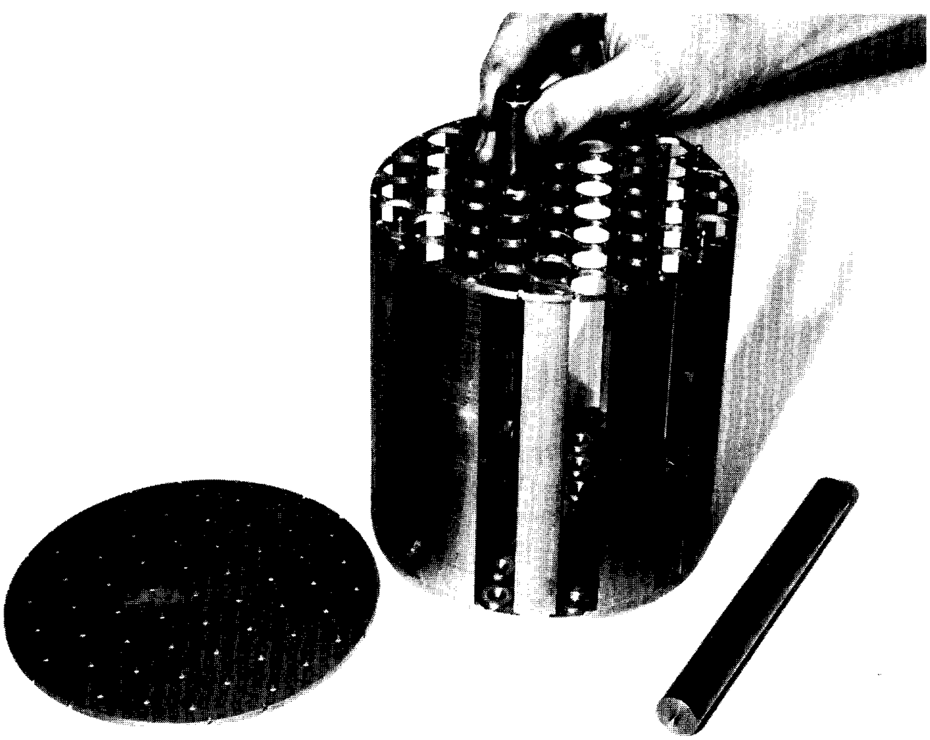


Fig. 5. Clementine fuel rod cage, constructed of mild steel. Mercury coolant circulated through the cage.

radiation dose.

The inherent safety, low cost, low fuel consumption (about 2 grams per year), and flexibility of the Water Boiler led to the construction of numerous solution-type research reactors. Between 1952 and 1974, Atomics International built at least 17 such reactors. some of which are still in operation, for institutions in the United States, Japan, Denmark, Germany. and Italy.

Los Alamos Canyon also has the distinction of being the site of the world's first fast plutonium reactor. Such a reactor was proposed and approved in 1945 on the basis that it would provide a much-needed high-intensity fission-neutron source and would be a means of exploring the adaptability of plutonium as a reactor fuel. The fact that a sufficient amount of plutonium was available at Los Alamos obviously influenced the selection of the fissile material.

In a fast reactor controlled fission is achieved with high-energy, or fast, neutrons. Since no moderating material is necessary, the proposed reactor could be of small size. More important, with no moderator the neutrons in the core region would have a fission energy spectrum except for a small perturbation caused by inelastic scattering in the fuel and other heavy materials. High intensities of such neutrons were at that time unavailable at the Laboratory but were needed for nuclear research and for acquiring data needed by the bomb designers. In addition, operation of the reactor would supply information about fast reactors, such as ease of control and nuclear breeding properties, that would be relevant to their possible use as devices for production of power and fissile materials.

The site chosen for the fast reactor was adjacent to the Water Boiler building. Construction began in August 1946 under the direction of Phillip Morrison. Near the time of first criticality a few months later, Morrison dubbed the reactor "Clementine," a name borrowed from the song "My Darling Clementine," which starts out "In a cavern, in a canyon, . . ." and is about the legendary forty-niners. Morrison's inspiration was that the reactor personnel were modern-day forty-niners inasmuch as 49 was the code name for plutonium (for $Z = 94$, $A = 239$). Clementine's plutonium fuel was in the form of small rods clad in steel jackets (Fig. 5), around which mercury coolant flowed at the rate of approximately 9 liters per minute. The mercury flow was maintained by an ingenious pump that contained no moving parts. Surrounding the fuel vessel was a 6-inch-thick reflector of natural uranium, most of which was silver-plated to reduce corrosion. Immediately outside the uranium blanket were 6 inches of steel reflector and 4 inches of lead shielding. Reactor control was effected by the positioning of uranium rods, a *positive* reactivity-control method in contrast to the poisoning method used in conventional reactors.

The final stages of construction and eventual power operation of the reactor were under the direction of David and Jane Hall. Although core criticality was achieved in late 1946, completion of the reactor took 27 more months. During this hectic period many nuclear measurements were made at low power, including determination of the neutron energy spectra in the core and the various experimental ports, the effect of alpha-phase plutonium and temperature on reactivity, danger coefficients, and activation cross sections. Also, as experience was gained in operation of a fast reactor, a number of changes were made in the control system.

After the design power of 25 kilowatts was reached in March 1949, Clementine (Fig. 6) maintained a full schedule for nearly a year, during which time several important weapon experiments were conducted. In March 1950 the reactor was shut down to correct a malfunction in the operation of the control and shim rods; during this shutdown a ruptured uranium rod was discovered and replaced. Reactor operation was resumed in September 1950 and continued until Christmas week of 1952, when it became evident that a fuel rod had ruptured and released plutonium into the mercury coolant. The hazard created by this situation and indications of serious abnormalities in the uranium reflector region prevented further operation of the reactor and prompted the decision to proceed with a complete disassembly.

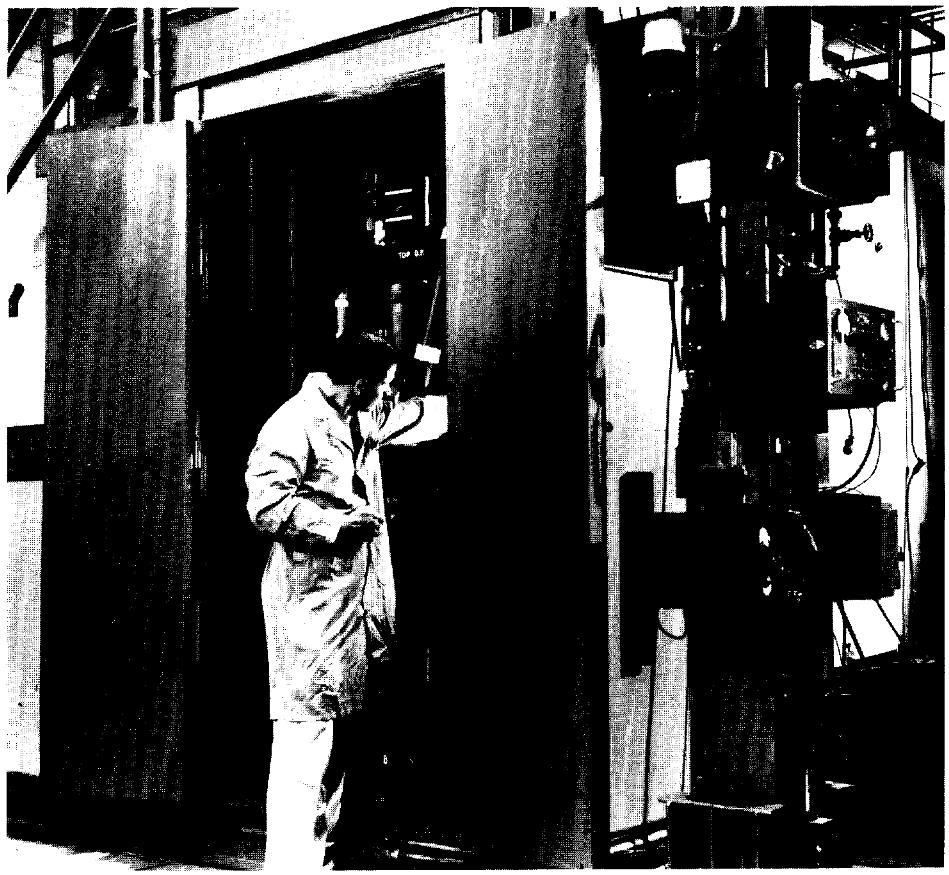


Fig. 6. Clementine's north face showing the enclosure for the mercury cooling system.

During the last year that Clementine was operated, the total neutron cross sections of 41 elements were measured with an accuracy of + 10 percent over a neutron energy range of 3 to 13 million electron volts. These data were of great utility to theorists engaged at that time in the design of both fission and fusion bombs. In spite of Clementine's early demise, most of the original objectives of the project were realized. Important weapon data had been acquired, and invaluable experience had been gained in the design and control of fast reactors. One of the lessons learned was that mercury was an unacceptable choice of coolant, largely due to its poor heat-transfer properties.

Planning for Clementine's replacement began almost immediately. The Water Boiler was still available, but higher neutron fluxes were needed to provide adequate support for the weapon program and to take advantage of new avenues of research that were rapidly developing around the world. Basic research was gaining increasing support at the Laboratory, and Los Alamos needed facilities that would be competitive with those at other research institutions.

After a few months of study in which various designs were considered, a reactor patterned after the Materials Testing Reactor at Idaho Falls was deemed the most attractive. Since that reactor's uranium-aluminum plate-type fuel elements had already undergone extensive testing, little time would be lost in core design or in obtaining licensing for the reactor.

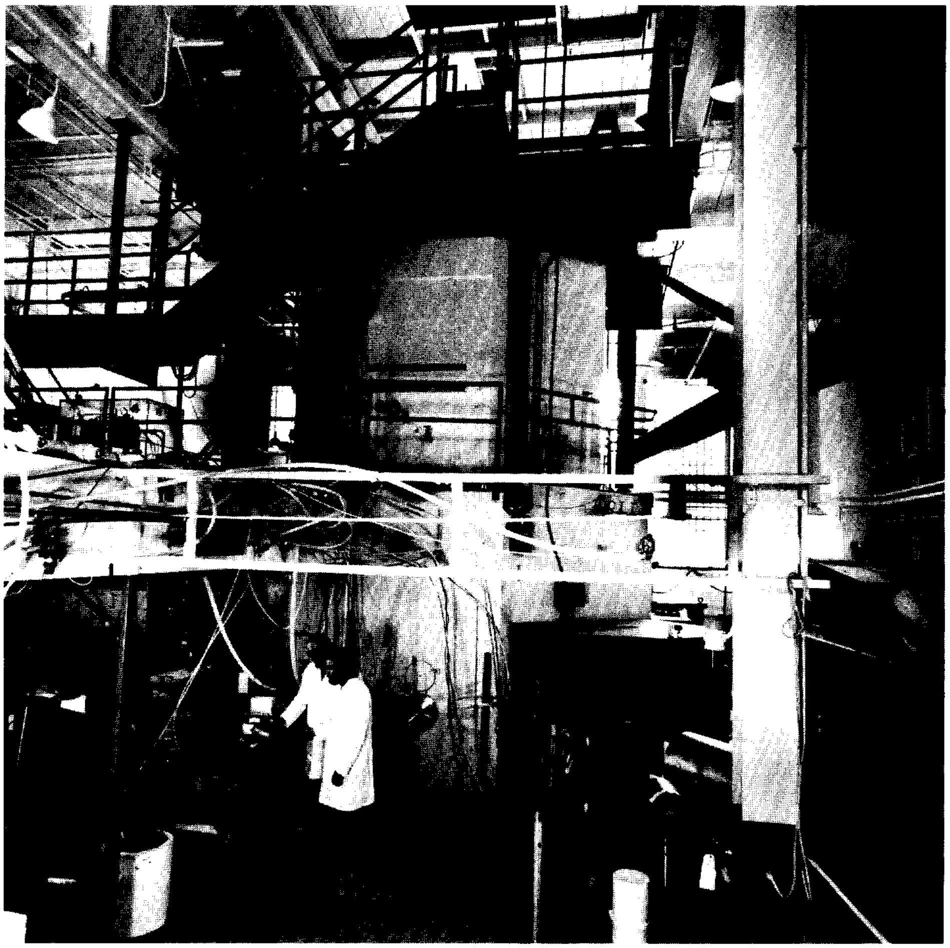


Fig. 7. North face of the Omega West Reactor. The numerous tubes above and behind the two employees are associated with automated neutron-activation analysis facilities.

The conceptual design was completed by the end of 1953. The fuel-element array was to sit at the bottom of an 8-foot-diameter, 24-foot-high tank of water and to be force-cooled by a water flow of 3500 gallons per minute. The proposed power level was 5 megawatts, but sufficient shielding was included for operation at 10 megawatts. Idaho Falls data indicated that the maximum thermal neutron flux at 5 megawatts would be 5 x 10^{13} neutrons per square centimeter per second. For reasons of expediency and economy, it was decided to build the reactor in the room previously occupied by Clementine.

As anticipated, approval to proceed was soon obtained, and construction began in mid 1954. The first criticality measurements were made in July 1956, and a few months later the so-called Omega West reactor (OWR) was operating at 1 to 2 megawatts. In a little over three and a half years, one reactor had been completely dismantled and another had risen in its place. Today, because of more extensive regulations and the many approvals required, such an operation would probably take at least 10 years,

Although many novel features were incorporated in the OWR (Fig. 7), it was built strictly as a research tool, not as a reactor experiment. As such, it has served the Laboratory remarkably well. During its first 16 years the reactor was routinely operated 120 hours per week; since 1972 it has been operated 40 hours per week. In 1968 the cooling system was modified in order to raise the power level to 8 megawatts and thereby increase the maximum thermal neutron flux to 9 x 10^{13} neutrons per square centimeter per second. At present the OWR is the highest power research reactor west of Missouri and the only reactor in operation at Los Alamos.

Major basic and applied research activities at the OWR have included measurement of weapon yields by comparison fission counting, neutron radiography of weapon components, studies of the structure and dynamics of condensed matter by neutron scattering, studies of the long-term behavior of components used in weapons, in-core testing of fuels and components for advanced power-reactor systems, measurement of post-shutdown heat evolution from reactor fuels, in-core testing of plasma thermocouples, studies of nuclear cross sections and energy levels by neutron-capture gamma-ray spectroscopy, nondestructive elemental assay of materials by neutron-activation analysis, and the production of radioisotopes for numerous Laboratory programs. Several hundred professional papers have been written about the results obtained through these activities.

In addition to the research reactors discussed above, three small power reactors of unique design were built and tested at Los Alamos between 1955 and 1963, beginning with LAPRE I and LAPRE 11 (LAPRE stands for Los Alamos power reactor experiment.) These two reactors, constructed at Ten Site by K-Division personnel, embodied an attempt to exploit some desirable properties of a fuel solution composed of highly enriched uranium dioxide (93.5 percent uranium-235) dissolved in 95-percent phosphoric acid. There was evidence that such a fuel solution would allow a reactor to operate as an essentially constant-temperature energy source whose output was determined only by external load demand. It was believed that such reactors might find application within the military establishment as portable power sources.

LAPRE II (Fig. 8), which was completed in 1959, exhibited the expected nuclear behavior up to its maximum power of 800 kilowatts. The temperature of the fuel solution and of the superheated steam output was set only by the uranium concentration in the fuel and by the position of an adjustable control rod. The principal problem encountered was that of achieving satisfactory fuel containment. Because high-temperature phosphoric acid is extremely corrosive, the stainless-steel fuel vessel and the heat-transfer coils had to be plated with gold. However, achieving absolute integrity of the gold cladding proved to be a persistent problem, and the project was terminated in 1960.

Another early project on power reactors was the development of a fast reactor fueled by molten plutonium and cooled by molten sodium. The thrust of this program was to explore the problems involved in using plutonium fuel in fast breeder reactors. The initial reactor design, designated LAMPRE I (for Los Alamos molten plutonium reactor experiment I), called for a 20-megawatt power level. The fuel was to be contained in a single connected region cooled by sodium flowing through tubes welded to the top and bottom plates of a cylindrical container. Soon after the detailed design of the core was begun, it became apparent that insufficient knowledge existed about the behavior of some of the core materials in a high-temperature, high-radiation environment. Consequently, the design of LAMPRE I was radically changed to that of a l-megawatt test reactor, which would provide much of the materials data needed to proceed with the 20-megawatt design (to be known as LAMPRE II). The core matrix was redesigned to accommodate up to 199 separate fuel elements, each consisting of plutonium-iron fuel material encased in a tantalum thimble. With this arrangement several fuel-element designs could be tested simultaneously.

The low power level of LAMPRE I made it possible to locate the facility in an existing building at Ten Site. A gas-fired 2-megawatt sodium cooling loop was also built to provide experience with high-temperature (600 degrees Celsius) sodium-to-water heat exchangers.

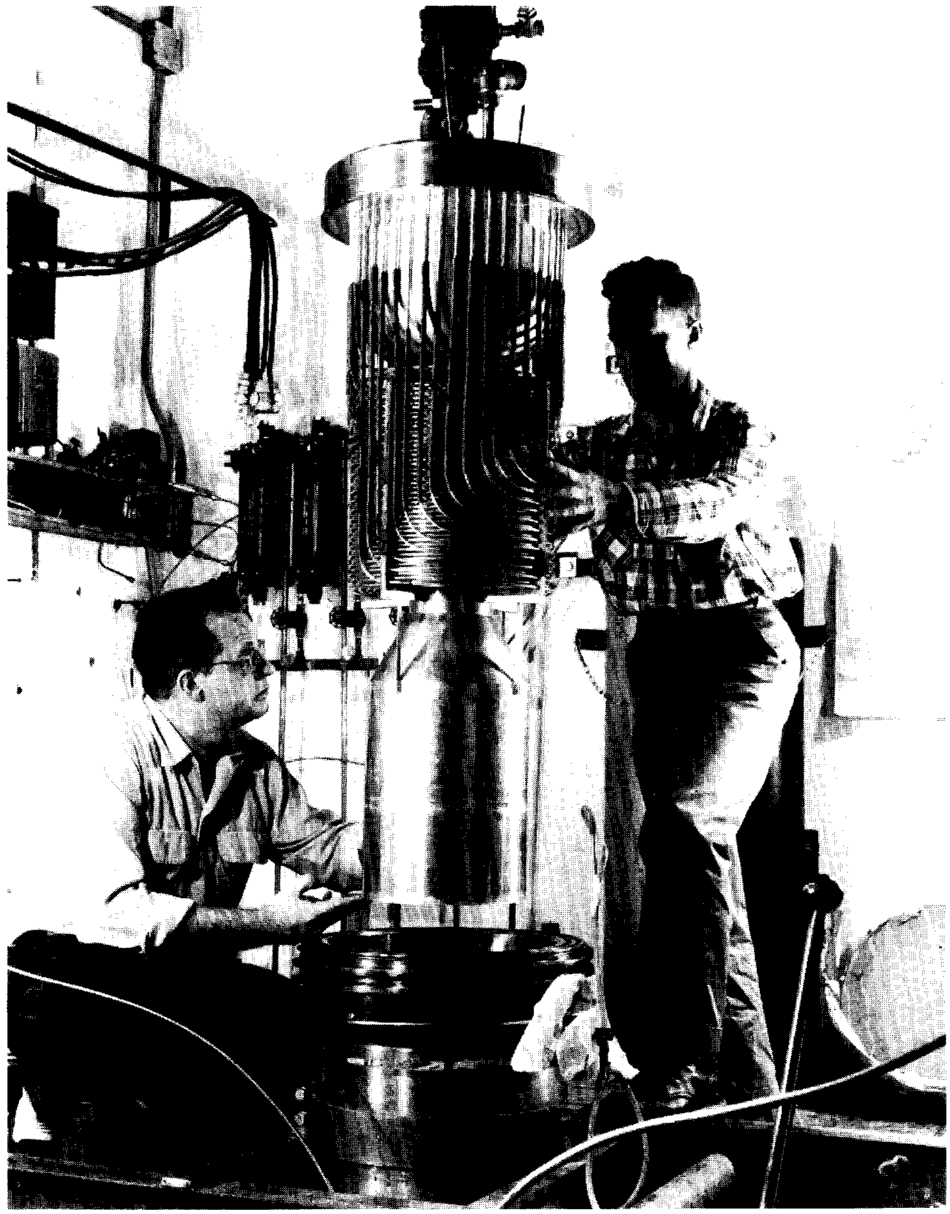


Fig. 8. LAPRE II core assembly. The baffle at the bottom enclosed the critical region. The upper section is the heat exchanger.

LAMPRE I was operated successfully for several thousand hours following initial criticality in early 1961. One of the major research efforts was learning how to minimize corrosion of the tantalum thimbles by the molten fuel and coolant. Among the fuel elements that exhibited no leakage after thousands of hours of high-temperature (450 to 600 degrees Celsius) operation were those composed of prestabilized plutonium-iron that contained no additives and tantalum thimbles that had been annealed at 1450 degrees Celsius.

By mid 1963 LAMPRE I had served its intended purpose and was shut down. Funding for the construction of LAMPRE II never materialized because the AEC Division of Reactor Development and Technology decided to divert all of its available resources into the further development of uranium oxide fuels, which appeared to be more versatile and more manageable than plutonium. The sodium cooling loop was also shut down in 1963 after more than 20,000 hours of operation-the most extensive and successful test of a high-temperature sodium cooling loop that had been conducted up to that time.

Utilizing the experience gained in the above pioneer endeavors, the Laboratory has continued to be active in the development of special-purpose reactors and reactors of advanced design, including nuclear "engines" for space vehicles. Altogether, the efforts here, whether successful or disappointing at the time, have had a significant impact on reactor technology and nuclear science in general. ■

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Plutonium

A Wartime Nightmare but a Metallurgist's Dream

by Richard D. Baker, Siegfried S. Hecker, and Delbert R. Harbur

In 1942 the theoretical outline for an atomic bomb was clear: compress enough fissionable material long enough to properly ignite a chain reaction. Construction of an actual weapon, however, required translation of "fissionable material" into real pieces of plutonium or uranium metal. These metals had to be free of impurities that would adversely affect the neutron flux during the chain reaction and yet be fabricable enough that precise shapes could be formed. Whether this would even be possible with plutonium was not then known, however, because plutonium was a new, manmade element and the metal had not been produced.

Accounts of the Manhattan Project have neglected (for security reasons, initially) the important metallurgical work that preceded fabrication of these materials into integral parts of real weapons. For example, the Smyth Report* devotes one short paragraph to the wartime work of the entire Chemistry and Metallurgy Division at Los Alamos—a division that in 1945 numbered 400 scientists and technicians. Our article will attempt to fill part of this gap for plutonium by highlighting key developments of the wartime research and will continue with some of the exciting research that has occurred since the war.

Research from 1943 to 1946

The Los Alamos work on plutonium and enriched uranium, the so-called special nuclear materials, was extensive, covering a variety of research problems ranging from purification of material received from reactors to the prevention of oxidation of the final product. Further, because many chemical processes and physics experiments required very pure materials, such as gold, beryllium oxide, graphite, and many plastics, considerable general materials research was also carried out.

Much of the chemistry and metallurgy of uranium was already known from the production of uranium metal for the uranium-graphite reactor pile at Chicago in 1942. The work remaining on enriched uranium included preparation of high-purity metal, fabrication of components, and recycling of residues. However, the most challenging research and development was carried out on the new element plutonium.

Table I gives the important dates in the early history of plutonium and shows the short time—four years—that elapsed between its discovery and its use in the first atomic device at Trinity. The discovery occurred, as predicted by nuclear theory, when uranium was bombarded with 16-million-electron-volt deuterons in the cyclotron at Berkeley. Within about a month it was shown that plutonium-239 fissioned when bombarded with slow neutrons, and a decision was made to build large reactors at Hanford for the production of plutonium—this before the uranium-graphite pile at Chicago had demonstrated that a sustained and controlled chain reaction was even possible! That demonstration soon followed, proving that large quantities of plutonium could be produced, although no plutonium was extracted from the Chicago reactor.

At this point only microgram amounts of plutonium had been separated from the targets used in the cyclotrons. Remarkably, the basic chemistry of plutonium was worked out at Berkeley and Chicago on this microgram scale, and it formed the basis for the scale-up—by a factor of a billion—needed for plants that would eventually separate plutonium from spent reactor fuel. At the same time the first micrograms of the metal were produced at Chicago by the

Henry DeWolf Smyth,* Atomic Energy for Military Purposes: The Official Report on the Development of the Atomic Bomb under the Auspices of the United States Government, 1940-1945 *(Princeton University Press, Princeton, 1945), pp. 221-222.

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Plutonium discovered	February 23,1941
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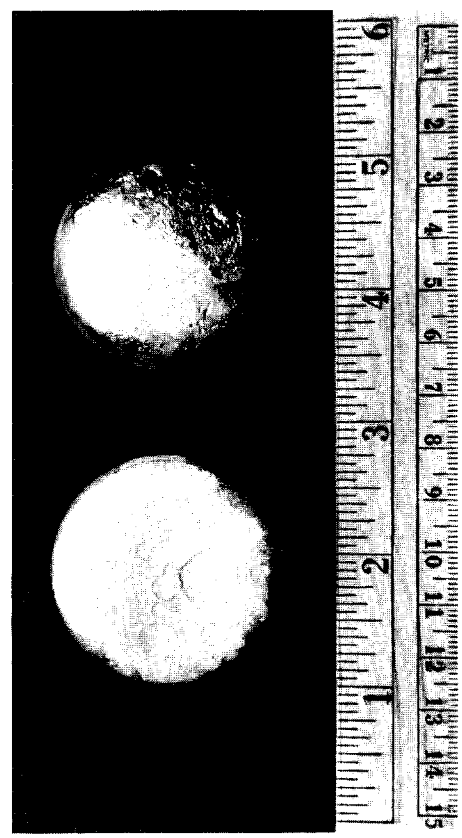
reduction of fluorides, and preliminary metallurgical properties were determined. However, the influence of impurities on such tiny samples distorted many of the results; for example, the melting point of plutonium was first thought to be about 1800 degrees Celsius, considerably above the true melting point of 641 degrees. Ultimately, the properties of plutonium were found to be incredibly sensitive to impurities.

It had been agreed that Los Alamos would not work on batches of plutonium of less than about 1 gram, and the microgram-scale work continued at Chicago. Finally, in early 1944 Los Alamos received plutonium nitrate samples containing half-gram amounts of the element from the "Clinton" reactor and pilot extraction plant at Oak Ridge. Later, larger amounts were received from the production facility at Hanford.

The plutonium nitrate arrived in relatively impure form, and techniques and equipment had to be developed for a number of processes, including purification, preparation of plutonium tetrafluoride and other compounds, reduction to metal, and metal fabrication. Also, because plutonium was in very short supply, it was imperative to develop processes to recycle all residues.

Initially, the purity requirements for the metal were very stringent because some elements, if present, would emit neutrons upon absorbing alpha particles from the radioactive plutonium, These extra neutrons were undesirable in the gun-type plutonium weapon then envisioned: they would initiate a chain reaction before the material had properly assembled into its supercritical configuration, and this "pre-initiation" would decrease the explosive force of the weapon. The purity requirement for certain elements was a few parts per million and for some, less than one part per million. As a result, all the materials used in the preparation of the plutonium metal, everything from the process chemicals to the containers, had to be of very high purity. This necessitated development work on many materials, including an extensive effort to obtain pure and nonreactive refractories to contain molten phutonium. The high purity requirements also necessitated the development of new methods for analysis of all materials, including plutonium.

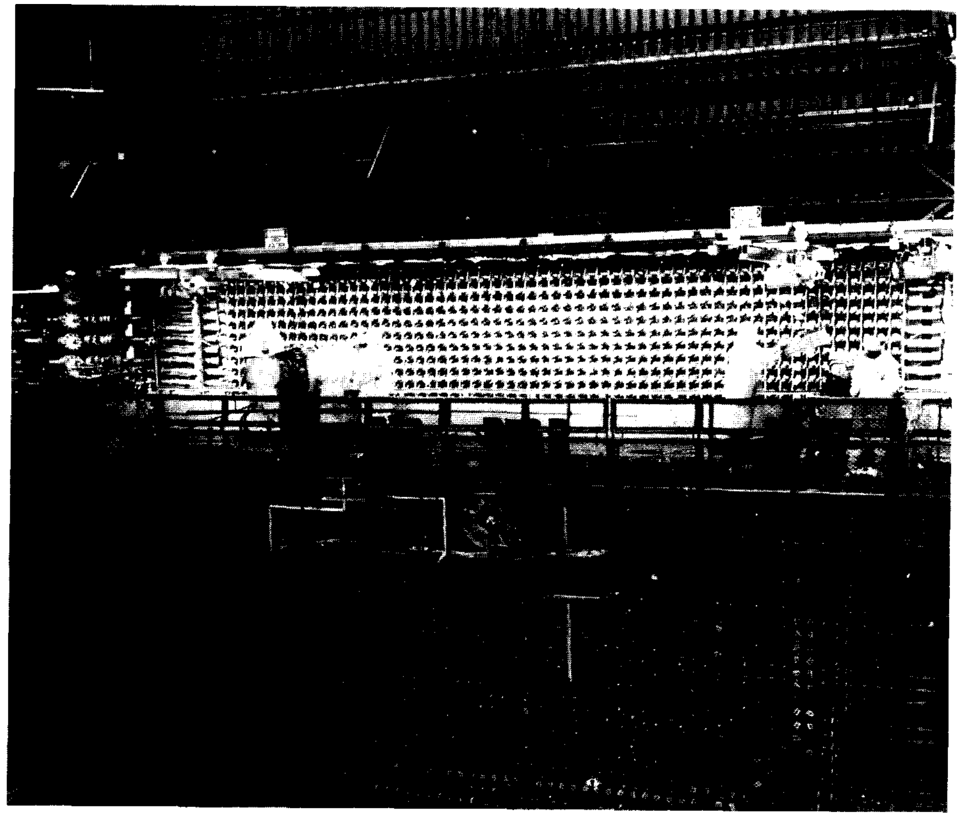
The potential health problem associated with the handling of plutonium had been recognized at Chicago, and work on the subject began with receipt of the first small amounts of plutonium. A Health Group was formed to monitor plutonium work areas, and, within the Chemistry and Metallurgy Division itself, committees were established to design suitable radiation detectors and apparatus for handling plutonium and to formulate safe handling procedures. Because alpha counters then lacked either sensitivity or portability and were in short supply, oiled filter paper was swiped over surfaces to pick up possible stray bits of plutonium and then measured at stationary counters. Similar procedures were used to detect suspected contamination of hands and nostrils. The air-conditioning system in the plutonium laboratory (D Building), which was installed initially to help maintain high purity by filtering out dust, ultimately served the more important function of confining the plutonium. The building was equipped with hoods with minimal ventilation and with the forerunner of the modern glove box—plywood "dry boxes." The successful handling of large quantities of plutonium without serious problems was at that time an outstanding achievement.



Two early discs of plutonium metal after reduction from the tetrafluoride. Plutonium generally arrived at the Laboratory from the Hanford reactors in the form of a relatively impure nitrate solution. Techniques were developed at Los Alamos for purification, preparation of various compounds, reduction to the metal, and metal fabrication.

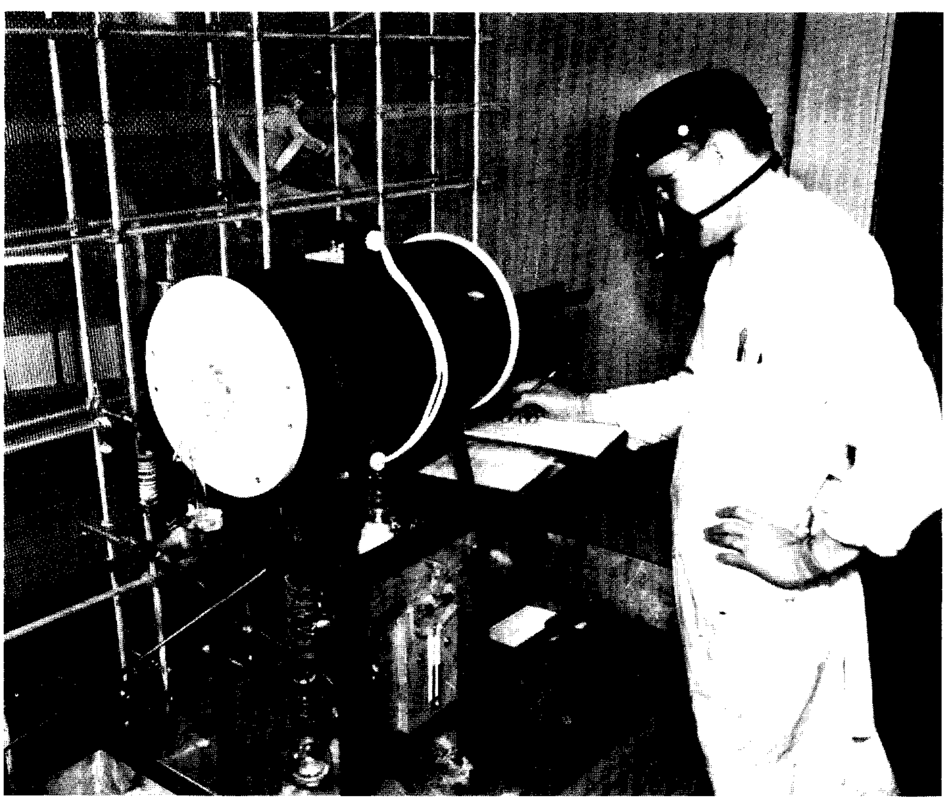
At first plutonium metal was prepared at Los Alamos either by lithium reduction of plutonium tetrafluoride in a centrifuge, the metal settling out as the closed reaction vessel rotated, or by the electrolysis of fused salts containing plutonium. Soon, however, calcium reduction of the tetrafluoride was perfected. The vessel used to contain this reaction was called a stationary "bomb" because the reaction was highly exothermic and the metal product settled out in the closed, nonrotating vessel simply by gravity. This technique became the preferred method and was used to prepare plutonium for almost all metallurgical studies and for the nuclear devices.

The microgram metallurgy at Chicago had provided values for the density of the metal that clustered about either 16 or 20 grams per cubic centimeter. This bimodal spread, due surely in part to impurities, nevertheless pointed toward interesting metallurgy by hinting that the element had more than one phase. Working with larger amounts, Los Alamos refined these measurements and by the middle of 1944 had discovered that plutonium was a nightmare: no less than five allotropic phases existed between room temperature and the melting point. Unfortunately, the room-temperature alpha (a) phase was brittle, and the metal experienced a large volume change when heated and then cracked upon cooling. These properties made fabrication very difficult, and there was not enough time for detailed fabrication development on the a-phase material. It was thought likely that another phase would be malleable and easily shaped; the problem was how to stabilize such a phase at room temperature. It was then discovered that alloying plutonium with small amounts of aluminum stabilized the delta (8) phase, which was, in fact, malleable. However, aluminum was one of those elements that emitted neutrons upon absorbing alpha particles and so would exacerbate the pre-initiation problem. Beneath aluminum in the periodic table was gallium, which did not



The first production reactor at Hanford with workmen loading uranium into its honeycomb face. (Photo courtesy of the Department of Energy.)

D Building, the air-conditioned wartime plutonium laboratory, bristling with separate external vents.



The hydrofluorination of plutonium. The upper photograph shows the chemical hoods in D Building used for this process, which converted the oxide to the tetrafluoride. Four furnace controllers are at the top of the panel with one controller open showing the temperature program cut into its rotating disc. Note the bucket of calcium oxide to be used for treatment of hydrogen fluoride burns. The lower photograph shows one of the hydrofluorination furnaces inside the hood.

undergo this type of nuclear reaction. Plutonium-gallium alloys were found to be stable in the δ phase and could be hot-pressed into the required hemispheres. Thus the problem of fabrication was solved. To avoid oxidation of the metal and to contain the radioactivity, the pieces were ultimately coated with nickel.

In July 1944 it was discovered that the plutonium-239 generated in the high-neutron-flux production reactors at Hanford contained too much plutonium-240. Plutonium-240 was undesirable because it had a much higher spontaneous fission rate than plutonium-239 and emitted far too many unwanted neutrons. As a result, pre-initiation in the gun weapon could not be avoided without the difficult task of separating these isotopes. Instead an intense effort was mounted to develop an implosion weapon in which pre-initiation could be avoided because of its higher assembly velocities. This turn of events allowed the purity requirements for the metal to be somewhat relaxed, simplifying many of the process operations. The necessary pieces of plutonium were then fabricated in time to construct the Trinity and Nagasaki devices.

The extreme press of time during the war allowed for the immediate problems of fabrication, stability, and oxidation protection to be solved only empirically. A comprehensive program of basic research on this most fascinating element had to wait until after the war. In Table II we summarize the properties of plutonium metal known in 1945.

Postwar Research and Development

As the war ended, construction began at DP West site on a new, more permanent facility for the plutonium effort. This activity reflected the government's decision to increase production of nuclear warheads and, thus. to scale up all processes associated with the fabrication of plutonium metal parts.

Because the plywood dry boxes of old D Building posed a tire hazard, they were

TABLE II

PROPERTIES OF PLUTONIUM METAL KNOWN IN 1945'

Phase	Temperature Range of Stability ('W)	Crystal Structure	Density (g/cm')	Average Linear Expansion Coefficient (per 'C)	Electrical Resistivity (@cm)	Temperature Coefficient of Resistivity (@cm per "C)
Alpha	Below 117	Orthorhombic (?)	19.8	55 x 10-^{6}	150 at 25°C	-29.7 x 10^{-4}
Beta	117 to 200	Unknown (complex)	17.8	35 x 10-^{6}	110 at 200°C	- o
Gamma	200 to 300	Unknown (complex)	Unknown	36 x 10^{-6}	110 at 300°C	˜ 0
Delta	300 to 475	Face-centered cubic	16.0	-21 x 10^{-6}	102 at 400°C	+1.5 x 10-^{4}
Epsilon	415 to 637	Body-centered cubic	16.4	4 x 10-^{6}	120 at 500°C	- 0
Liquid	Above 637±5					

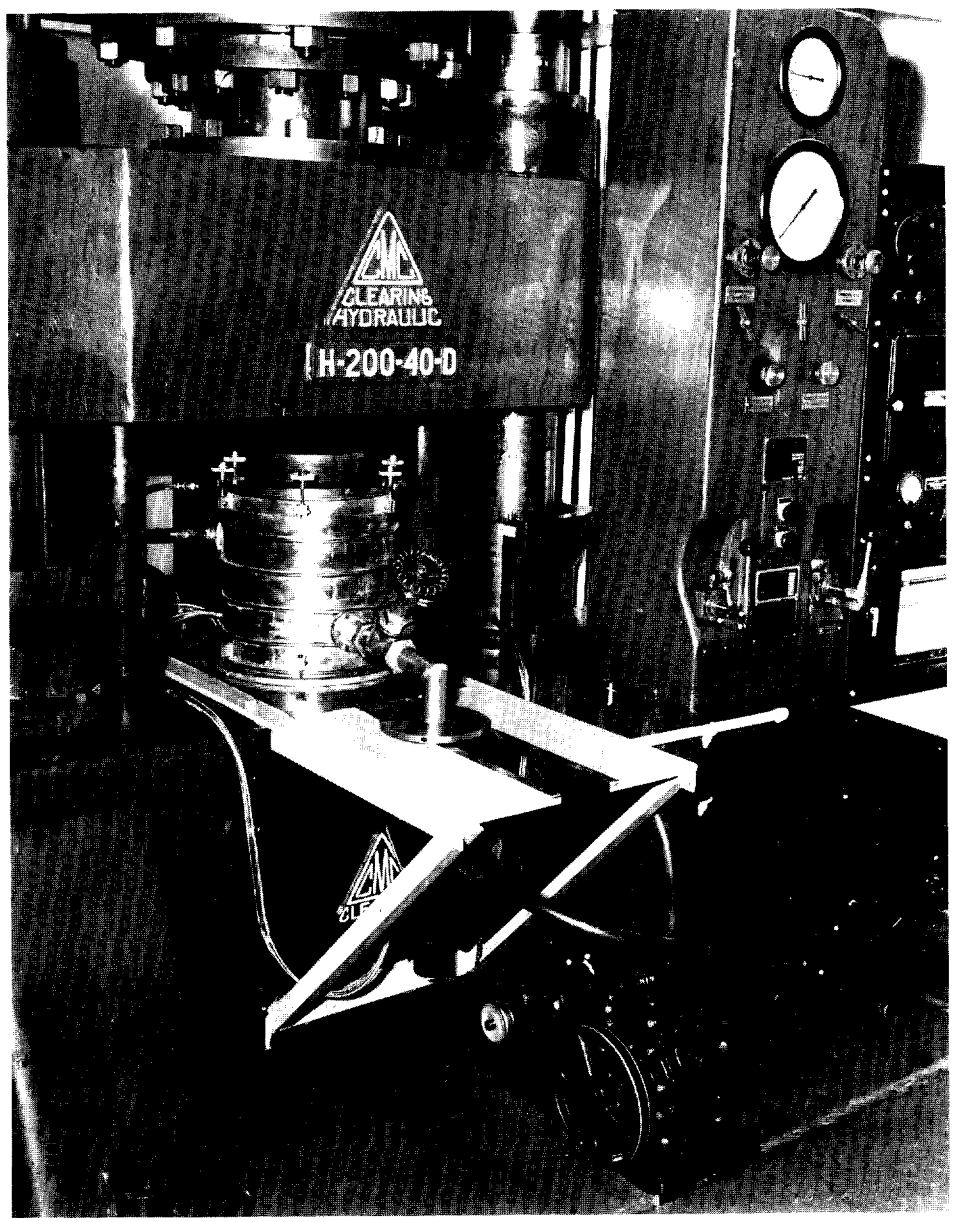
[a]*From Cyril Stanley Smith, Journal of Nuclear Materials 100,3-10 (1981).*

replaced with stainless-steel glove boxes. To better contain the plutonium, the glove boxes were equipped with elaborate ventilation-filtration systems devised to keep the atmosphere within each glove box at a lower pressure than the surrounding air so that any leak in the system would not release plutonium to the room. In addition, the breathing air in the laboratories was filtered and changed several times each hour.

Since all of the processes for purification, preparation, and fabrication of the metal and for recycling of the residues of plutonium and enriched uranium were developed at Los Alamos during the war, there was no other place for the production of nuclear warheads. It was decided that Los Alamos should not continue in production but should concentrate on research and development. The transfer of all the special processes to be used in the new production plants was a major postwar undertaking. Plutonium processes were transferred to Hanford, Savannah River, and Rocky Flats. The enriched uranium processes were transferred to Oak Ridge.

The work at DP West thus settled into a program of basic research and development, and major advances were made in the fledgling plutonium technologies of vacuum casting, metal working, machining, electrorefining, and aqueous processing of scrap. Several plutonium reactor fuels, both metallic and ceramic, and the plutonium-238 heat sources for thermoelectric generators for space and other missions had their beginnings at DP West.

In 1978 the plutonium activities at DP West were moved to the newly completed Los Alamos Plutonium Facility, the most modern and complete plutonium research



Presses used during the war to hot press plutonium metal into the shapes required for the Trinity and Nagasaki devices.

The current Los Alamos plutonium research and development facility. Note, in contrast to the wartime laboratory, the absence of external ventilation on the large research building to the right (upper photograph). The facility uses the most modern equipment, including a computerized plutonium accountability system (lower photograph).

and development center in the country. It incorporates state-of-the-art designs and equipment for the safe containment of plutonium and the protection of workers during all credible accidents or natural disasters, including earthquakes and tornado-force winds.

After the war the continued improvement in process chemistry and applied metallurgy of plutonium came about through a better understanding of its basic properties. Aqueous processes were developed for separating plutonium from virtually every element in the periodic table. The wartime "bomb" reduction process was augmented with other pyrochemical processes such as direct reduction of the oxide and electrorefining. These processes not only yielded a purer product but also minimized the amount of plutonium-bearing residues and the associated radiation exposure of personnel.

Plutonium casting was first carried out with ceramic crucibles and molds because they were known to be compatible with molten plutonium. The discovery that slightly oxidized tantalum was quite unreactive with molten plutonium led to the development of reusable foundry hardware. Also, the development of several ceramic coating processes, based on either calcium fluoride or the stable oxides of zirconium or yttrium, permitted the use of easily machined graphite molds. It was discovered that microcracks resulting from the multiple phase changes that occur as the metal cools and freezes could almost be eliminated by casting the pure metal into chilled aluminum molds, a process that virtually by-passes most of the intermediate phase transformations.

The development of new plutonium alloys for both reactor and weapons use proceeded hand in hand with the determination of the equilibrium phase diagrams of plutonium with most other elements, and the associated complex crystal structures of phases and compounds. Early on we realized that we were dealing with alloys that were metastable in many environments. Thus, it became

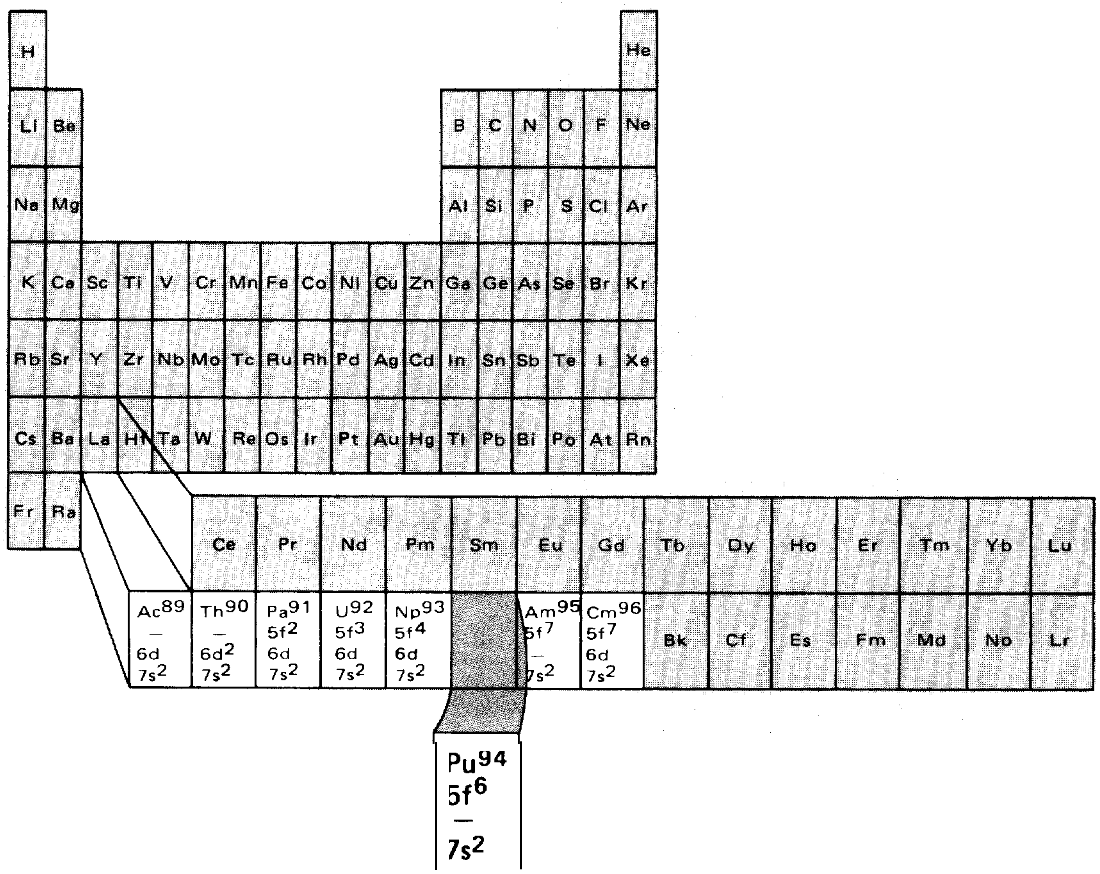


Fig. 1. The actinides and the configuration of their outermost electrons. This series of elements is characterized by the filling of 5* f *electron orbitals. (The 5* f *orbitals of thorium lie above the Fermi level but are sufficiently close to induce some* f *character in the electrons near the Fermi level. Thorium is therefore regarded as the first actinide.) The properties of the 5* f *electrons, particularly their participation in atomic bonding, are the key to the unusual properties of these elements.

imperative to understand microsegregation of alloying elements and phase stability during all processing steps. Complex heat treatments were developed to homogenize the alloys or to further stabilize the proper phases.

In 1954 the purity of plutonium was increased sufficiently that a new phase, delta prime (δ′), was discovered between the already known δ and epsilon (ϵ) phases. While this new phase proved to be inconsequential to applied plutonium technology, its discovery certainly showed the necessity of using high-purity, well-characterized plutonium in basic research. A seventh allotrope of plutonium, the zeta (ζ) phase, was discovered many years later in 1970 during careful studies of the equilibrium pressure-temperature phase diagram of plutonium. This phase exists only at high temperature over a limited pressure range and has such a complex crystal structure that it still today has not been positively identified.

Current Understanding of Plutonium

During the postwar period major activities in plutonium metallurgy in the United States were centered at Argonne National Laboratory, Hanford, Lawrence Livermore Laboratory, Rocky Flats, and of course, Los Alamos. Important contributions were also made at the Atomic Weapons Research Establishment's facility at Aldermaston in the United Kingdom and at Centre d'Etudes Nucleaires de Fontenay-aux-Roses in France. Except at Los Alamos much of the research activity was terminated or severely curtailed in the 1970s. However, we are currently seeing a revival of plutonium research at several locations.

In spite of many years of concentrated research and great strides in the practical aspects of plutonium metallurgy, this field is still in its infancy. A comparison with steel supports this perspective. The metallurgy of steel has been studied intensely in many countries for more than 100 years, yet important discoveries are still commonplace. Metallurgists have learned to manipulate the three allotropic phases of iron to tailor the properties of steel to specific applications. The six allotropic phases of plutonium and its much wider range of crystal structures and atomic volumes provide many more possibilities—and pitfalls.

The focus of the postwar research was to study all aspects of the behavior of this new element and, thus, be prepared for all of its peculiarities. In contrast, the focus of the past decade has been to exploit the complexities of plutonium. Much of the effort has been devoted to alloy development and the determination of structural properties and has resulted in several new alloys with interesting properties. Many of these results re-

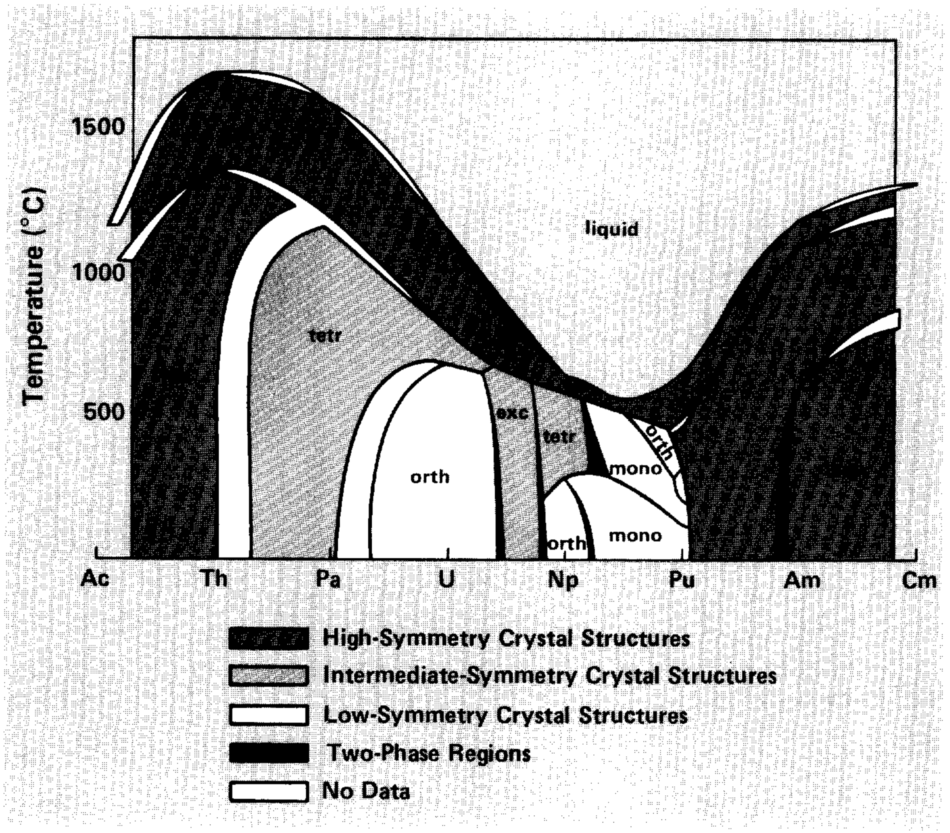


Fig. 2. A connected phase diagram of binary alloys of the actinides (prepared by E. A. Kmetko and J.L. Smith) shows the preponderance of low-symmetry crystal structures, the large number of phase changes, and the depression @melting points in the vicinity of plutonium. The crystal structures are body-centered cubic (bcc),face-centered cubic (fcc), tetragonal (tetr), orthorhombic (orth), exotic cubic (exe), monoclinic (mono), and double hexagonal close-packed (dhcp).

main classified.

In the past decade we have also turned our attention toward a more fundamental understanding of plutonium on the atomic level. This effort has opened a most fascinating chapter in solid-state physics—the electronic properties of the actinides, the seventh period in the periodic table. Interest in the actinides had stemmed primarily from their special nuclear properties. Yet, it is the properties of the electrons (not the nuclei) that govern all chemical and structural behavior. The actinides are characterized, as shown in Fig. 1, by the progressive filling of *5f* electron orbitals. It is the participation of these *5f* electrons in atomic bonding that leads to the peculiar and complex behavior of actinide metals and alloys.

Although details of the *5f* bonding in the actinides are still being contested, it is generally agreed that the *5f* electrons in the early actinides (through plutonium) are not fully localized and thus participate in bonding. The *5f* bonding increases to a maximum at plutonium and vanishes as the electrons become localized near americium. The effects of *5f* bonding on the behavior of the lighter actinides are dramatic, and most dramatic for plutonium. We will highlight here only three of the most important effects. These are demonstrated in Fig. 2, a connected binary alloy phase diagram of the elements in the actinide series. First, as one moves from actinium to plutonium, a change from highly symmetric cubic to low-symmetry crystal structures occurs. Second, the number of allotropic phases increases. Finally, the melting points decrease dramatically.

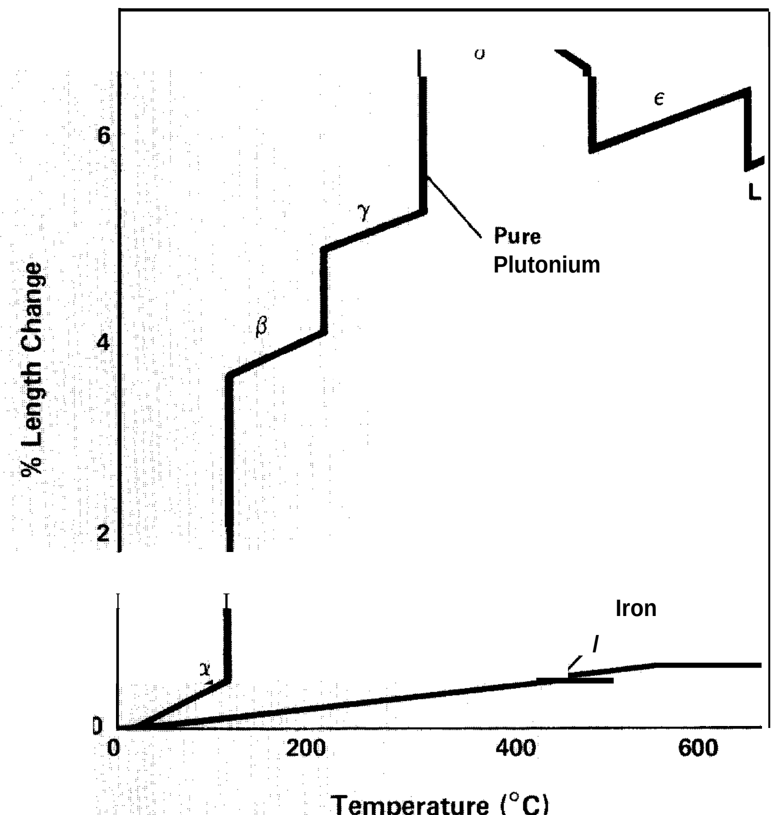
How can these effects be explained? To begin, the wave functions off electrons (like those of *p* electrons and unlike those ofs and *d* electrons) have odd symmetry. This property is not compatible with symmetric cubic crystal structures but rather favors the low-symmetry crystal structures and, thus, the stability of monoclinic and orthorhombic phases. Only with increased temperature and lattice vibrations is the *f* character sufficiently overcome in plutonium to permit cubic crystal structures. Beyond plutonium the localization (nonbonding) of the *f elec*trons leads to a return of more typical metallic behavior. Also, because the *f elec*trons are just on the verge of becoming localized and magnetic, small changes in temperature, pressure, or alloying have dramatic effects on phase stability and properties. Hence, allotropy is promoted. Finally, the *f* electrons bond quite easily in the liquid phase because its less rigid structure increases rotational freedom. This ease of bonding promotes the stability of the liquid (or, equivalently, limits the stability range of the solid) and lowers the melting points.

We see that the *f* electrons are the cause of many of plutonium's peculiarities and complexities, which have important practical consequences. Its low melting point and limited solid stability are particularly important because, as a liquid, plutonium is extremely reactive and corrosive and hence difficult to contain. Liquid plutonium also has the greatest known surface tension and viscosity among metals because off bonding. A less obvious consequence arises from the fact that most rate processes in solids depend upon homologous temperature, that is, temperature relative to the absolute melting point. Hence, diffusion and other thermally activated processes are quite rapid at room and slightly elevated temperatures.

The most significant consequence of plutonium's large number of phases is thermal instability of the solid. This property is best illustrated by a plot of length change during heating. Figure 3 compares the behavior of plutonium with that of iron. Most phase transitions in plutonium are accompanied by large length and thus volume changes. Such volume changes are difficult to accommodate in solids at relatively low temperatures without loss of physical integrity. In addition, plutonium's α, β, γ, and ϵ phases have very large thermal expansion coefficients. For example, the thermal expansion coefficient of the a phase is about five times greater than that of iron. Therefore special compatibility problems arise wherever plutonium is in contact with other metals. Figure 3 illustrates two exceptionally peculiar properties of plutonium: the negative thermal expansion coefficients of the δ and δ′ phases and the contraction upon melting, which results from increased *f*- electron bonding in the liquid phase.

The crystal structures and the corresponding densities are also listed in Fig. 3. Note that the three structures that are stable at temperatures closest to room temperature are of low symmetry. The cubic structures that are typical of most metals appear only at high temperatures where the 5 *f*-electron bonding is overwhelmed. The low-symmetry structures (especially the a phase) exhibit very directional bonding. The a-phase monoclinic structure is essentially covalently bonded. Its unit cell contains 16 atoms with 8 different bond lengths ranging from 2.57 to 3.71 angstroms. Consequently, most of its physical properties are also very directional. In addition, the *a* phase is a poor conductor and is highly compressible.

The low symmetry and nearly covalent nature of bonding in the a phase greatly affect its mechanical properties, which more nearly resemble those of covalently bonded minerals than those of metals. The a phase is strong and brittle because the low symmetry controls the nature and motion of defects. The face-centered cubic δ phase, on the other hand. behaves much like a normal metal. In fact, the δ phase possesses the strength and malleability of aluminum. One must remember, however, that at δ-phase temperatures



	Crystal Structure	Density (g/cm³)
α	Simple Monoclinic	19.86
β	Body-Centered Monoclinic	17.70
γ	Face-Centered Orthorhombic	17.14
δ	Face-Centered Cubic	15.92
δ′	Body-Centered Tetragonal	16.00
ϵ	Body-Centered Cubic	16.51
L	Liquid	16.65

Fig.* 3, *A plot of percentage length change as a function of temperature illustrates the dramatic changes that occur with each of plutonium's phase changes. The more sedate behavior of iron is shown for comparison.

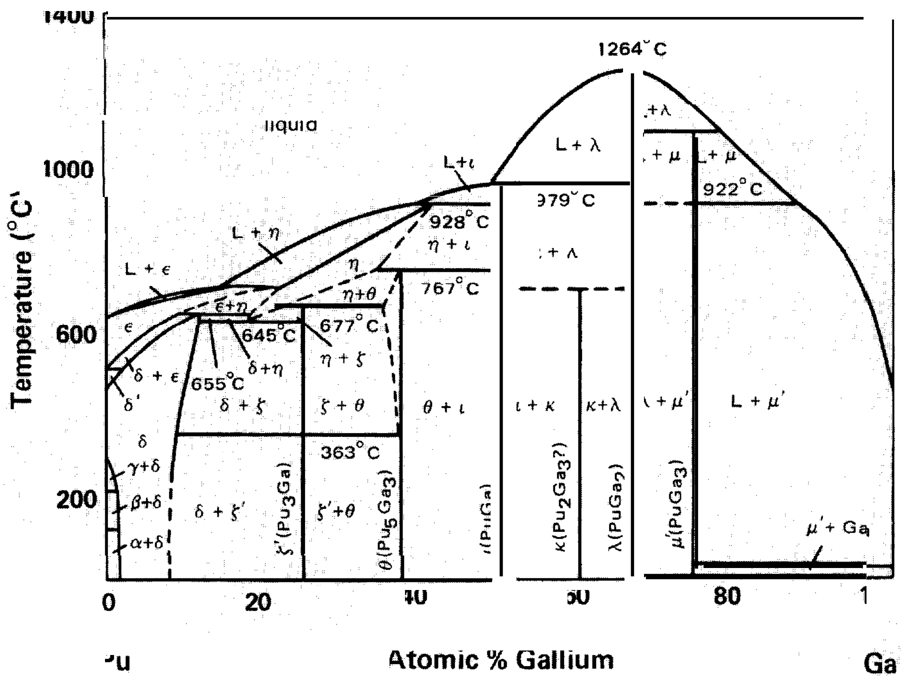


Fig. 4. The plutonium-gallium phase diagram serves as an example of the complexities that occur when plutonium is alloyed. The region of concern during the war was the lower left corner where the malleable δ phase extends down to room temperature at gallium concentrations below 10 per cent.

the *5f* bonding is essentially gone.

The δ phase can be stabilized to room temperature by alloying. As we pointed out earlier, this fact was already recognized during the war and led to the use of gallium to stabilize this phase. It is now recognized that most trivalent solutes, such as gallium, aluminum, cerium, americium, iridium, and scandium, stabilize the δ phase. Figure 4 shows the plutonium-gallium equilibrium phase diagram as determined at Los Alamos in the postwar era. Note the expanded field of the δ phase on the left and the enormous complexities that result from alloying plutonium. The δ phase in plutonium alloys behaves much like a normal metal and has several advantages over the a phase, including excellent ductility (fabricability), a much larger range of thermal stability, and a lower thermal expansion coefficient (nearly zero for most alloys).

So far we have not mentioned the effects of pressure. As one might expect, hydrostatic pressure tends to collapse the low-density crystal phases. Hence, in pure plutonium the δ phase disappears at pressures of less than 1 kilobar. Here is where the seventh allotrope of plutonium, the ζ phase, appears before giving way to α or β phases at high pressures. Only moderate pressures are required to collapse the alloyed δ phase to higher density phases. When dealing with alloys at high pressures, we are faced with the problem of what happens to the solute atoms, since they are generally insoluble in the α and β phases. This topic and the question of the response of alloys under nonequilibrium cooling conditions typify the fascinating world of nonequilibrium phase transformations in plutonium, which is beyond the scope of this article.

Plutonium is without question the most complex and interesting of all metals. More so than in any other metal, a fundamental understanding of its metallurgical behavior must be rooted in an understanding of electronic structure. We have highlighted the peculiarity and complexity of plutonium resulting from the *5f* electrons. The complexity, hidden until after the war, makes the accomplishments of the metallurgists and chemists during the Manhattan Project even more remarkable. ■

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Plutonium

A Wartime Nightmare but a Metallurgist's Dream

by Richard D. Baker, Siegfried S. Hecker, and Delbert R. Harbur

In 1942 the theoretical outline for an atomic bomb was clear: compress enough fissionable material long enough to properly ignite a chain reaction. Construction of an actual weapon, however, required translation of "fissionable material" into real pieces of plutonium or uranium metal. These metals had to be free of impurities that would adversely affect the neutron flux during the chain reaction and yet be fabricable enough that precise shapes could be formed. Whether this would even be possible with plutonium was not then known, however, because plutonium was a new, manmade element and the metal had not been produced.

Accounts of the Manhattan Project have neglected (for security reasons, initially) the important metallurgical work that preceded fabrication of these materials into integral parts of real weapons. For example, the Smyth Report* devotes one short paragraph to the wartime work of the entire Chemistry and Metallurgy Division at Los Alamos—a division that in 1945 numbered 400 scientists and technicians. Our article will attempt to fill part of this gap for plutonium by highlighting key developments of the wartime research and will continue with some of the exciting research that has occurred since the war.

Research from 1943 to 1946

The Los Alamos work on plutonium and enriched uranium, the so-called special nuclear materials, was extensive, covering a variety of research problems ranging from purification of material received from reactors to the prevention of oxidation of the final product. Further, because many chemical processes and physics experiments required very pure materials, such as gold, beryllium oxide, graphite, and many plastics, considerable general materials research was also carried out.

Much of the chemistry and metallurgy of uranium was already known from the production of uranium metal for the uranium-graphite reactor pile at Chicago in 1942. The work remaining on enriched uranium included preparation of high-purity metal, fabrication of components, and recycling of residues. However, the most challenging research and development was carried out on the new element plutonium.

Table I gives the important dates in the early history of plutonium and shows the short time—four years—that elapsed between its discovery and its use in the first atomic device at Trinity. The discovery occurred, as predicted by nuclear theory, when uranium was bombarded with 16-million-electron-volt deuterons in the cyclotron at Berkeley. Within about a month it was shown that plutonium-239 fissioned when bombarded with slow neutrons, and a decision was made to build large reactors at Hanford for the production of plutonium—this before the uranium-graphite pile at Chicago had demonstrated that a sustained and controlled chain reaction was even possible! That demonstration soon followed, proving that large quantities of plutonium could be produced, although no plutonium was extracted from the Chicago reactor.

At this point only microgram amounts of plutonium had been separated from the targets used in the cyclotrons. Remarkably, the basic chemistry of plutonium was worked out at Berkeley and Chicago on this microgram scale, and it formed the basis for the scale-up—by a factor of a billion—needed for plants that would eventually separate plutonium from spent reactor fuel. At the same time the first micrograms of the metal were produced at Chicago by the

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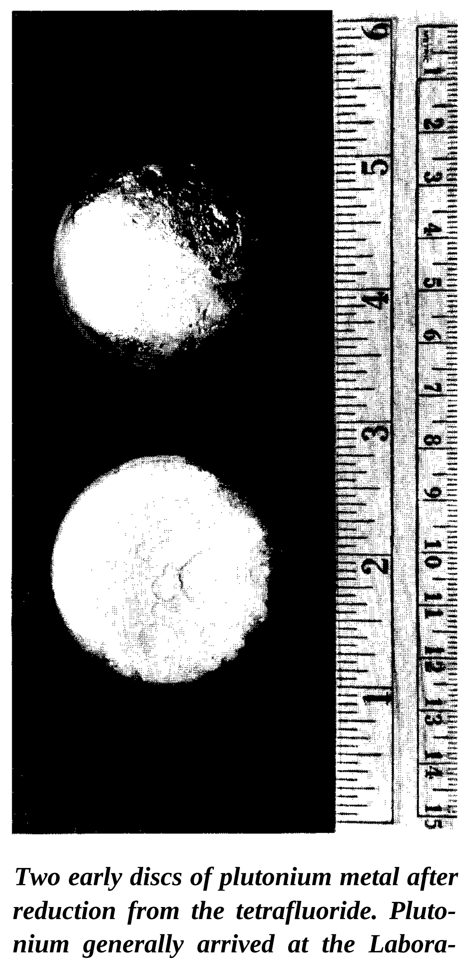
The plutonium nitrate arrived in relatively impure form, and techniques and equipment had to be developed for a number of processes, including purification, preparation of plutonium tetrafluoride and other compounds, reduction to metal, and metal fabrication. Also, because plutonium was in very short supply, it was imperative to develop processes to recycle all residues.

Initially, the purity requirements for the metal were very stringent because some elements, if present, would emit neutrons upon absorbing alpha particles from the radioactive plutonium, These extra neutrons were undesirable in the gun-type plutonium weapon then envisioned: they would initiate a chain reaction before the material had properly assembled into its supercritical configuration, and this "pre-initiation" would decrease the explosive force of the weapon. The purity requirement for certain elements was a few parts per million and for some, less than one part per million. As a result, all the materials used in the preparation of the plutonium metal, everything from the process chemicals to the containers, had to be of very high purity. This necessitated development work on many materials, including an extensive effort to obtain pure and nonreactive refractories to contain molten phutonium. The high purity requirements also necessitated the development of new methods for analysis of all materials, including plutonium.

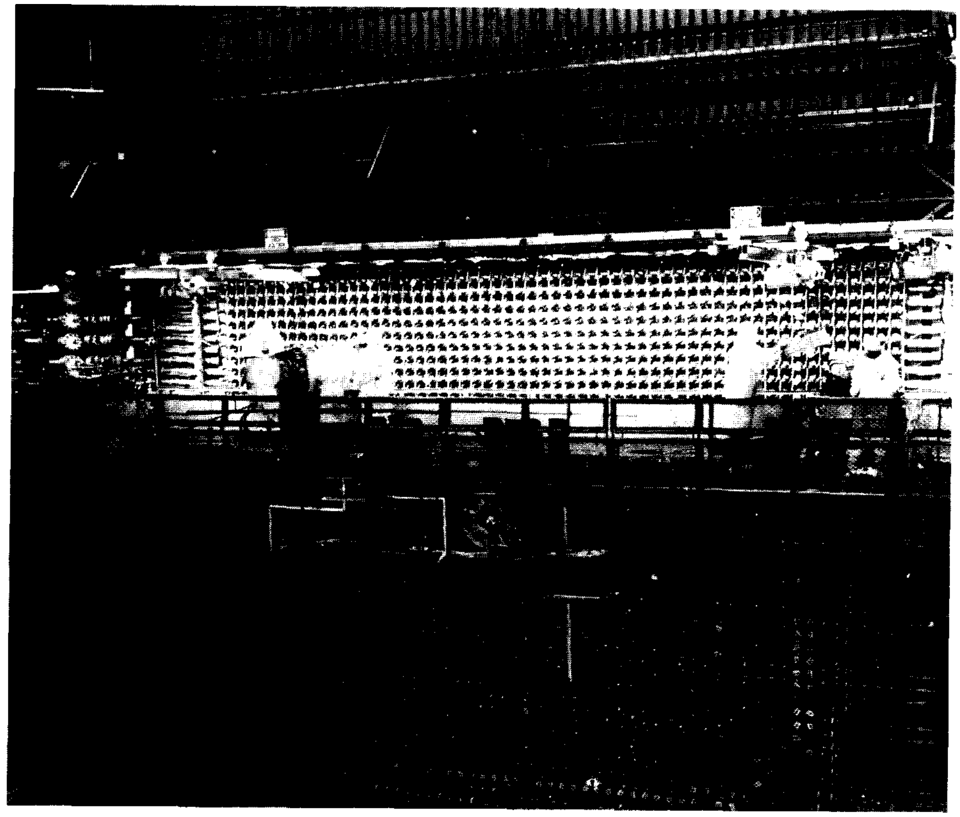
The potential health problem associated with the handling of plutonium had been recognized at Chicago, and work on the subject began with receipt of the first small amounts of plutonium. A Health Group was formed to monitor plutonium work areas, and, within the Chemistry and Metallurgy Division itself, committees were established to design suitable radiation detectors and apparatus for handling plutonium and to formulate safe handling procedures. Because alpha counters then lacked either sensitivity or portability and were in short supply, oiled filter paper was swiped over surfaces to pick up possible stray bits of plutonium and then measured at stationary counters. Similar procedures were used to detect suspected contamination of hands and nostrils. The air-conditioning system in the plutonium laboratory (D Building), which was installed initially to help maintain high purity by filtering out dust, ultimately served the more important function of confining the plutonium. The building was equipped with hoods with minimal ventilation and with the forerunner of the modern glove box—plywood "dry boxes." The successful handling of large quantities of plutonium without serious problems was at that time an outstanding achievement.



Two early discs of plutonium metal after reduction from the tetrafluoride. Plutonium generally arrived at the Laboratory from the Hanford reactors in the form of a relatively impure nitrate solution. Techniques were developed at Los Alamos for purification, preparation of various compounds, reduction to the metal, and metal fabrication.

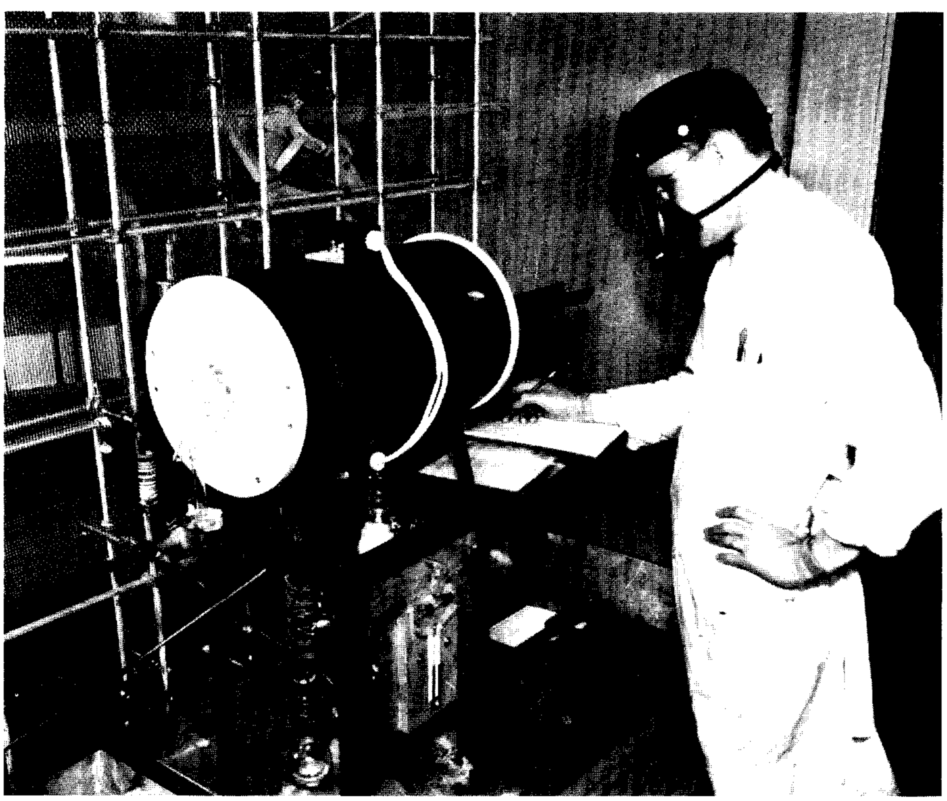
At first plutonium metal was prepared at Los Alamos either by lithium reduction of plutonium tetrafluoride in a centrifuge, the metal settling out as the closed reaction vessel rotated, or by the electrolysis of fused salts containing plutonium. Soon, however, calcium reduction of the tetrafluoride was perfected. The vessel used to contain this reaction was called a stationary "bomb" because the reaction was highly exothermic and the metal product settled out in the closed, nonrotating vessel simply by gravity. This technique became the preferred method and was used to prepare plutonium for almost all metallurgical studies and for the nuclear devices.

The microgram metallurgy at Chicago had provided values for the density of the metal that clustered about either 16 or 20 grams per cubic centimeter. This bimodal spread, due surely in part to impurities, nevertheless pointed toward interesting metallurgy by hinting that the element had more than one phase. Working with larger amounts, Los Alamos refined these measurements and by the middle of 1944 had discovered that plutonium was a nightmare: no less than five allotropic phases existed between room temperature and the melting point. Unfortunately, the room-temperature alpha (a) phase was brittle, and the metal experienced a large volume change when heated and then cracked upon cooling. These properties made fabrication very difficult, and there was not enough time for detailed fabrication development on the a-phase material. It was thought likely that another phase would be malleable and easily shaped; the problem was how to stabilize such a phase at room temperature. It was then discovered that alloying plutonium with small amounts of aluminum stabilized the delta (8) phase, which was, in fact, malleable. However, aluminum was one of those elements that emitted neutrons upon absorbing alpha particles and so would exacerbate the pre-initiation problem. Beneath aluminum in the periodic table was gallium, which did not



The first production reactor at Hanford with workmen loading uranium into its honeycomb face. (Photo courtesy of the Department of Energy.)

D Building, the air-conditioned wartime plutonium laboratory, bristling with separate external vents.



The hydrofluorination of plutonium. The upper photograph shows the chemical hoods in D Building used for this process, which converted the oxide to the tetrafluoride. Four furnace controllers are at the top of the panel with one controller open showing the temperature program cut into its rotating disc. Note the bucket of calcium oxide to be used for treatment of hydrogen fluoride burns. The lower photograph shows one of the hydrofluorination furnaces inside the hood.

undergo this type of nuclear reaction. Plutonium-gallium alloys were found to be stable in the δ phase and could be hot-pressed into the required hemispheres. Thus the problem of fabrication was solved. To avoid oxidation of the metal and to contain the radioactivity, the pieces were ultimately coated with nickel.

In July 1944 it was discovered that the plutonium-239 generated in the high-neutron-flux production reactors at Hanford contained too much plutonium-240. Plutonium-240 was undesirable because it had a much higher spontaneous fission rate than plutonium-239 and emitted far too many unwanted neutrons. As a result, pre-initiation in the gun weapon could not be avoided without the difficult task of separating these isotopes. Instead an intense effort was mounted to develop an implosion weapon in which pre-initiation could be avoided because of its higher assembly velocities. This turn of events allowed the purity requirements for the metal to be somewhat relaxed, simplifying many of the process operations. The necessary pieces of plutonium were then fabricated in time to construct the Trinity and Nagasaki devices.

The extreme press of time during the war allowed for the immediate problems of fabrication, stability, and oxidation protection to be solved only empirically. A comprehensive program of basic research on this most fascinating element had to wait until after the war. In Table II we summarize the properties of plutonium metal known in 1945.

Postwar Research and Development

As the war ended, construction began at DP West site on a new, more permanent facility for the plutonium effort. This activity reflected the government's decision to increase production of nuclear warheads and, thus. to scale up all processes associated with the fabrication of plutonium metal parts.

Because the plywood dry boxes of old D Building posed a tire hazard, they were

TABLE II

PROPERTIES OF PLUTONIUM METAL KNOWN IN 1945’

Phase	Temperature Range of Stability (’W)	Crystal Structure	Density (g/cm’)	Average Linear Expansion Coefficient (per ‘C)	Electrical Resistivity (@cm)	Temperature Coefficient of Resistivity (@cm per “C)
Alpha	Below 117	Orthorhombic (?)	19.8	55 x 10^{-6}	150 at 25°C	-29.7 x 10^{-4}
Beta	117 to 200	Unknown (complex)	17.8	35 x 10^{-6}	110 at 200°C	- o
Gamma	200 to 300	Unknown (complex)	Unknown	36 x 10^{-6}	110 at 300°C	˜ 0
Delta	300 to 475	Face-centered cubic	16.0	-21 x 10^{-6}	102 at 400°C	+1.5 x 10^{-4}
Epsilon	415 to 637	Body-centered cubic	16.4	4 x 10^{-6}	120 at 500°C	- 0
Liquid	Above 637±5					

[a]*From Cyril Stanley Smith, Journal of Nuclear Materials 100,3-10 (1981).*

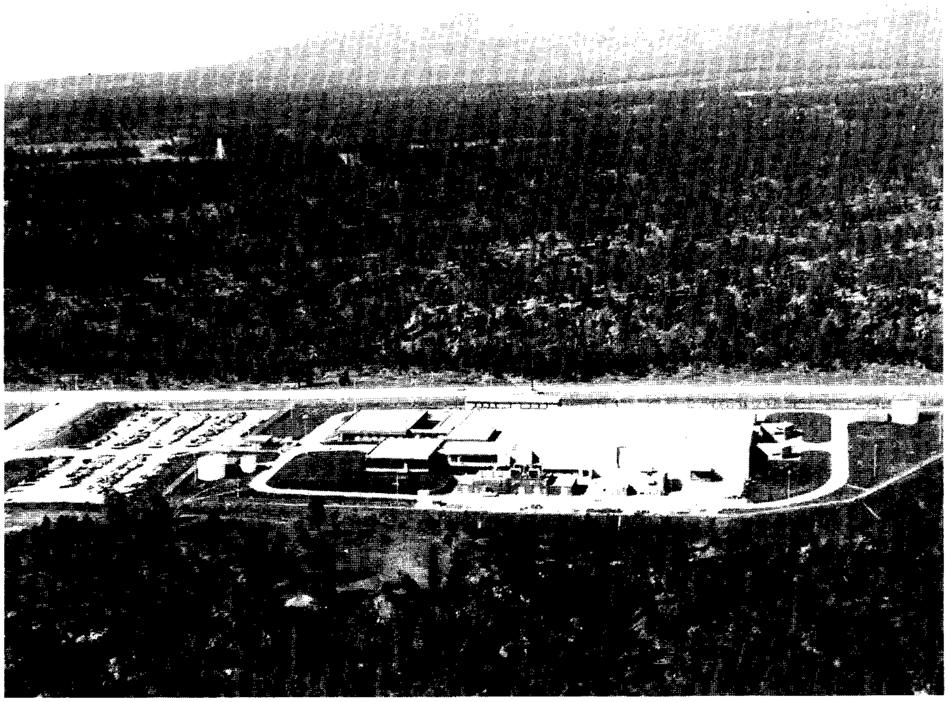
replaced with stainless-steel glove boxes. To better contain the plutonium, the glove boxes were equipped with elaborate ventilation-filtration systems devised to keep the atmosphere within each glove box at a lower pressure than the surrounding air so that any leak in the system would not release plutonium to the room. In addition, the breathing air in the laboratories was filtered and changed several times each hour.

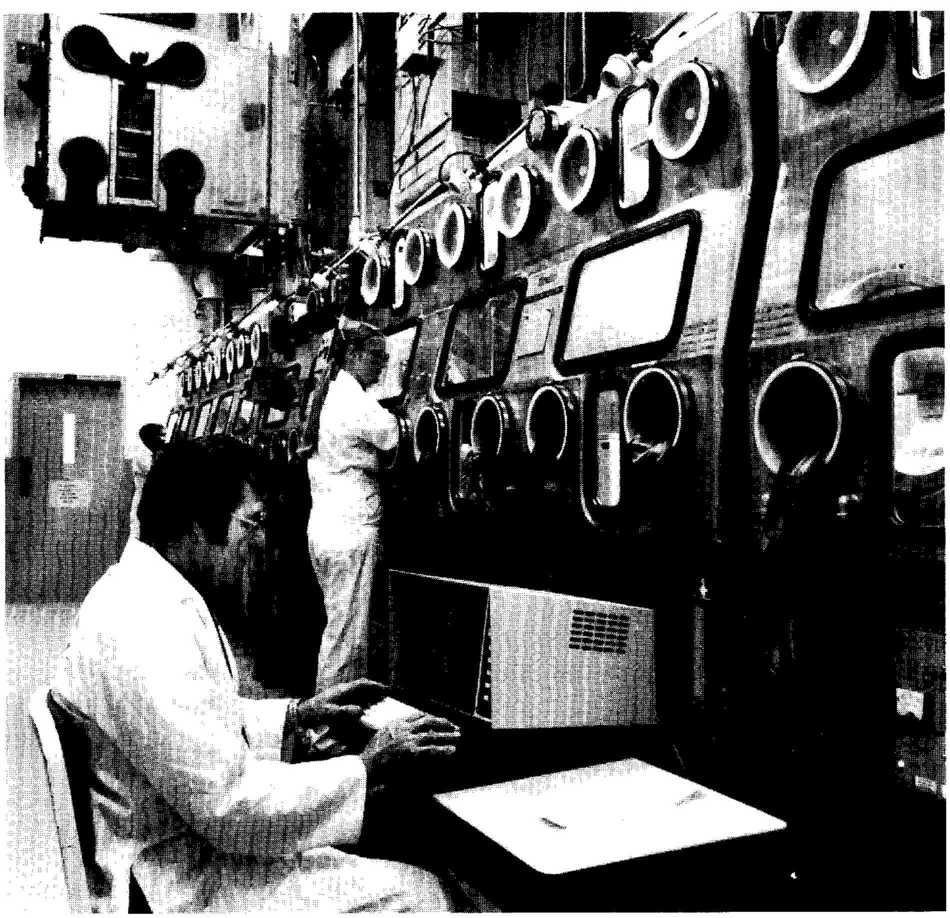
Since all of the processes for purification, preparation, and fabrication of the metal and for recycling of the residues of plutonium and enriched uranium were developed at Los Alamos during the war, there was no other place for the production of nuclear warheads. It was decided that Los Alamos should not continue in production but should concentrate on research and development. The transfer of all the special processes to be used in the new production plants was a major postwar undertaking. Plutonium processes were transferred to Hanford, Savannah River, and Rocky Flats. The enriched uranium processes were transferred to Oak Ridge.

The work at DP West thus settled into a program of basic research and development, and major advances were made in the fledgling plutonium technologies of vacuum casting, metal working, machining, electrorefining, and aqueous processing of scrap. Several plutonium reactor fuels, both metallic and ceramic, and the plutonium-238 heat sources for thermoelectric generators for space and other missions had their beginnings at DP West.

In 1978 the plutonium activities at DP West were moved to the newly completed Los Alamos Plutonium Facility, the most modern and complete plutonium research

Presses used during the war to hot press plutonium metal into the shapes required for the Trinity and Nagasaki devices.





The current Los Alamos plutonium research and development facility. Note, in contrast to the wartime laboratory, the absence of external ventilation on the large research building to the right (upper photograph). The facility uses the most modern equipment, including a computerized plutonium accountability system (lower photograph).

and development center in the country. It incorporates state-of-the-art designs and equipment for the safe containment of plutonium and the protection of workers during all credible accidents or natural disasters, including earthquakes and tornado-force winds.

After the war the continued improvement in process chemistry and applied metallurgy of plutonium came about through a better understanding of its basic properties. Aqueous processes were developed for separating plutonium from virtually every element in the periodic table. The wartime "bomb" reduction process was augmented with other pyrochemical processes such as direct reduction of the oxide and electrorefining. These processes not only yielded a purer product but also minimized the amount of plutonium-bearing residues and the associated radiation exposure of personnel.

Plutonium casting was first carried out with ceramic crucibles and molds because they were known to be compatible with molten plutonium. The discovery that slightly oxidized tantalum was quite unreactive with molten plutonium led to the development of reusable foundry hardware. Also, the development of several ceramic coating processes, based on either calcium fluoride or the stable oxides of zirconium or yttrium, permitted the use of easily machined graphite molds. It was discovered that microcracks resulting from the multiple phase changes that occur as the metal cools and freezes could almost be eliminated by casting the pure metal into chilled aluminum molds, a process that virtually by-passes most of the intermediate phase transformations.

The development of new plutonium alloys for both reactor and weapons use proceeded hand in hand with the determination of the equilibrium phase diagrams of plutonium with most other elements, and the associated complex crystal structures of phases and compounds. Early on we realized that we were dealing with alloys that were metastable in many environments. Thus, it became

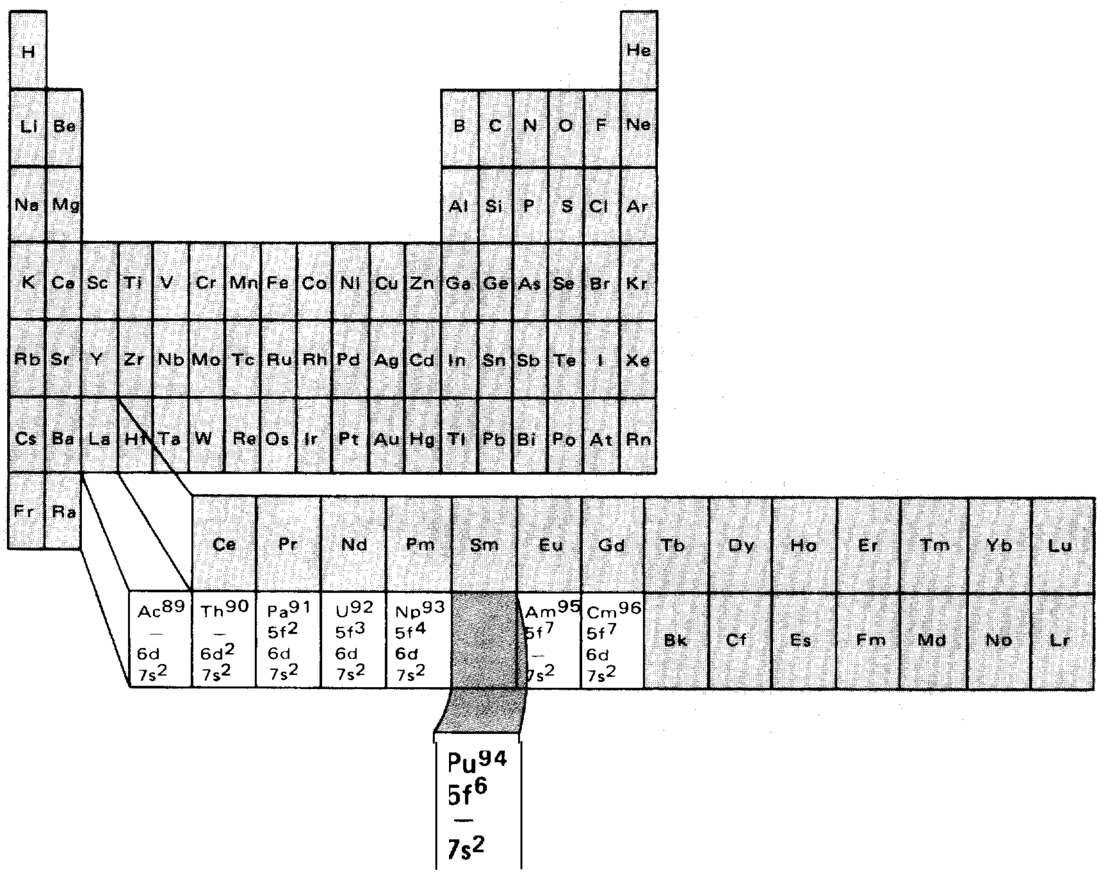


Fig. 1. The actinides and the configuration of their outermost electrons. This series of elements is characterized by the filling of 5 f *electron orbitals. (The 5* f *orbitals of thorium lie above the Fermi level but are sufficiently close to induce some* f *character in the electrons near the Fermi level. Thorium is therefore regarded as the first actinide.) The properties of the 5* f *electrons, particularly their participation in atomic bonding, are the key to the unusual properties of these elements.*

imperative to understand microsegregation of alloying elements and phase stability during all processing steps. Complex heat treatments were developed to homogenize the alloys or to further stabilize the proper phases.

In 1954 the purity of plutonium was increased sufficiently that a new phase, delta prime (δ'), was discovered between the already known δ and epsilon (ϵ) phases. While this new phase proved to be inconsequential to applied plutonium technology, its discovery certainly showed the necessity of using high-purity, well-characterized plutonium in basic research. A seventh allotrope of plutonium, the zeta (ζ) phase, was discovered many years later in 1970 during careful studies of the equilibrium pressure-temperature phase diagram of plutonium. This phase exists only at high temperature over a limited pressure range and has such a complex crystal structure that it still today has not been positively identified.

Current Understanding of Plutonium

During the postwar period major activities in plutonium metallurgy in the United States were centered at Argonne National Laboratory, Hanford, Lawrence Livermore Laboratory, Rocky Flats, and of course, Los Alamos. Important contributions were also made at the Atomic Weapons Research Establishment's facility at Aldermaston in the United Kingdom and at Centre d'Etudes Nucleaires de Fontenay-aux-Roses in France. Except at Los Alamos much of the research activity was terminated or severely curtailed in the 1970s. However, we are currently seeing a revival of plutonium research at several locations.

In spite of many years of concentrated research and great strides in the practical aspects of plutonium metallurgy, this field is still in its infancy. A comparison with steel supports this perspective. The metallurgy of steel has been studied intensely in many countries for more than 100 years, yet important discoveries are still commonplace. Metallurgists have learned to manipulate the three allotropic phases of iron to tailor the properties of steel to specific applications. The six allotropic phases of plutonium and its much wider range of crystal structures and atomic volumes provide many more possibilities—and pitfalls.

The focus of the postwar research was to study all aspects of the behavior of this new element and, thus, be prepared for all of its peculiarities. In contrast, the focus of the past decade has been to exploit the complexities of plutonium. Much of the effort has been devoted to alloy development and the determination of structural properties and has resulted in several new alloys with interesting properties. Many of these results re-

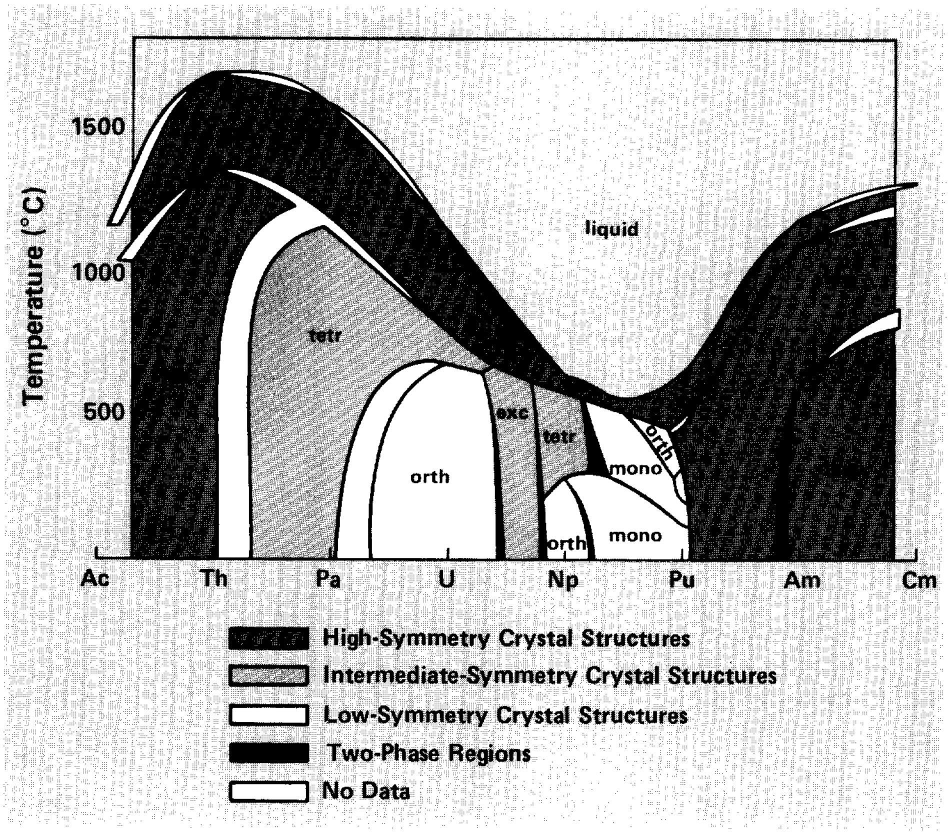


Fig. 2. A connected phase diagram of binary alloys of the actinides (prepared by E. A. Kmetko and J.L. Smith) shows the preponderance of low-symmetry crystal structures, the large number of phase changes, and the depression @melting points in the vicinity of plutonium. The crystal structures are body-centered cubic (bcc),face-centered cubic (fcc), tetragonal (tetr), orthorhombic (orth), exotic cubic (exe), monoclinic (mono), and double hexagonal close-packed (dhcp).

main classified.

In the past decade we have also turned our attention toward a more fundamental understanding of plutonium on the atomic level. This effort has opened a most fascinating chapter in solid-state physics—the electronic properties of the actinides, the seventh period in the periodic table. Interest in the actinides had stemmed primarily from their special nuclear properties. Yet, it is the properties of the electrons (not the nuclei) that govern all chemical and structural behavior. The actinides are characterized, as shown in Fig. 1, by the progressive filling of *5f* electron orbitals. It is the participation of these *5f* electrons in atomic bonding that leads to the peculiar and complex behavior of actinide metals and alloys.

Although details of the *5f* bonding in the actinides are still being contested, it is generally agreed that the *5f* electrons in the early actinides (through plutonium) are not fully localized and thus participate in bonding. The *5f* bonding increases to a maximum at plutonium and vanishes as the electrons become localized near americium. The effects of *5f* bonding on the behavior of the lighter actinides are dramatic, and most dramatic for plutonium. We will highlight here only three of the most important effects. These are demonstrated in Fig. 2, a connected binary alloy phase diagram of the elements in the actinide series. First, as one moves from actinium to plutonium, a change from highly symmetric cubic to low-symmetry crystal structures occurs. Second, the number of allotropic phases increases. Finally, the melting points decrease dramatically.

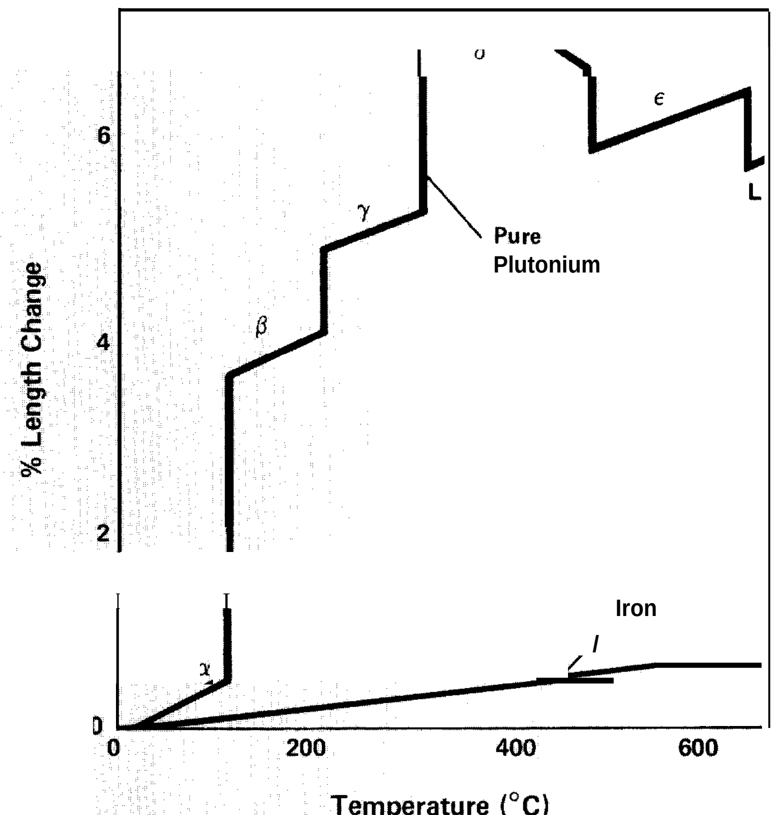
How can these effects be explained? To begin, the wave functions off electrons (like those of *p* electrons and unlike those ofs and *d* electrons) have odd symmetry. This property is not compatible with symmetric cubic crystal structures but rather favors the low-symmetry crystal structures and, thus, the stability of monoclinic and orthorhombic phases. Only with increased temperature and lattice vibrations is the *f* character sufficiently overcome in plutonium to permit cubic crystal structures. Beyond plutonium the localization (nonbonding) of the *f electrons* leads to a return of more typical metallic behavior. Also, because the *f electrons* are just on the verge of becoming localized and magnetic, small changes in temperature, pressure, or alloying have dramatic effects on phase stability and properties. Hence, allotropy is promoted. Finally, the *f* electrons bond quite easily in the liquid phase because its less rigid structure increases rotational freedom. This ease of bonding promotes the stability of the liquid (or, equivalently, limits the stability range of the solid) and lowers the melting points.

We see that the *f* electrons are the cause of many of plutonium's peculiarities and complexities, which have important practical consequences. Its low melting point and limited solid stability are particularly important because, as a liquid, plutonium is extremely reactive and corrosive and hence difficult to contain. Liquid plutonium also has the greatest known surface tension and viscosity among metals because off bonding. A less obvious consequence arises from the fact that most rate processes in solids depend upon homologous temperature, that is, temperature relative to the absolute melting point. Hence, diffusion and other thermally activated processes are quite rapid at room and slightly elevated temperatures.

The most significant consequence of plutonium's large number of phases is thermal instability of the solid. This property is best illustrated by a plot of length change during heating. Figure 3 compares the behavior of plutonium with that of iron. Most phase transitions in plutonium are accompanied by large length and thus volume changes. Such volume changes are difficult to accommodate in solids at relatively low temperatures without loss of physical integrity. In addition, plutonium's α, β, γ, and ε phases have very large thermal expansion coefficients. For example, the thermal expansion coefficient of the a phase is about five times greater than that of iron. Therefore special compatibility problems arise wherever plutonium is in contact with other metals. Figure 3 illustrates two exceptionally peculiar properties of plutonium: the negative thermal expansion coefficients of the δ and δ′ phases and the contraction upon melting, which results from increased *f*- electron bonding in the liquid phase.

The crystal structures and the corresponding densities are also listed in Fig. 3. Note that the three structures that are stable at temperatures closest to room temperature are of low symmetry. The cubic structures that are typical of most metals appear only at high temperatures where the 5 *f*-electron bonding is overwhelmed. The low-symmetry structures (especially the a phase) exhibit very directional bonding. The a-phase monoclinic structure is essentially covalently bonded. Its unit cell contains 16 atoms with 8 different bond lengths ranging from 2.57 to 3.71 angstroms. Consequently, most of its physical properties are also very directional. In addition, the *a* phase is a poor conductor and is highly compressible.

The low symmetry and nearly covalent nature of bonding in the a phase greatly affect its mechanical properties, which more nearly resemble those of covalently bonded minerals than those of metals. The a phase is strong and brittle because the low symmetry controls the nature and motion of defects. The face-centered cubic δ phase, on the other hand. behaves much like a normal metal. In fact, the δ phase possesses the strength and malleability of aluminum. One must remember, however, that at δ-phase temperatures



	Crystal Structure	Density (g/cm^3)
α	Simple Monoclinic	19.86
β	Body-Centered Monoclinic	17.70
γ	Face-Centered Orthorhombic	17.14
δ	Face-Centered Cubic	15.92
δ′	Body-Centered Tetragonal	16.00
ε	Body-Centered Cubic	16.51
L	Liquid	16.65

Fig.* 3, *A plot of percentage length change as a function of temperature illustrates the dramatic changes that occur with each of plutonium's phase changes. The more sedate behavior of iron is shown for comparison.

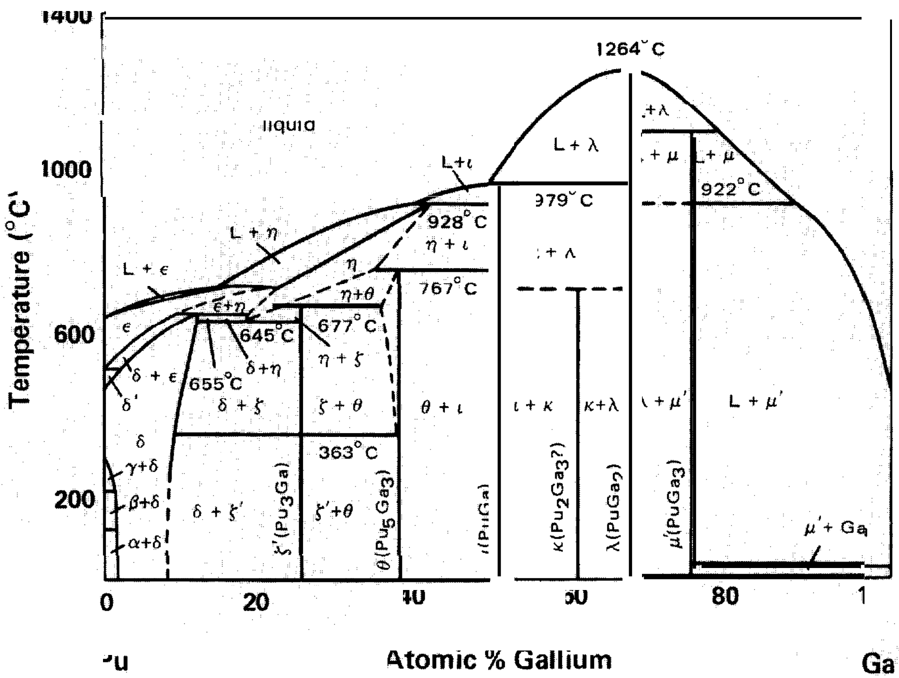


Fig. 4. The plutonium-gallium phase diagram serves as an example of the complexities that occur when plutonium is alloyed. The region of concern during the war was the lower left corner where the malleable δ phase extends down to room temperature at gallium concentrations below 10 per cent.

the *5f* bonding is essentially gone.

The δ phase can be stabilized to room temperature by alloying. As we pointed out earlier, this fact was already recognized during the war and led to the use of gallium to stabilize this phase. It is now recognized that most trivalent solutes, such as gallium, aluminum, cerium, americium, iridium, and scandium, stabilize the δ phase. Figure 4 shows the plutonium-gallium equilibrium phase diagram as determined at Los Alamos in the postwar era. Note the expanded field of the δ phase on the left and the enormous complexities that result from alloying plutonium. The δ phase in plutonium alloys behaves much like a normal metal and has several advantages over the a phase, including excellent ductility (fabricability), a much larger range of thermal stability, and a lower thermal expansion coefficient (nearly zero for most alloys).

So far we have not mentioned the effects of pressure. As one might expect, hydrostatic pressure tends to collapse the low-density crystal phases. Hence, in pure plutonium the δ phase disappears at pressures of less than 1 kilobar. Here is where the seventh allotrope of plutonium, the ζ phase, appears before giving way to α or β phases at high pressures. Only moderate pressures are required to collapse the alloyed δ phase to higher density phases. When dealing with alloys at high pressures, we are faced with the problem of what happens to the solute atoms, since they are generally insoluble in the α and β phases. This topic and the question of the response of alloys under nonequilibrium cooling conditions typify the fascinating world of nonequilibrium phase transformations in plutonium, which is beyond the scope of this article.

Plutonium is without question the most complex and interesting of all metals. More so than in any other metal, a fundamental understanding of its metallurgical behavior must be rooted in an understanding of electronic structure. We have highlighted the peculiarity and complexity of plutonium resulting from the *5f* electrons. The complexity, hidden until after the war, makes the accomplishments of the metallurgists and chemists during the Manhattan Project even more remarkable. ■

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Criticality
The Fine Line of Control

by Hugh C. Paxton

In the early days of the Manhattan Project, no one had experience in handling the large quantity of fissionable material needed to build a weapon because, quite simply, it hadn't been made yet. That was soon to change as Oak Ridge began to separate small amounts of uranium-235 and to prepare for processing kilogram amounts. This large a quantity posed the danger of accidental criticality—setting off a fission chain reaction—as scientists on Project Y well knew. But, as Feynman relates,* the demands for secrecy meant that this information was not widespread:

> . . . The higher people [at Oak Ridge] knew they were separating uranium, but they didn't know how powerful the bomb was, or exactly how it worked or anything. The people underneath didn't know at all what they were doing. . . . Segre insisted they'd never get the assays right, and the whole thing would go up in smoke. So he finally went down [from Los Alamos] to see what they were doing, and as he was walking through he saw them wheeling a tank carboy of water, green water—which is uranium nitrate solution.
>
> He says, "Uh, you're going to handle it like that when it's purified too? Is that what you're going to do?"
>
> They said, "Sure-why not?"
>
> "Won't it explode?" he says.
>
> . . . The Army had realized how much stuff we needed to make a bomb—20 kilograms or whatever it was—and they realized that this much material, purified, would never be in the plant, so there was no danger. But they did not know that the neutrons were enormously more effective when they are slowed down in water. And so in water it takes less than a tenth—no, a hundredth—as much material to make a reaction that makes radioactivity. It kills people around and so on. So, it was *very* dangerous, and they had not paid any attention to the safety at all.

Thereafter, criticality safety became an important focus at Oak Ridge and Los Alamos, but when I arrived in Los Alamos, late in 1948, the state of the art was still fairly primitive. I was asked to head the critical assemblies group in Pajarito Canyon. With this assignment I became the Laboratory's immediate expert on nuclear criticality safety, although I had no pertinent background. Now, from the vantage point of today's abundant criticality information, I realize I should have been dismayed. But then there existed only a few-page summary of experimental data from Los Alamos, a couple of reports giving Oak Ridge measurements, and no reliable calculations (excellent methods were being developed but remained unconfirmed). This amount of information was certainly not overwhelming.

I had to learn rapidly the techniques for avoiding accidental criticality in processing, fabricating, storing, and transporting fissile materials. (At that time we had plutonium and uranium enriched in uranium-235; uranium-233 was added later.) These techniques were meant to control any variable that affects criticality, such as mass, dimensions, density, and concentration in solution. Criticality also is influenced by nearby objects that act as neutron reflectors, returning neutrons that otherwise would be lost to the fissile material. As mentioned in Feynman's tale, neutron moderation, especially by intermixing the fissile material with hydrogenous material, such as water, is particularly important to criticality. Hydrogen is very effective at moderating (decreasing the energy of) fission neutrons by scattering, and these less energetic neutrons are much more effective at initiating further fissions.

In the late 1940s it usually was necessary to compensate for insufficient data by introducing large factors of safety. This situation was acceptable for operations in processing plants because production rates of fissile material were still low. Weapons, however, were another matter. Design subtlety had not yet reduced their content of fissile

From Richard P. Feynman,, "Los Alamos From Below," in* Reminiscences of Los Alamos [943-1945, *Lawrence Badash, Joseph O. Hirschfelder, and Herbert P. Broida, Eds. (D. Reidel Publishing Co., Dordrecht, Holland, 1980), pp. 120-132.

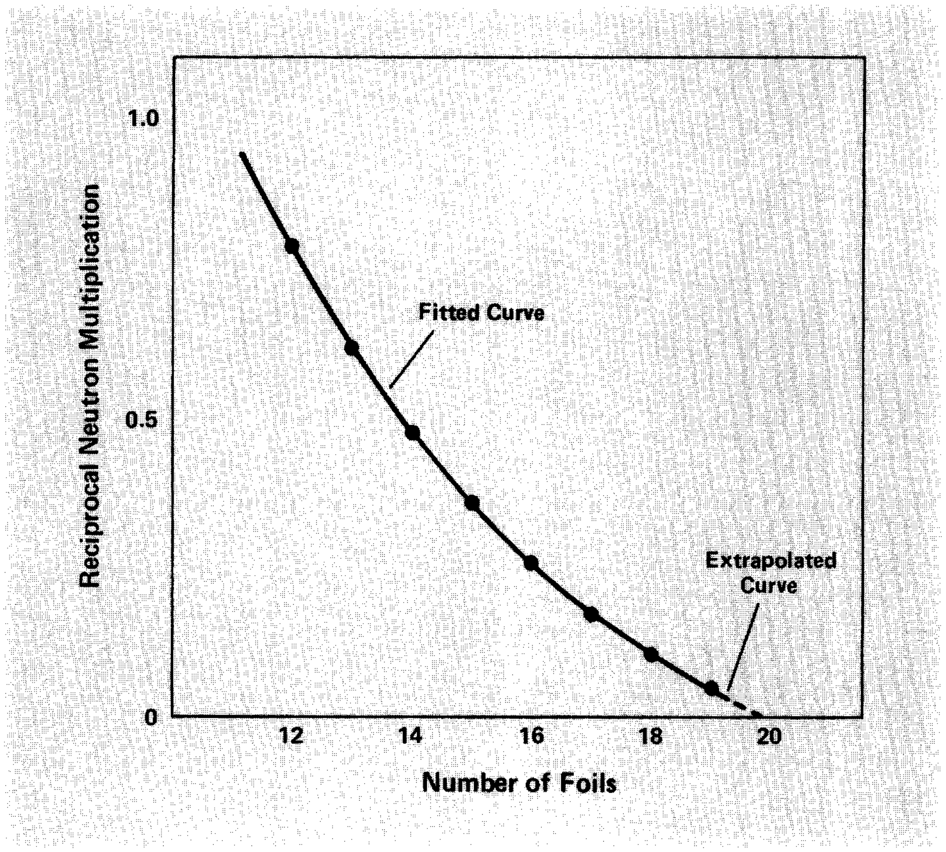


Fig. 1. The data points above were obtained from neutron count-rate measurements on a "sandwich" containing, alternately, slabs of Lucite (a neutron moderator) and foils of enriched uranium. As the sandwich is allowed to approach the critical state by adding uranium-Lucite layers one by one, the neutron count rate rises rapidly. Plotted above are reciprocal neutron multiplication values (ratios of count rate for the original sandwich to count rates as each layer is added) versus number of foils. Extrapolation of the fitted curve to zero establishes the critical number offoils.

material. and many weapons contained as much fissile material as could be introduced safely. Excessive safety factors could not be tolerated, and special measurements by the critical assemblies group were required for reasonably, but not excessively. safe designs.

Because the Pajarito group was capable and smoothly functioning when I arrived, it performed well while I learned from it about the conduct of critical experiments and their relation to weapon design. I learned about neutron-multiplication measurements with so-called long counters that responded uniformly to neutrons with a wide range of energy. I learned how multiplication, represented by neutron count rate, increases as the mass of plutonium or enriched uranium is increased and tends toward infinity as criticality is approached. The critical mass could be established. however, without actually reaching it. A plot of reciprocal neutron multiplication versus fissile mass (or other variable used to approach criticality) extrapolates to zero at criticality (Fig. 1) and thus establishes the critical mass by means of subcritical measurements.

To appreciate the significance of criticality, let us first note that a nuclear explosion is the result of a runaway fission chain reaction in which neutrons from fission produce an increased number of fissions, which in turn produce an increased number of neutrons, and so on. The term supercritical describes this state. In the critical state the fission rate and the number of neutrons remain steady. A sphere of the most dense phase of plutonium is just critical at a mass of 10.5 kilograms if bare, but the critical mass drops to about 6 kilograms if the plutonium is surrounded by a natural uranium reflector that returns neutrons to the plutonium. A more spectacular decrease, to a critical mass less than 0.6 kilogram, may occur in a uniform mixture of plutonium and water surrounded by a water reflector. This decrease is a result of neutron moderation by hydrogen.

Strictly, the steady-state fission chain reaction occurs at *delayed* criticality. That is, it depends upon the delayed neutrons emitted during decay of the fission products as well as the prompt neutrons emitted during fission. At steady state the delayed neutrons constitute less than 1 per cent of the total neutron population. The addition of a small amount of fissile material (1 per cent for plutonium and 2 per cent for uranium) to a critical mass produces *prompt* criticality. That is, delayed neutrons no longer influence the chain reaction, and fission power increases so rapidly that it is uncontrollable. If the increment between delayed and prompt criticality is termed 100 cents, prompt criticality may be exceeded a few cents without damaging a uranium metal system, but the intense radiation pulse would endanger a person nearby. At an excess of 10 cents, damage to the system would begin. The damage would become severe at a 15-cent excess, and the runaway chain reaction would lead to an explosion at an excess of 50 cents or less.

In weapon design it is important to know the delayed critical state because it must be exceeded during detonation but must not be attained during assembly, storage, and transportation, As plutonium and enriched uranium began to accumulate at Los Alamos. priority was attached to experiments that determined critical conditions by extrapolation from subcritical measurements. Before 1946 these urgent experiments had been conducted manually by persons who remained beside the experiment. Typically, the experiments involved the stepwise addition of reflector material to a fissile core with a multiplication measurement at each step.

Twice, criticality was attained accidentally during these experiments. The first incident, in 1945, resulted in fatal radiation injury to Harry Daghlian. It occurred when a heavy uranium block slipped from Daghlian's hand onto a near-critical assembly consisting of a plutonium ball and a natural uranium reflector. The damaging radiation consisted of neutrons and gamma rays from the intense fission chain reaction. Manipulation by hand continued until Louis Slotin suffered a similar fate about a year later. Again something slipped—in this case a screwdriver being used to lower a beryllium reflector shell toward the same plutonium ball involved in the earlier accident. The shell dropped instead of being held short of criticality. In neither accident was equipment damaged. Manual control was outlawed after the second accident, and the facility in Pajarito Canyon was rushed to completion.

At the Pajarito facility experiments are carried out by remote control from a control room one-quarter mile away. (Other critical assembly facilities of the time used massive shielding, rather than distance. for personnel protection.) The building in which the experiments were carried out (Fig. 2) was called the kiva, a term borrowed from the Pueblo Indians and referring to their ceremonial chambers. The facility became available for subcritical measurements in 1947 and for critical operation a year later. In subsequent years two other kivas were added. Separate control rooms for the three kivas are located in a central building.

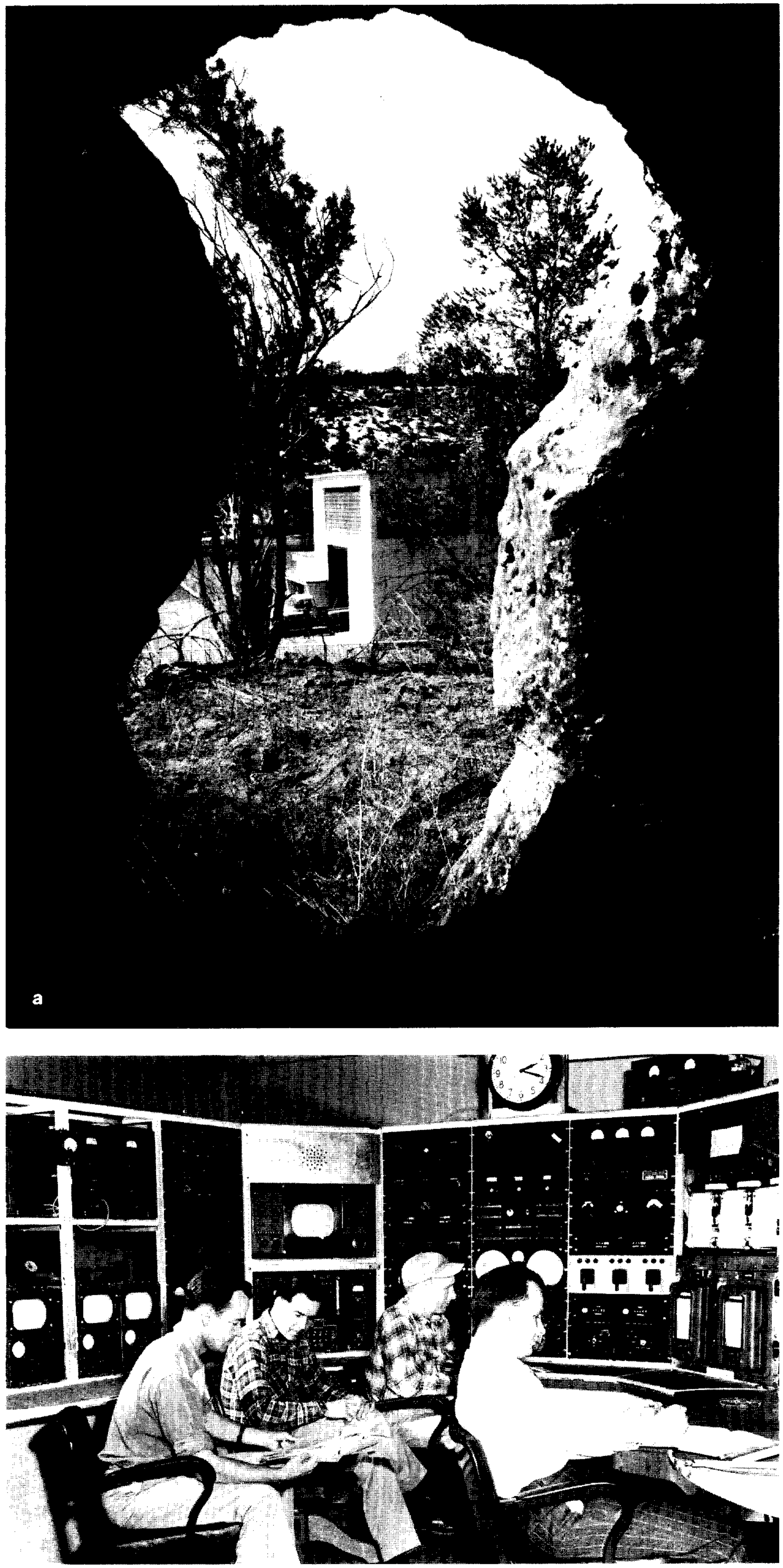
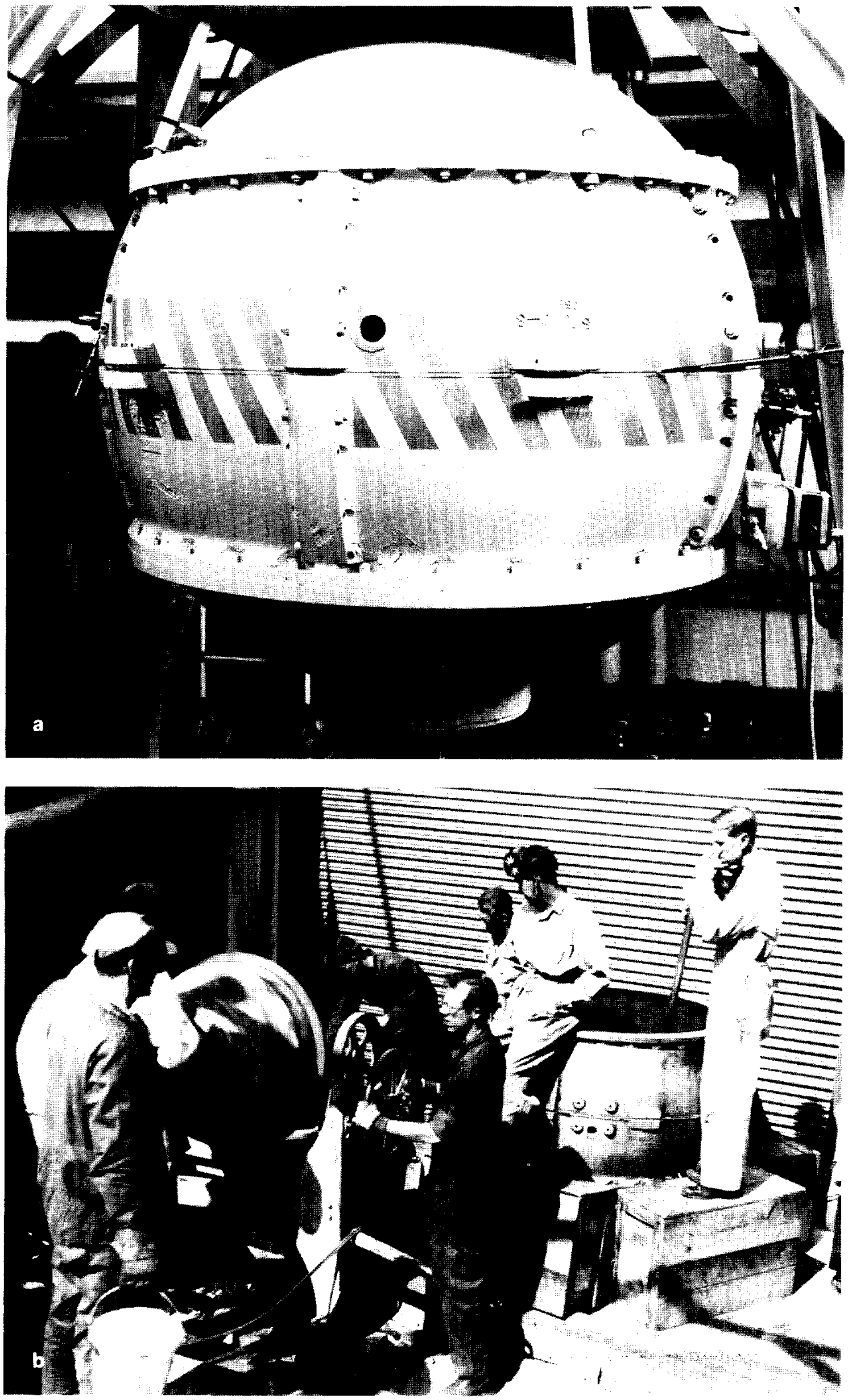


Fig. 2. (a) The original kiva, photographed from an Indian cave in the nearby wall of Pajarito Canyon, and (b) its control room, which was first housed in an existing shack. The racks contain controls for gradually separating and bringing together the parts of a critical assembly, displays of the long-counter responses that indicate neutron multiplication, radiation monitors that trigger a scram (automatic disassembly) if the level should become higher than intended, and a television screen for viewing the assembly. From left to right, Vernal Josephson, Roger Paine, Lester Woodward, and Hugh Karr. Paine and Woodward were military personnel who contributed invaluably to our critical experiments.



The Bomb Mockup (Fig. 3), the first remotely controlled machine for bringing together two parts of a near-critical assembly, was similar in size to Fat Man, the Nagasaki weapon. The two hemispheres of the Bomb Mockup were separated, and a core of fissile material was placed in a recess in the lower hemisphere. After personnel retreated to the control room, remotely actuated controls brought the two hemispheres together and instruments recorded the neutron count rate. The process was repeated with increasing masses of fissile material until extrapolation to criticality was acceptable.

These subcritical neutron-multiplication measurements with the Bomb Mockup demonstrated safe loading of implosion-weapon components, confirmed the intended reactivity (deviation from the critical state) of production cores, and provided safety guidance for new implosion-weapon designs. To

Fig. 3. (a) The Bomb Mockup, a simulation of an implosion weapon in Kiva I. After a fissile core was placed in a cavity in the lower hemisphere, neutron count rates were measured as the two hemispheres were gradually brought together by remote control. Before personnel could re-enter the kiva, the two halves of the mockup had to be separated. Neutron-multiplication measurements in this mockup established subcritical limits for weapons of more advanced design than the Nagasaki weapon. (b) An adult version of mud pies was an essential preliminary to experiments with the Bomb Mockup. Surrounding the fissile core in the mockup was a material that simulated the neutron reflection and moderation properties of high explosives. The photograph shows the material being mixed and tamped into parts of the mockup. Identifiable are William Wenner holding the bucket, Gustave Linenberger in the center foreground, and James Roberts standing above.

supplement experiments with the Bomb Mockup, flooding tests confirmed subcriticality should a core fall accidentally into a body of water. The flooding tests were carried out in a temporary setup consisting of a tank that was filled by remote control and had a large dump valve as a safety device. Other safety tests involved cores surrounded by paraffin, concrete, and natural uranium.

Information to guide the safe storage of weapon components was obtained in 1947 with another temporary setup (Fig. 4). It consisted of a concrete vault of adjustable size that was closed by remote control and opened automatically when the radiation near the vault exceeded a safe level. Multiplication measurements on arrays of implosion-weapon cores or capsules as they were built up stepwise within the vault (Fig. 5) provided the required guidance. Some years later these measurements were supplemented by neutron-multiplication tests on arrays of cores in storage arrangements simulated at Rocky Flats and, finally, by other measurements at an actual storage site.

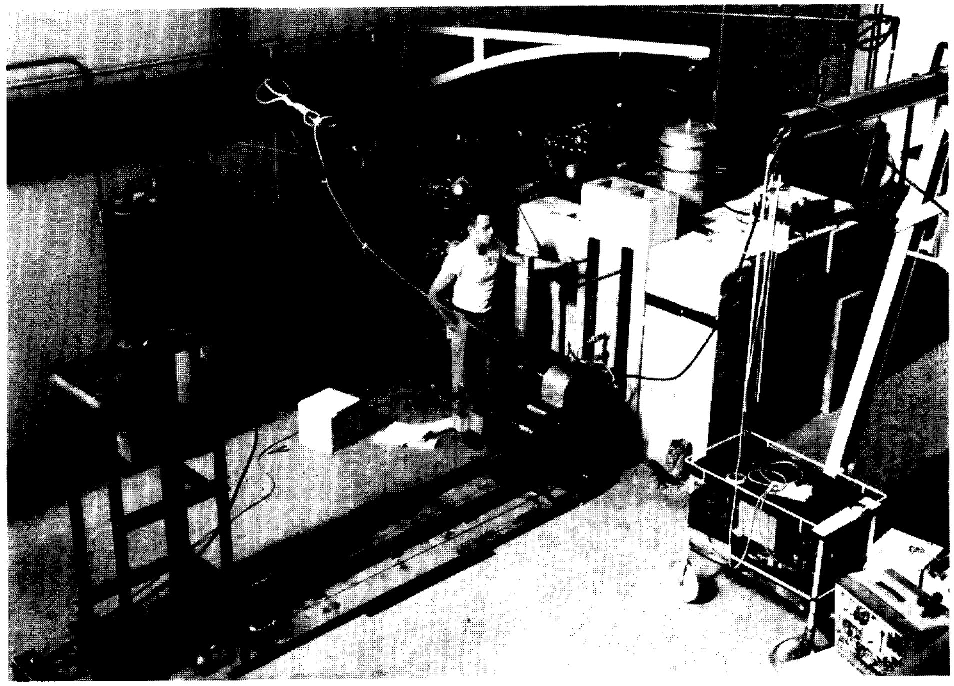


Fig. 4. A concrete vault in Kiva I for criticality tests on weapon cores arranged as they might be during storage. As many as 27 cores (the country's entire stockpile) were supported on two lightweight frames similar to jungle gyms (within the vault in this photograph and shown schematically in Fig. 5). Each frame was mounted on a track and could be moved in and out of the vault by remote control. A portion of the vault wall-a "door''—moved with each frame. Raemer Schreiber is shown beside the one visible drive mechanism and track (the other drive mechanism and track are hidden behind the vault). The number of cores on the frames was increased a few at a time, and neutron multiplication was measured as the frames were moved into the vault and the doors closed. Stringent security measures were maintained during these experiments, including a special contingent of military guards, machine gun emplacements on the walls of Pajarito Canyon, and a requirement that all personnel wear distinctive jackets while moving between buildings. Operations were conducted around the clock to minimize the time the stockpile was removed from its usual location.

Only once did we use a live weapon for measurements at Pajarito Site. The purpose was to determine how well our high-explosive mockup material simulated the neutron reflection and moderation properties of real high explosive. The tests were performed on Sunday so that few people would be at risk if something should go wrong. There was one scary moment when the capsule assembly stuck as it was being inserted by remote control into the high explosive. (Neutron multiplication was so low that this difficulty was corrected easily by hand.) On comparing notes with those who brought the high-explosive assembly, we learned that they breathed a sigh of relief when they left our dangerous fissile material behind, just as we did when they departed with their dangerous high explosive.

At no other time was explosive permitted at our facility. Over the years mockup material was improved to simulate precisely the elemental composition of high explosive. Thus it became prudent to test the material to be sure that the simulation was not so good that it, too, might be explosive.

Criticality considerations for gun-type weapons differed from those for implosion weapons because of the requirement that the total mass of fissile material become supercritical as soon as its subcritical components were engaged. Experiments on a new design first established the total fissile mass needed for the weapon. Then, the measured separation of components at criticality provided a basis for choosing a safe initial separation. Other tests demonstrated safety of assembly operations, including reaching down into the cavity to perform manual adjustment with components in place. As gun devices became smaller than the Hiroshima weapon, experimental safety guidance had to include the effects of surrounding materials in, for example, the breech of a naval gun.

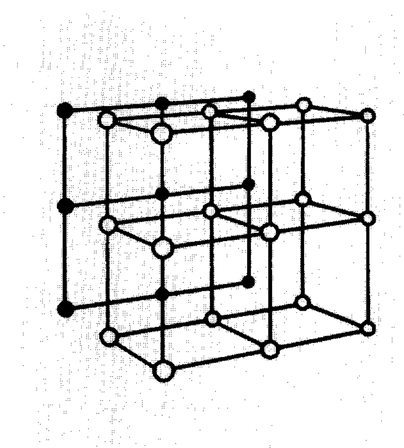


Fig. 5. Schematic arrangement of weapon cores during the criticality tests with the vault shown in Fig. 4. Two separate frames supported the cores at the positions represented by the solid and open circles.

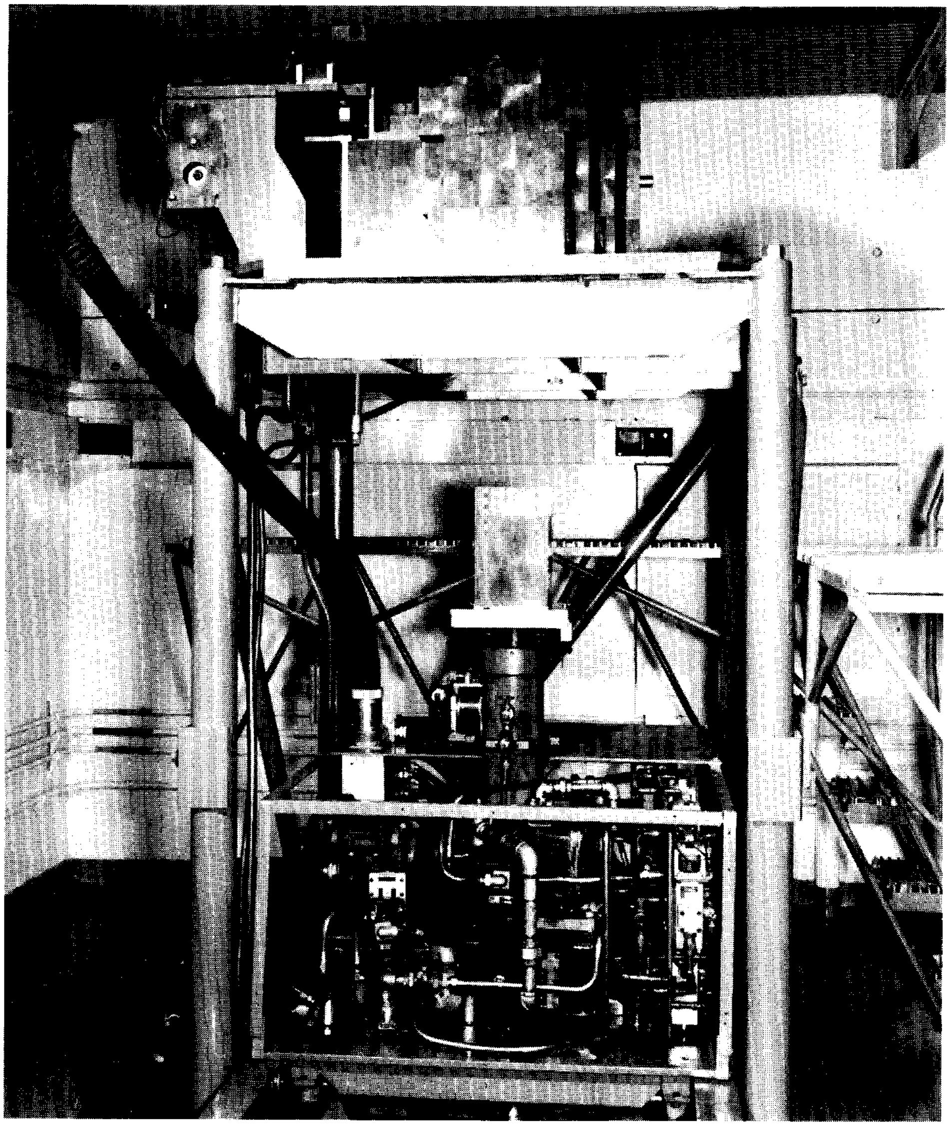


Fig. 6. The Topsy critical assembly. The central box-like structure contains an enriched-uranium core embedded in some natural uranium reflector. This structure is raised by remote control into a cavity in the main reflector body, the pile of large uranium blocks above. Spherical or cylindrical cores were approximated by arrays of half-inch cubes of enriched uranium.

Interaction among most simple implosion weapons of modern design is not a consideration except, perhaps, for clustered configurations. For some fission-fusion devices, however, interaction of weapons may be sufficiently important to require measurement. In one instance we tested an array of fission-fusion weapons that simulated a shipboard storage proposal. The tests were carried out at an assembly site because transportation of the weapons to a critical assembly facility was undesirable.

In the 1950s the critical assemblies group became involved in reactor-related activities culminating in the Rover rocket-propulsion reactor program. Although these activities eventually occupied most of our effort, weapon tests retained the highest priority. We had to be prepared for short-notice safety checks on each device destined for testing in the Pacific or Nevada. Typically, about one day was available for the safety check between completion of the device and shipment to the test site. Obtaining meaningful data on short notice was challenging but exhilarating.

Measured criticality data for easily calculated systems have also been of value for improving or confirming the detailed neutronic calculations that enter weapon design. The first critical assembly for this purpose (Fig. 6) began operating in late 1948. Named Topsy—she just grew—the assembly consisted of a nearly spherical core of highly enriched uranium embedded in thick natural uranium. Topsy was followed in 1951 by a bare sphere of highly enriched uranium, named Lady Godiva by Raemer Schreiber because, like the lady of Coventry, she was unclad. Ultimately we also obtained data on plutonium and uranium-233 assemblies as bare spheres and spheres reflected by thick natural uranium. Other simple assemblies consisted of combinations of fissile materials of interest to weapon designers, some in thin reflectors of various materials. Over the years hundreds of critical specifications have accumulated, which, when used for validation, have greatly expanded the range and reliability of detailed neutronic calculations.

Criticality control is necessary in aspects of the weapons program other than weapon safety. Accidental criticality must be avoided in the purification of fissile material, the production of metal, the fabrication of components, and the recovery of scrap. Other nuclear programs. such as the production of reactor fuel, involve similar operations and therefore require similar criticality information for safety measures. Criticality data from Los Alamos have been incorporated in compilations and safety guides and standards. Thus the scope of Los Alamos criticality safety activities has been national and even international. For example, Los Alamos has hosted two international meetings on criticality, and our short courses on criticality safety, conducted in cooperation with the University of New Mexico, have been attended by interested persons from other countries. ■

Further Reading

Hugh C. Paxton, "Thirty-Five Years at Pajarito Canyon Site," Los Alamos Scientific Laboratory report LA-7121-H, Rev. (1981).

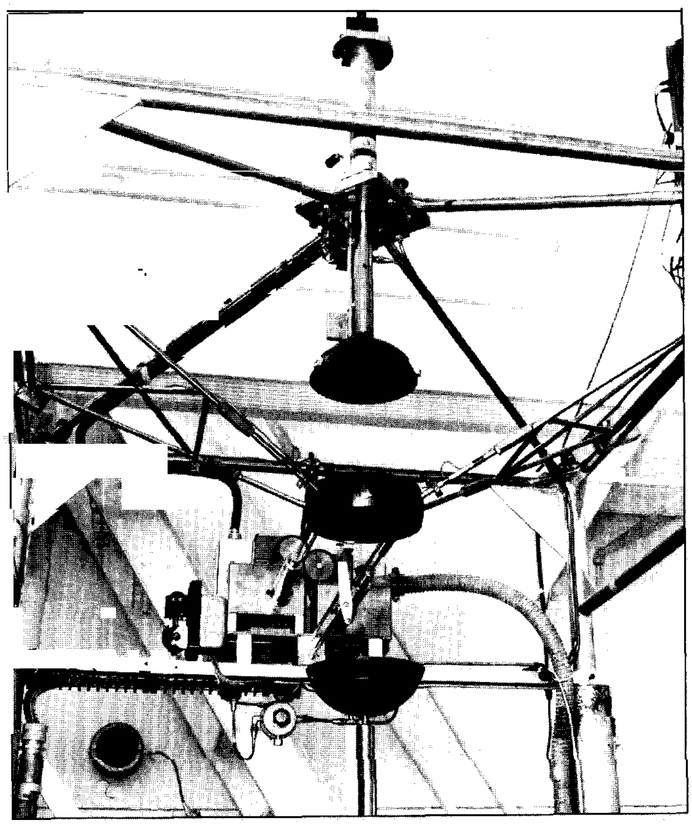
Hugh C. Paxton, "A History of Critical Experiments at Pajarito Site," Los Alamos National Laboratory report LA-9685-H (to be published).

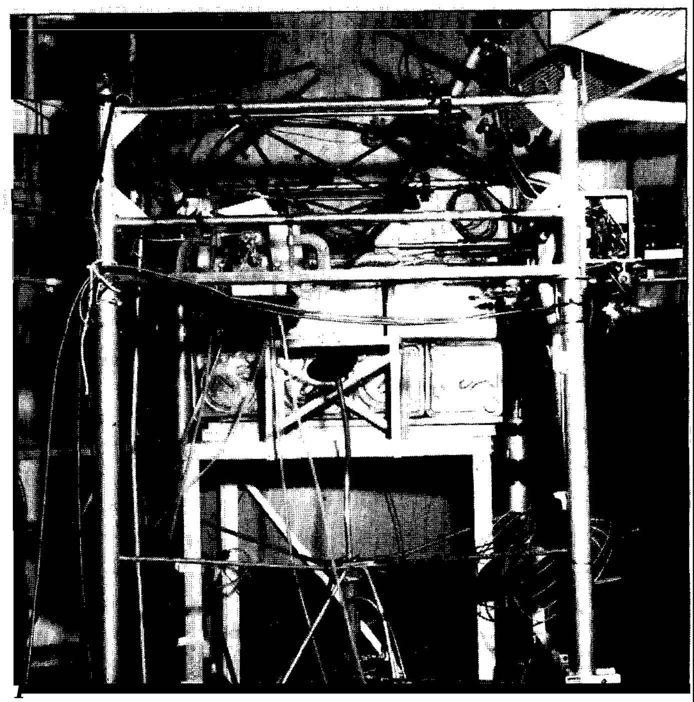
Prompt Criticality Under Control

Lady Godiva became the forerunner of the family of fast-pulse reactors at Los Alamos, Sandia National Laboratories, White Sands Missile Range, Aberdeen Proving Ground, and Oak Ridge National Laboratory. These reactors simulate the radiation from a weapon that occurs beyond the weapon's blast-damage range and therefore are used to test instruments, rocket guidance systems, and electronic equipment for proper functioning in the presence of a weapon burst.

In mid 1953 Lady Godiva, essentially an unreflected sphere of highly enriched uranium, was coaxed gingerly to prompt criticality (the usually forbidden region) and slightly beyond. The typical result was radiation from a sharp, intense fission puke terminated by expansion of the uranium. Although the intent was simply to confirm predictions about the assembly's behavior at superprompt criticality, these pulses were immediately in demand as nearly instantaneous sources of radiation for experiments in areas ranging from biology to solid-state physics, and soon they were used to proof-test instrumentation and controls that were supposed to withstand the radiation from a nuclear explosion.

The total of about 1000 prompt pulses from Lady Godiva was not without incident, for twice the safe limit beyond prompt criticality was overstepped. The first incident did not cause irreparable damage, but in the second uranium parts became too badly warped and corroded for further use. The assembly was then replaced by Godiva 11, designed specifically for burst production. This first of the fast-pulse reactors has been succeeded at Los Alamos by Godiva IV. ●





Top. The Lady Godiva critical assembly of highly enriched uranium. A nearly spherical, unreflected critical assembly was formed as the upper cap was dropped and the lower cap was slowly raised. Lady Godiva was portable and was even operated outdoors to eliminate the effects of neutron reflection from the kiva walls.

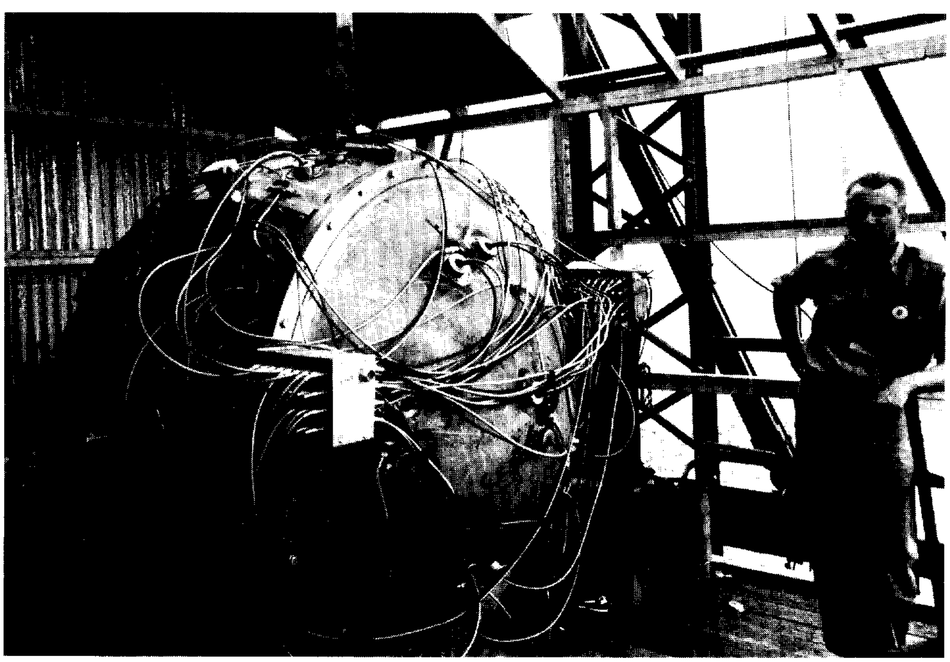
Bottom. Lady Godiva after the accident that led to her retirement. The enriched-uranium parts were severely warped and corroded, having approached the melting point at the center of the assembly. The support was damaged as a result of mechanical shock.

Weapon Design

We've Done a Lot but We Can't Say Much

by Carson Mark, Raymond E. Hunter, and Jacob J. Wechsler

The first atomic bombs were made at Los Alamos within less than two and a half years after the Laboratory was established. These first weapons contained a tremendous array of high-precision components and electrical and mechanical parts that had been designed by Los Alamos staff scientists, built by them or under their direction, and installed by them in much the same way as they might have put together a complicated setup of laboratory equipment. Immediately following the end of the war, a large fraction of those who had been involved with these matters left Los Alamos to resume activities interrupted by the war. They left behind little written information about the manufacture, testing, and assembly of the various pieces of a bomb.



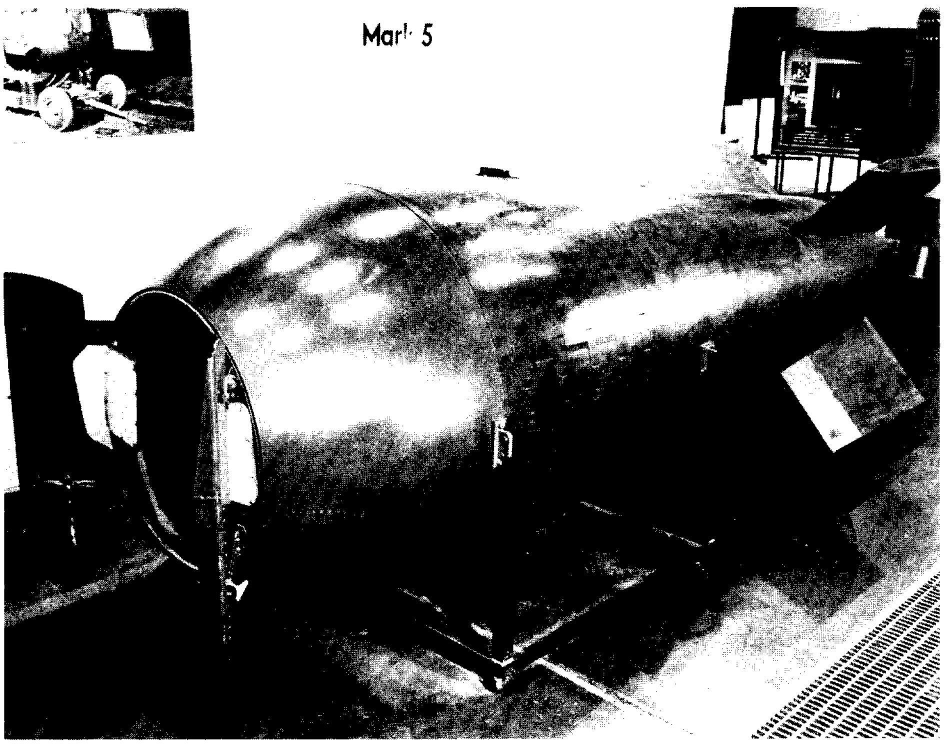
The Trinity device, the first nuclear weapon, atop the 100-foot tower on which it was mounted for the test on July 16, 1945. Norris Bradbury stands next to the device.

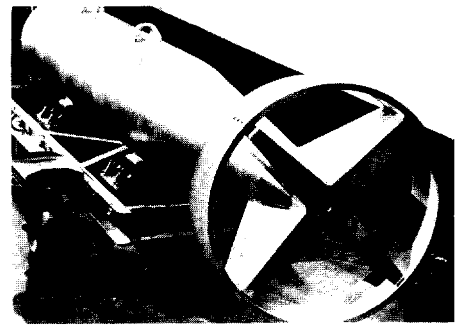
This gap had to be filled by the Laboratory, and particularly by the newly formed Z Division, which was responsible for ordnance engineering. Z Division had been moved to Sandia Base in Albuquerque where it could be in closer touch with the military personnel who might ultimately have to assemble and maintain completed weapons and where storage facilities for weapons and components were to be established.

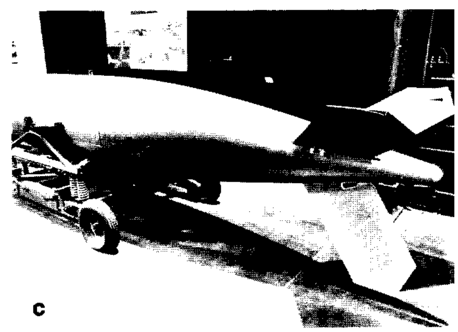
For several years the Laboratory people at Sandia, and many of those at Los Alamos, were heavily engaged in preparing a complete set of instructions, manuals, and manufacturing specifications, in establishing production lines for various parts, and in instructing military teams in the handling, testing, and assembly processes for weapons having the original pattern. Los Alamos continued to supply the more exotic components, including the nuclear parts, initiators, and detonators required for the stockpile.

At the same time, work at Los Alamos proceeded on developing a completely new implosion system, which evolved into the Mark 4, with improved engineering and production and handling characteristics. Successful demonstration of essential features of the new system, in the Sandstone

Mark 5







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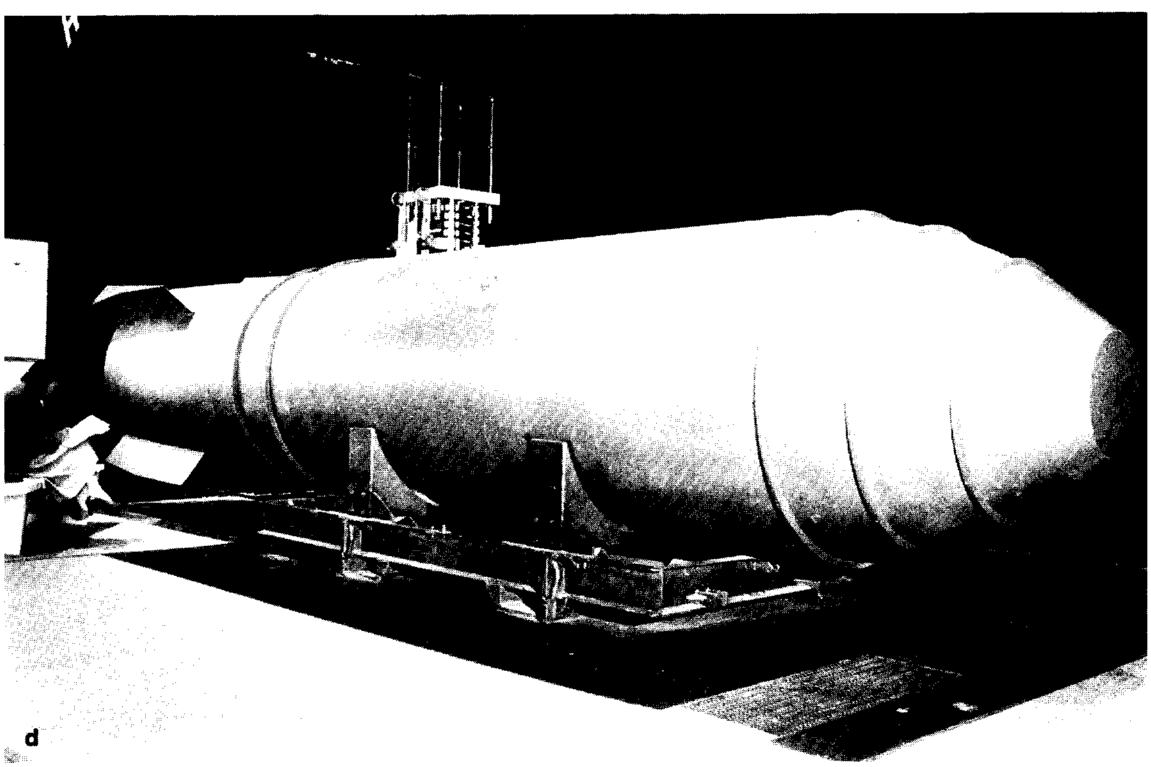

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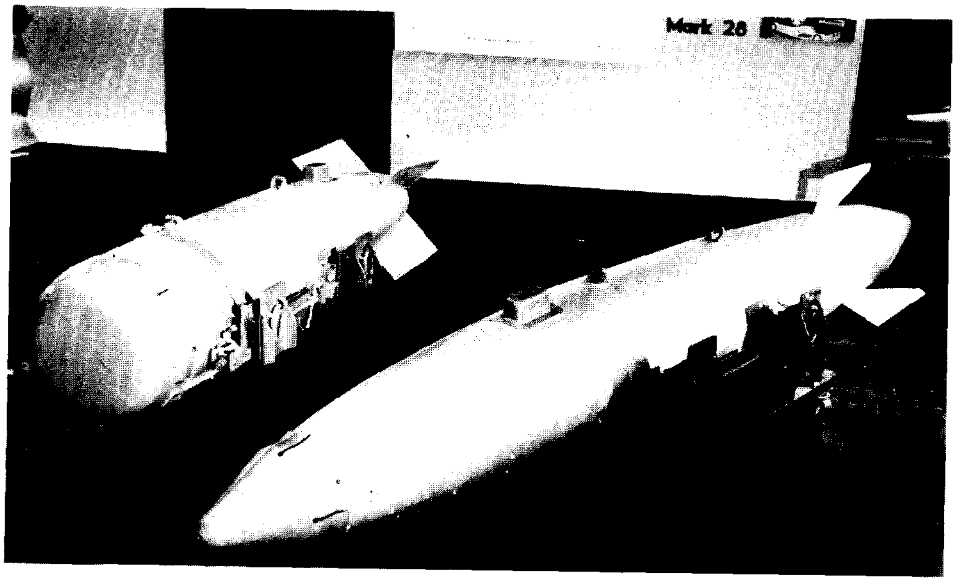
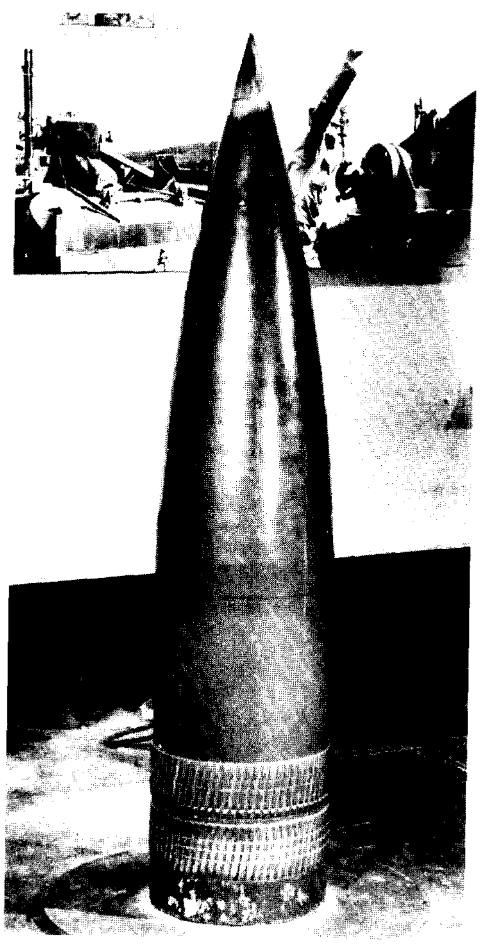




The nation's stockpile of nuclear weapons has included about fifty designed by the Laboratory, each having unique nuclear yield, size, weight, shape, ballistic performance, and safety features. Shown here are a number of early designs. (a) The Mark 5 was a smaller and lighter implosion weapon than previous designs. Its weight was one-third that of the Hiroshima weapon and one-half that of the Nagasaki weapon. The nuclear warhead was loaded through the doors in the casing. (b) The Mark 7, which could be carried on the outside of an airplane rather than in a bomb bay, added nuclear capability to smaller, faster fighter aircraft. (c) The Mark 8, an early penetration bomb, could penetrate 22 feet of reinforced concrete, 90 feet of hard sand, 120 feet of clay, or 5 inches of armor plate before detonating. (d) The Mark 17 was the first deliverable thermonuclear weapon. This massive bomb weighed 21 tons and could be carried in a B-36 after modifications were made to the bomb bay. Pilots who test-dropped the weapon reported that the plane rose hundreds of feet after the weapon was dropped, as if the bomb released the plane rather than the reverse. (e) Two weapons armed with the W28 warhead. The W28 warhead was a high-yield, small-diameter thermonuclear device. (f) The Mark 19, a projectile weapon, added nuclear capability to artillery that previously fired conventional shells. (Photographed at the National Atomic Museum, Albuquerque, New Mexico.)

test series at Eniwetok in the spring of 1948, ended the laboratory-style layout of weapons and opened the way for mass production of components and the use of assembly-line techniques. In addition, the Sandstone tests confirmed that the growing stockpile of uranium-235 could be used in implosion weapons, which were much more efficient than the gun-type weapons in which uranium-235 had previously been used.

In mid 1949 the Sandia branch of the Los Alamos Laboratory was established as a separate organization: the Sandia Laboratories, operated under a contract with Western Electric. New plants set up at various locations around the country gradually took over the production of components for stockpile weapons, although Los Alamos continued to carry appreciable responsibilities of this sort until some time in 1952.

The experience gained in the successful development of the Mark 4 put the Laboratory in a position to move much more rapidly and with more assurance on the development of other new systems. A smaller and lighter weapon, called the Mark 5, was tested successfully in 1951. Further advances followed very rapidly in subsequent test series and have resulted in today's great range of options as to weapon size, weight, yield, and other characteristics. The Laboratory can now prepare a new design for nuclear testing in a form that can readily be transferred to the manufacturing plants for production of stockpile models.

The early concern for safety in handling nuclear weapons, especially during the takeoff of aircraft, led to the development of mechanical safing mechanisms that ensured no nuclear explosion would occur until release of the weapon over a target. These mechanisms eliminated the tricky and somewhat hazardous assembly of the final components of a bomb during flight.

Studies of the possibilities of using thermonuclear reactions to obtain very large explosions began in the summer of 1942—almost a year before the Los Alamos Laboratory was formed. Such studies continued here during the war, though at a necessarily modest rate partly because the Laboratory's primary mission was to develop a fission bomb as rapidly as possible, partly because a fission bomb appeared to be prerequisite to the initiation of any thermonuclear reaction, and partly because the theoretical investigation of the feasibility of achieving a large-scale thermonuclear reaction—at least the "Classical Super" form then considered—was enormously more difficult than that required in connection with obtaining an explosive fission reaction. Studies of possible thermonuclear weapons continued here in the years immediately after the war, but these too were necessarily limited in scope. Only one of the small but capable group working on the Super during the war continued on the Los Alamos staff after the spring of 1946. In addition, the need for improvements in fission weapons was evident and pressing. And, for several years at least, the computing resources available here (or anywhere else in the country) were completely inadequate for a definitive handling of the problems posed by a thermonuclear weapon.

Nevertheless, in 1947 the pattern emerged for a possible "booster," that is, a device in which a small amount of thermonuclear fuel is ignited by a fission reaction and produces neutrons that in turn enhance the fission reaction. In 1948 it was decided to include a test of such a system in the series then planned for 1951. Following the first test of a fission bomb by the Soviets in August 1949, President Truman decided at the end of January 1950 that the United States should undertake a concerted effort to achieve a thermonuclear weapon even though no clear and persuasive pattern for such a device was available at that time. In May of 1951, as part of the Greenhouse test series, two experiments involving thermonuclear reactions were conducted. One, the George shot, the design of which resulted from the crash program on the H-bomb, confirmed that our understanding of means of initiating a small-scale thermonuclear reaction was adequate. The other, the Item shot, demonstrated that a booster could be made to work.

Quite fortuitously, in the period between one and two months preceding these experiments but much too late to have any effect on their designs, a new insight concerning thermonuclear weapons was realized. Almost immediately this insight gave promise of a feasible approach to thermonuclear weapons, provided only that the design work be done properly. This approach was the one of which Robert Oppenheimer was later (1954) to say, "The program we had in 1949 was a tortured thing that you could well argue did not make a great deal of technical sense . . . . The program in 1951 was technically so sweet that you could not argue about that." On this new basis and in an impressively short time, considering the amount and novelty of the design work and engineering required, the Mike shot, with a yield of about 10 megatons, was conducted in the Pacific on November 1, 1952.

As tested, Mike was not a usable weapon: it was quite large and heavy, and its thermonuclear fuel, liquid deuterium, required a refrigeration plant of great bulk and complexity. Nevertheless, its performance amply confirmed the validity of the new approach. In the spring of 1954, a number of devices using the new pattern were tested, including the largest nuclear explosion (about 15 megatons) ever conducted by the United States. Some of these devices were readily adaptable (and adopted) for use in the stockpile.

Since 1954 a large number of thermonuclear tests have been carried out combining and improving the features first demonstrated in the Item and Mike shots. The continuing objective has been weapons of smaller size and weight, of improved efficiency, more convenient and safe in handling and delivery, and more specifically adapted to the needs of new missiles and carriers.

Other developments in weapon design, though less conspicuous than those already referred to, have also had real significance. Some of the more important of these have to do with safety. The rapidly developing capability in fission weapon design made it possible to design a weapon that would perform as desired when desired and yet that would have only a vanishingly small probability of producing a measurable nuclear yield through an accidental detonation of the high explosive. Thus, the mechanical safing systems were replaced by weapons that, because of their design, had intrinsic nuclear safety. Today all nuclear weapons are required to have this intrinsic safety.

Another major development in nuclear weapon safety has to do with the high explosives themselves. Most of the explosives that have been used in nuclear weapons are of intermediate sensitivity. They can reliably withstand the jolts and impacts associated with normal handling and can even be dropped from a modest height without detonating. Still, they might be expected to detonate if dropped accidentally from an airplane or missile onto a hard surface. Since, as noted above, all weapons are intrinsically incapable of producing an accidental nuclear yield, accidental detonation of the high explosive would not cause a nuclear explosion. Detonating explosive would, however, be expected to disperse any plutonium associated with it as smoke or dust and thereby contaminate an appreciable area with this highly toxic substance. To reduce this hazard, much less sensitive high explosives are, where possible, being employed in new weapon designs or retrofitted to existing designs.

A quite different development has to do with weapon security. In the event, for example, that complete weapons should be captured by enemy troops or stolen by a terrorist group, it would evidently be desirable to make their use difficult or impossible. A number of schemes to achieve such a goal can be imagined, ranging from coded switches on essential circuits (so that the weapon could not be detonated without knowing the combination) to self-destruct mechanisms set to act if the weapon should be tampered with. A variety of inhibitory features have been considered, and some have been installed on weapons deemed to warrant such protection.

A final development worthy of attention is the advent of "weapon systems." This term refers to the integration of a carrier missile and its warhead, that is. to the specific tailoring of the warhead to the weight, shape, and size characteristics of the missile—as in the case of a Minuteman ICBM or a submarine-launched ballistic missile. The missile-cum-warhead constitutes an integrated system that is optimized as a unit. This integration contrasts with the earlier situation in which nuclear devices were to be taken from a storage facility and loaded on one or another suitable plane (or mated to a separately designed re-entry vehicle) to meet the mission of the moment. One should also note that the great improvements realized in missile guidance and accuracy have made it possible to meet a given objective with a smaller explosion and, hence, a smaller nuclear device. A missile can therefore now carry a number of warheads, each specifically tailored to meet the characteristics of the carrier. A consequence of integration is that the weapon system—a carrier with its warhead or warheads—is required to be ready for immediate use over long periods of time.

This change from general-purpose bombs to weapon systems has had significant effects on warhead design and production. For one thing, a very much larger premium attaches to reducing the maintenance activities associated with a nuclear device to an absolute minimum. Today, warheads require essentially no field maintenance and will operate reliably over large extremes in environmental conditions. As a separate matter, since a new carrier involves considerably greater cost and lead-time than does a new warhead, the production schedules (and budget limitations) for the carrier govern the production schedules and quantities of the warheads.

In response to the considerations mentioned here, as well as to new insights in explosive device behavior, a rapid evolution in design requirements and objectives has occurred and may be expected to continue. ■

Field Testing

The Physical Proof of Design Principles

by Bob Campbell, Ben Diven, John McDonald, Bill Ogle, and Tom Scolman
edited by John McDonald

For the past four decades, Los Alamos has performed full-scale nuclear tests as part of the Laboratory's nuclear weapons program. The Trinity Test, the world's first man-made nuclear explosion, occurred July 16, 1945, on a 100-foot tower at the White Sands Bombing Range, New Mexico. The actual shot location was about 55 miles northwest of Alamogordo, at the north end of the desert known as Jornada del Muerto which extends between the Rio Grande and the San Andres Mountains.

The actual detonation of a nuclear device is necessary to experimentally verify the theoretical concepts that underlie its design and operation. In particular, for modern weapons, such tests establish the validity of sophisticated refinements that explore the limits of nuclear weapons design. In addition, occasional proof tests are conducted of fully weaponized warheads before entry into the stockpile, and from time to time weapons are withdrawn from the stockpile for confidence tests. Also, tests characterized by a high degree of complexity are conducted to study military vulnerability and effects.

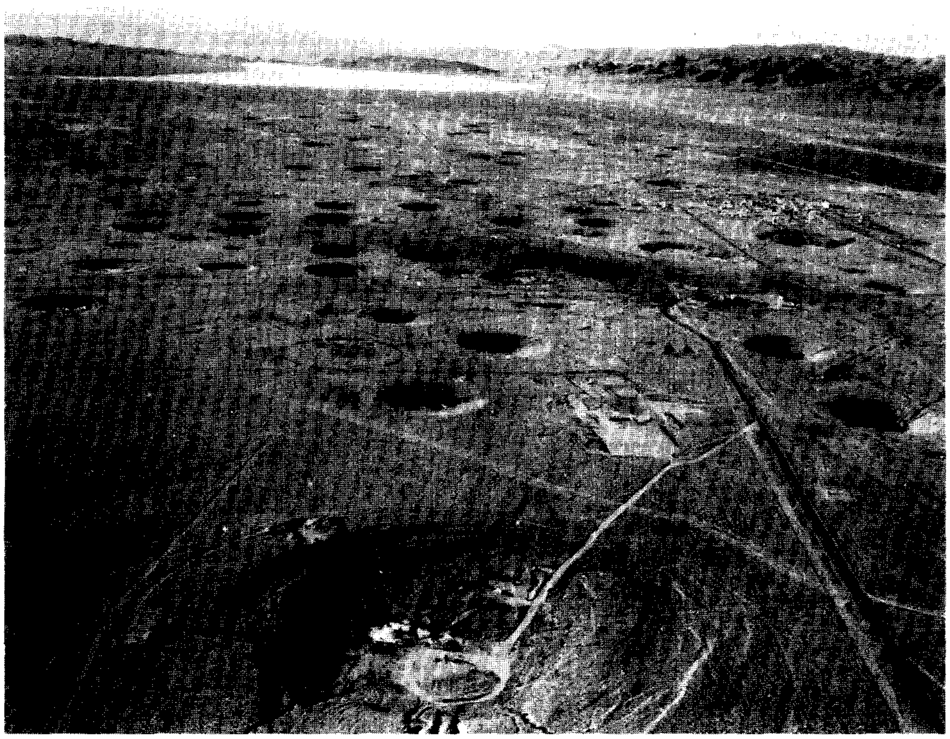
Information from test detonations assures that weapons designs which match their delivery systems can be produced in a manner consistent with the availability of fissile material and other critical resources. The interplay of field testing and laboratory design is orchestrated to optimize device performance, to guarantee reliability, to analyze design refinements and innovations, and to study new phenomena that can affect future weapons.

The advent of versatile, high-capacity computers makes it possible to model the behavior of nuclear weapons to a high degree of similitude. However, subtle and imperfectly understood changes in design parameters, such as small variations in mass, shape, or materials, have produced unexpected results that were discovered only through full-scale nuclear tests. Whereas the symmetry and compression of mock fissile material can be studied by detonating high explosives in a controlled laboratory environment without producing a nuclear yield, the actual performance of a weapon, particularly one of the thermonuclear type, cannot be simulated in any conceivable laboratory experiment and must be done in an actual nuclear test.

Field testing is the culmination of the imposing array of scientific and engineering effort necessary to discharge the Laboratory's role in developing and maintaining nuclear weapons technology to support the United States national security policy of nuclear deterrence. Embedded therein is the paradox: How do you test a bomb, undisguisedly an instrument of destruction, without hurting anyone?

From the beginning, field testing of nuclear weapons has followed commonsense guidelines that accord prudent and balanced concern for operational and public safety, obtaining the maximum amount of diagnostic information from the high-energy-density region near the point of explosion, and meeting the exacting demands of engineering and logistics in distant (and sometimes hostile) environments. The extreme boundaries of the arena of nuclear testing encompass tropical Pacific atolls and harsh Aleutian islands, rocket-borne reaches into the upper atmosphere, and holes deep underground. Since 1945, tests have occurred atop towers, underwater, on barges, suspended from balloons, dropped from aircraft, lifted by rockets, on the earth's surface, and underground. The locations evoke the words of a once-popular song, "Faraway Places with Strange-sounding Names''—Bikini, Eniwetok, Amchitka, Christmas Island; and nearer to home, at the Nevada Test Site (NTS), Frenchman Flat, Yucca Lake, and Pahute Mesa, among others. These names, no longer so strange sounding, have become familiar parts of the test community's language.

At various times between June 1946 and November 1962, atmospheric and under-



Aerial view of subsidence craters from underground nuclear tests in Yucca Flat at the NTS. The so-called Yucca Lake is in the background, and the Control Point complex is to the right of the dry lake.

ground tests were conducted by the U.S. principally on Eniwetok and Bikini Atolls in the Marshall Islands and on Christmas Island and Johnston Atoll in the Pacific Ocean; at the Nevada Test Site; and over the South Atlantic Ocean. Since November 1962, even before the atmospheric test ban treaty of 1963 came into effect, all U.S. nuclear weapons tests have been underground, most of them at the NTS, as part of an ongoing weapons program. Three underground tests were conducted on Amchitka Island in the Aleutians. Some tests for safety studies, peaceful uses of nuclear energy, and test detection research were conducted on the Nellis AFB Bombing Range in Nevada, and at other locations in Colorado, Nevada, New Mexico, and Mississippi. The accompanying table summarizes testing activities.

A nuclear test moratorium initiated in 1958 was ended abruptly in August 1961 when the Soviets resumed atmospheric testing. During the period of nontesting, the U.S. made substantial progress in its mathematical modeling capability, but because substantial preparations for atmospheric tests had not been made, it was not until the late spring of 1962 that atmospheric nuclear experiments could be fielded. Underground tests had been resumed in the early fall of 1961.

In conjunction with ratification of the Limited Test Ban Treaty (LTBT) in October 1963, the Joint Chiefs of Staff defined four safeguards, which, with the strong support of Congress, were to have significant impact upon the Laboratory.

The first safeguard was, in effect, a promise that the nuclear weapons laboratories would be kept strong and viable. The second called for a strong underground test program. The third concerned maintenance of the capability to return to testing in the "prohibited environments''—the atmosphere, underwater, and space—should that be necessary, and the fourth recognized the need to monitor carefully the nuclear test activities of other nations.

The first two safeguards provided new justification for underground testing, including tests purely scientific in nature. The third safeguard led to nonnuclear atmospheric physics tests in Alaska, northern Canada, and the Pacific region. The facilities and capabilities held in readiness for nuclear tests were used in many scientific endeavors, including solar eclipse expeditions and auroral studies. The fourth safeguard was responsible for triggering Laboratory activity in space, as Los Alamos developed a satellite test-monitoring capability that arose from the Vela program. This in turn has led to a number of first-rate scientific space programs.

At present, the Los Alamos test program is carried out by approximately 385 Laboratory employees from the Test Operations Office and various divisions, including WX, P, ESS, MST, INC, M, X, and H. Their efforts are supplemented by about 740 contractor employees of the DOE's Nevada Operations Office working at the NTS. Notable among the contractors are the Reynolds Electrical Engineering Company (REECo) for drilling and field construction, EG&G for technical support, Holmes and Narver (H&N) for construction architecture and engineering; and Fenix and Scisson (F&S) for drilling architecture and engineering. The dedicated efforts of all these people are necessary to execute nuclear tests as a vital element of the Los Alamos weapons program.

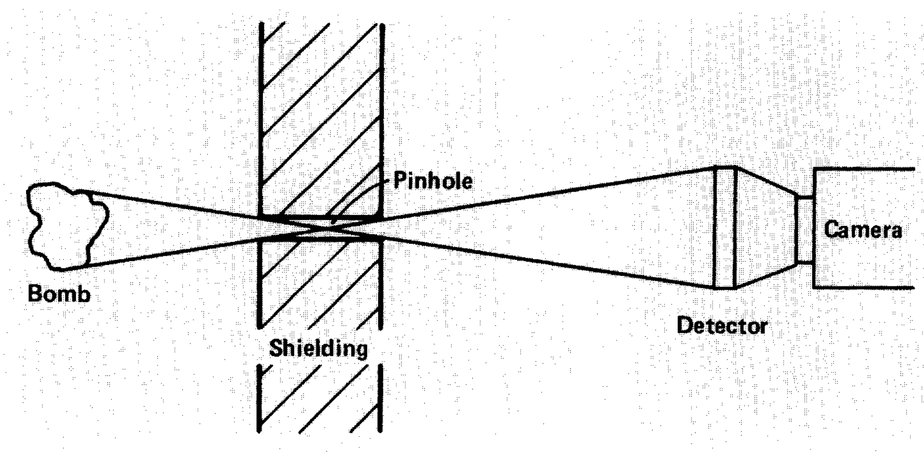
Diagnostics and Testing Technology

Before the Trinity test, estimates of its yield varied from zero to 20 or more kilo-

NUCLEAR WEAPONS TEST OPERATIONS[a]

Operation	Announced U.S. Nuclear Tests[b]	Dates	Location
Trinity	1	July 1945	Alamogordo New Mexico
Crossroads	2	June - July 1946	Bikini Atoll
Sandstone	3	April - May 1948	Eniwetok Atoll
Ranger	5	January - February 1951	Nevada Test Site
Greenhouse	4	April - May 1951	Eniwetok Atoll
Buster-Jangle	7	October - November 1951	Nevada Test Site
Tumbler-Snapper	8	April - June 1952	Nevada Test Site
Ivy	2	October - November 1952	Eniwetok Atoll
Upshot-Knothole	11	March - June 1953	Nevada Test Site
Castle	6	February - May 1954	Bikini and Eniwetok Atolls
Teapot	14	February - May 1955	Nevada Test Site
Wigwam	1	April 1955	East Pacific
Project 56	4	November 1955- January 1956	Nevada Test Site
Redwing	17	May - July 1956	Eniwetok and Bikini Atolls
Project 57	1	April 1957	Nevada Test Site
Plumbbob	30	May - October 1957	Nevada Test Site
Project 58	2	December 1957	Nevada Test Site
Project 58A	1	February - March 1958	Nevada Test Site
Hardtack Phase I	35	April - August 1958	Eniwetok and Bikini Atolls; Johnston Island
Argus	3	August - September 1958	South Atlantic
Hardtack Phase II	37	September - October 1958	Nevada Test Site
Nougat	45	September 1961- June 1962	Nevada Test Site; Carlsbad, New Mexico
Dominic	31	April 1962- October 1962	Christmas and Johnston Islands
Fishbowl	5	July 1962- November 1962	Johnston Island
Storax	56	July 1962- June 1963	Nevada Test Site
Niblick	27	August 1963- June 1964	Nevada Test Site; Fallon, Nevada
Whetstone	35	July 1964- June 1965	Nevada Test Site; Hattiesburg, Mississippi
Flintlock	40	July 1965- June 1966	Nevada Test Site; Amchitka, Alaska
Latchkey	27	July 1966- June 1967	Nevada Test Site; Hattiesburg, Mississippi
Crosstie	30	July 1967- June 1968	Nevada Test Site; Dulce, New Mexico
Bowline	26	July 1968- June 1969	Nevada Test Site
Mandrel	42	July 1969- June 1970	Nevada Test Site; Grand Valley, Colorado; Amchitka, Alaska
Emery	10	October 1970- June 1971	Nevada Test Site
Grommet	12	July 1971- May 1972	Nevada Test Site; Amchitka, Alaska
Toggle	11	July 1972- June 1973	Nevada Test Site; Rifle, Colorado
Arbor	5	October 1973- June 1974	Nevada Test Site
Bedrock	15	July 1974- June 1975	Nevada Test Site
Anvil	18	September 1975- August 1976	Nevada Test Site
Fulcrum	11	November 1976- September 1977	Nevada Test Site
Cresset	16	October 1977- September 1978	Nevada Test Site
Quicksilver	16	November 1978- September 1979	Nevada Test Site
Tinderbox	15	November 1979- September 1980	Nevada Test Site
Guardian	16	October 1980- September 1981	Nevada Test Site
Praetorian	22	October 1981- September 1982	Nevada Test Site
Phalanx	—	November 1982-	Nevada Test Site

[a] *The Hiroshima and Nagasaki detonations of World War II were August 5 and 9,1945, respectively.*
[b] *All tests before August 5, 1963, and after June 14, 1979, have been announced.*



Schematic of a pinhole imaging experiment.

tons. Even if the yield had been known in advance, estimates of the effects of the explosion were based on speculation plus some extrapolation from a 100-ton shot of high explosive, This rehearsal shot, consisting of 100 tons of TNT laced with fission products, was made prior to Trinity to provide calibration of blast and shock measurement techniques and to evaluate fallout. The yield of Trinity was measured by observation of the velocity of expansion of the fireball as photographed by super-high-speed movie cameras, by radiochemical analysis of the debris, and by observation of blast pressure versus time and distance. If the yield had been disappointingly low, the most important diagnostic for understanding the reason for failure would have been measurement of the generation time, that is, the length of time spent in increasing the fission reaction rate by a given factor. Effects measurements were needed to predict the damage that would be done to the enemy by blast and radiation and also to evaluate possible damage to the delivery aircraft.

The Trinity measurements were amazingly successful considering it was the first shot observed. The photographic coverage was superb. The fireball yield technique was confirmed by radiochemical data. The generation-time data were successfully recorded on the only calibrated oscilloscope fast enough to make the measurement. Observations of debris deposition patterns led to the first fallout model. Dozens of other experiments, such as blast pressures versus distance, neutron fluences in several energy ranges, gamma-ray emissions, and thermal radiation effects, also gave useful data.

Postwar tests had the same general requirements for diagnostics as Trinity, but allowed more time for diagnostic development to improve the original techniques and to add new measurements. Yield is still measured by radiochemical techniques that were pioneered for Trinity, although they have been greatly improved upon since then. In addition, for as long as atmospheric testing was done, fireball measurements gave reliable yield determinations. Methods were developed to obtain the yield from accurate measurements of the spectrum of neutrons from the devices by careful observations of the emerging gamma rays, and, for underground shots, where a fireball cannot be observed, from the transit velocity of the shockwave through the ground. Generation-time measurements that covered only a small interval of the complete reaction history of the Trinity explosion have been expanded to cover changes in reaction rate and gamma output over as many as 17 orders of magnitude. Detectors and recording equipment have been developed to follow the later faster reacting devices. Methods have been developed to observe the flow of radiant energy that emerges from a device in the form of low energy x rays by observation of the x-ray spectrum as a function of time. Along with development of the various diagnostic detectors have been improved methods of transmitting data from detector to the recording stations. In addition to use of coaxial cables. which were first used at Trinity, we now use modern instrumentation that includes fiber optics, digital systems, and microwave transmission.

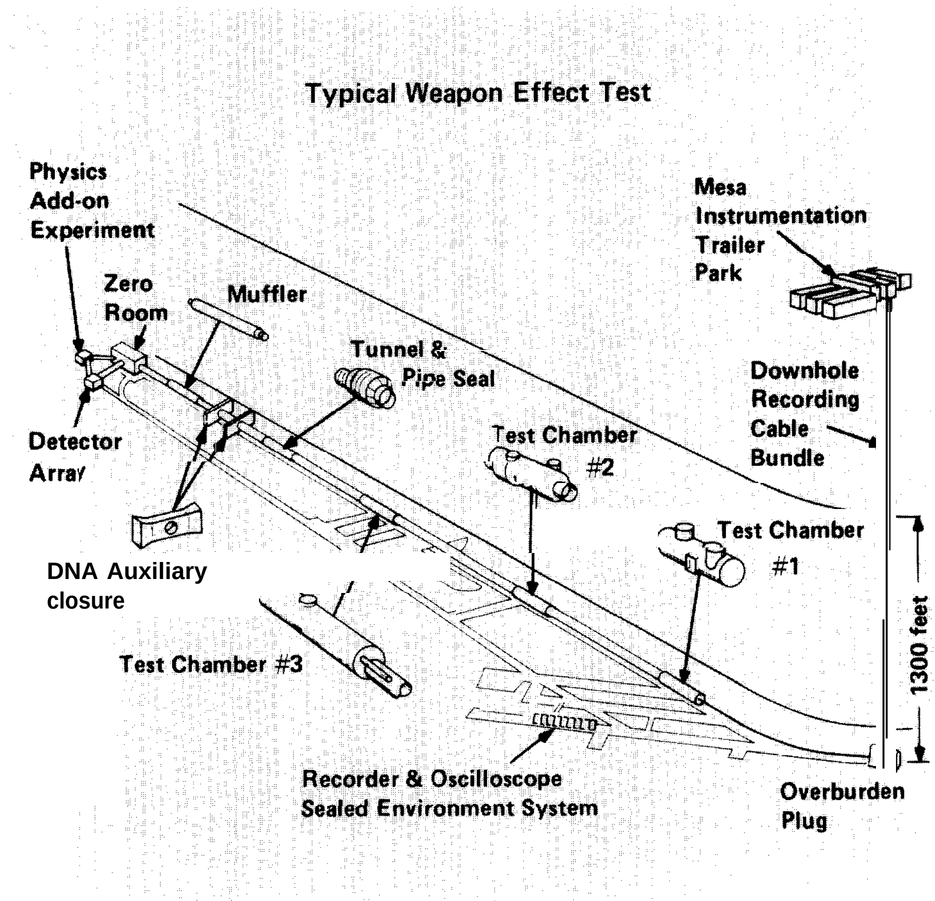
Photographic coverage of atmospheric events, starting with Trinity, reached a peak of perfection in the art of high-speed data recording, calling on the combined intellectual and technical resources of the Laboratory as well as a number of contractors, notably Edgerton, Germeshausen and Grier, who made significant contributions in oscilloscope and photographic technology, and the Naval Research Laboratory and the University of California Radiation Laboratory, who were successful in carrying out highly complex experiments. The innovations born of this expertise have proliferated beyond nuclear weapons testing to find application in many scientific activities requiring high-speed data resolution, ranging from endeavors as separate as studies of transient phenomena of interest in fusion energy release for civilian power to picosecond cameras used in studies of photosynthesis.

As a more detailed example of an experiment on a weapons test, consider a very useful diagnostic tool developed during atmospheric testing and modified and refined for underground use. A pinhole camera is used to take a picture of the actual shape and size of the fissile material of a fission bomb as it explodes or of the burning fuel in a thermonuclear bomb. A tiny pinhole through a thick piece of shielding located between the exploding device and a detector projects an image of the device onto the detector.

Gamma rays and neutrons from the reacting material are transmitted through the bomb parts, such as high explosive and bomb case, and reach the detector (for example, a fluor) and cause it to light up with a brightness proportional to the intensity of incident radiation. The resulting image is a two-dimensional picture of the reacting fuel, as seen through the bomb debris. The brilliant light and x rays from the bomb surroundings are eliminated by a thin screen of metal between the bomb and the fluor. A TV camera then transmits the picture to a recording station. It is even possible by use of various schemes to produce gamma rays or neutron pictures of selected energies or to get several frames of motion of the reacting region separated by a few billionths of a second.

We were presented with new challenges when, in 1963 as a result of the LTBT, all tests had to be conducted underground. Underground emplacement of a nuclear device at the Nevada Test Site occurs in one of two basic modes: in a vertical shaft or a horizontal tunnel, with appropriate arrays of diagnostics for weapons development tests or for weapons effects and vulnerability studies. Of course, when any test is conducted for whatever reason, as many experiments and diagnostics measurements are added as can be accommodated in the limited volume of subsurface placement to make optimum use of the device's unique and costly output. Diagnostic information typically is obtained with sensors that "look" at the test device through a line-of-sight (LOS) pipe or by close-in sensors whose output is transmitted over coaxial or fiber optics cables to remotely located high-data-rate recorders. A variety of techniques is used to protect diagnostic equipment long enough to obtain and transmit data before being engulfed in the nuclear explosion.

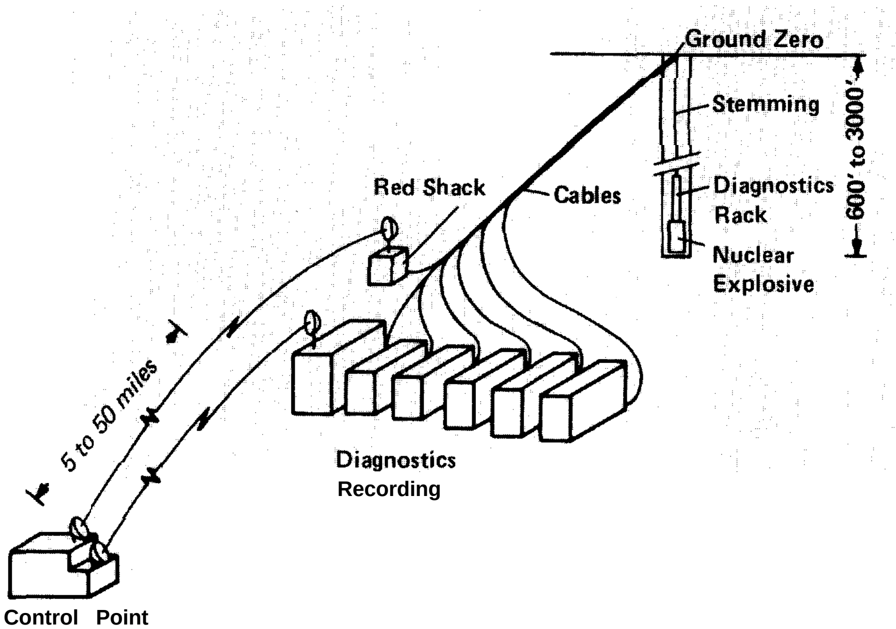
During atmospheric testing, we measured yield, radiation, blast, and thermal effects, but we also studied weapons phenomenology: how the weapons' outputs interacted



Cooperation between Los Alamos and the military services in weapons effects testing began soon after the close of World War II. The damage from atmospheric, underwater, and surface detonations was assessed by positioning a variety of military hardware at various distances from the device. When above-ground tests were prohibited, effects tests were transferred to horizontal tunnels deep underground. The figure shows a typical modern-day Defense Nuclear Agency effects test arrangement. A Los Alamos (or Livermore) supplied device is located in the Zero Room, which is connected to a long, horizontal line of sight (HLOS) containing several test chambers. Various rapid closure mechanisms in the HLOS allow radiation generated by the nuclear device to reach test chambers but prevent the escape of debris and radioactive gases. Following the test, military hardware and components that have been placed in the test chamber are retrieved and the effects of radiation exposure are evaluated at DNA contractor laboratories. The radiation output from the device provides a unique source for answering physics questions of interest to weapons designers. Occasionally such physics experiments are mounted simultaneously with effects tests. Usually the add-on experiments consist of one or more line-of-sight pipes with appropriate detectors as shown near the Zero Room in the figure.

with the environment and the effects of weapons-generated electric and magnetic fields. Information on these subjects gleaned from early tests has been extremely helpful with respect to present problems, specifically, the interference of electromagnetic pulse (EMP) signals with power grids, communication links, and satellites, and typical

Typical Weapon Development Test



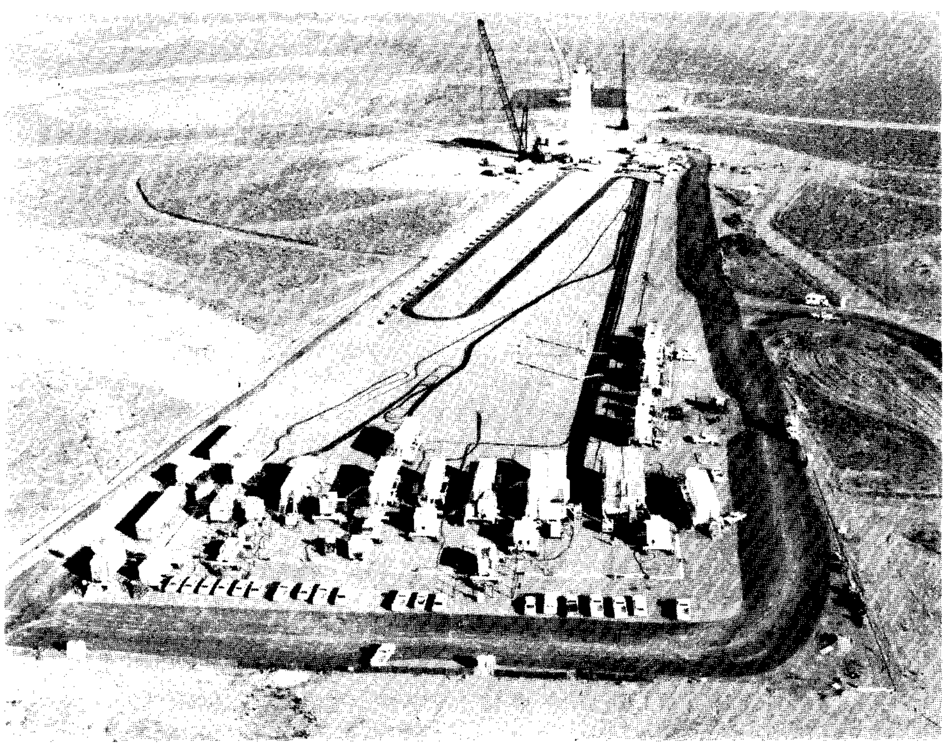
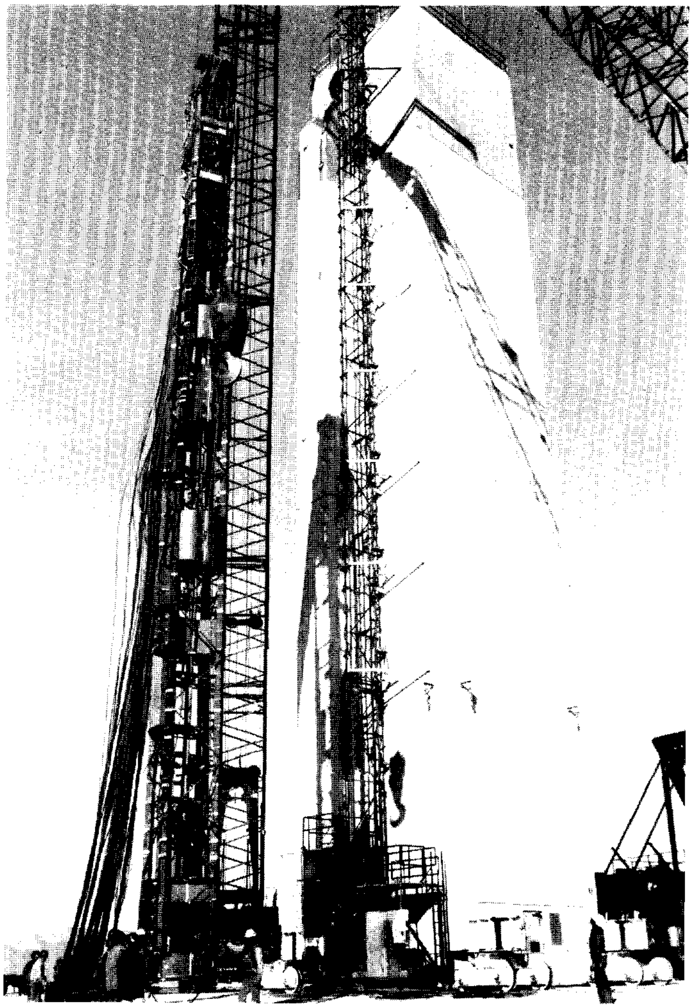
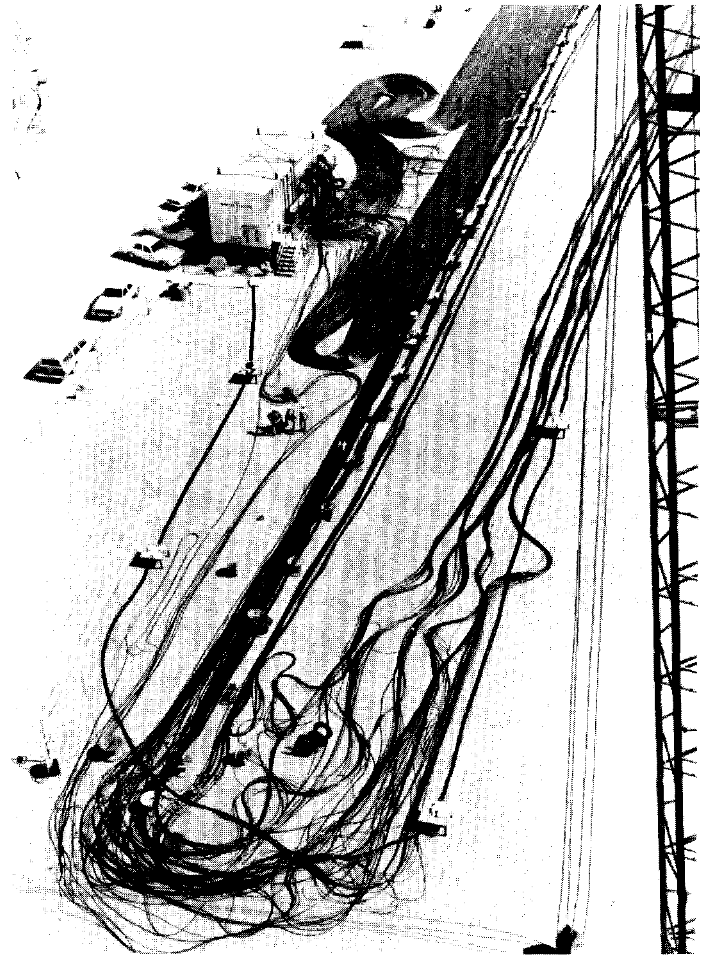


Diagram at left: Most weapons development tests are conducted in vertical shafts drilled deep into the ground. A rack holding the device, the associated firing components, and the diagnostics detectors and sensors is lowered into the emplacement hole and the she@ is backfilled with a combination of sand, gravel, concrete, and epoxy that stems the hole to ensure containment of the nuclear explosion. The test is fired by sending a specific sequence of signals from the Control Point to the "Red Shack" near Ground Zero. (The Red Shack houses the arming and firing equipment.) The diagnostics instruments detect outputs from the nuclear device and the information is sent uphole through cables. Usually within a fraction of a millisecond following the detonation the sensors and cables will be destroyed by the detonation, but by that time the data have been transmitted by cables to recording stations a few thousand feet from Ground Zero or by microwave to the Control Point. Photograph: Aerial view of Ground Zero rack tower, diagnostic cables, and diagnostic-recording trailer park. Final test preparations include emplacing miles of cable downhole. The cables will transmit vital test information to the diagnostics trailers in the foreground of the picture. A rack containing instrumentation to go downhole is assembled in the tower at the top of the picture.



A device diagnostics rack suspended from a crane prior to being installed inside the Ground Zero rack tower. The modular rack tower is erected over the emplacement hole to provide protection against wind and weather while diagnostics equipment is installed and prepared for the test. Finally the rack and the device canister are lowered into the hole, the rack tower is disassembled, and the hole is backfilled with appropriate stemming material.



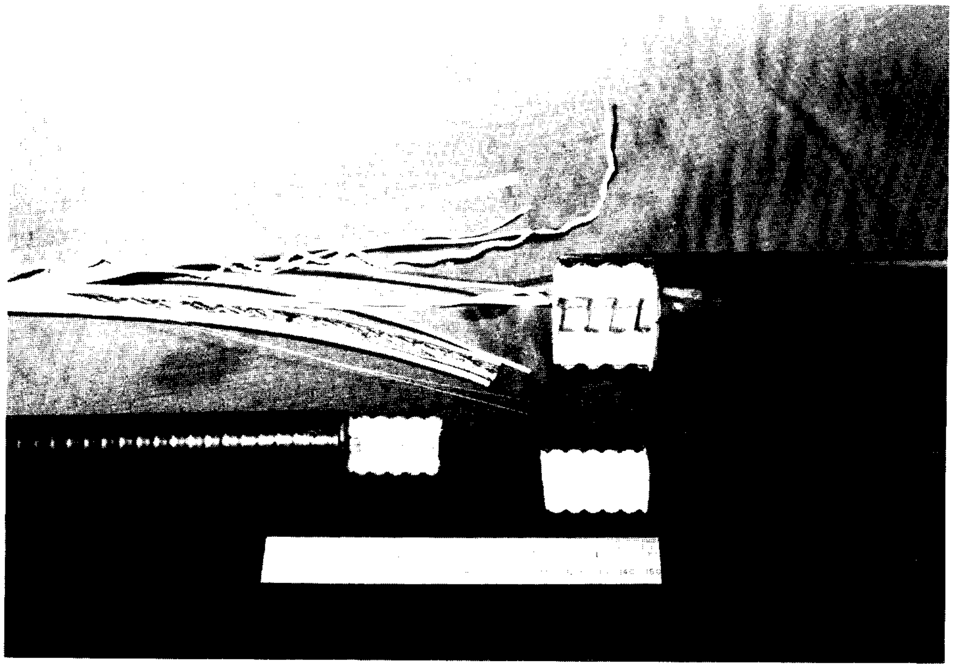
This photo contrasts the information capacity of fiber optics cables (orange) with those of coaxial cables (black). A single bundle of fiber optics cables (orange cable at lower right) carries data in the form of light signals from the underground diagnostics rack at Ground Zero to a photomultiplier station where the light signals are converted to electrical impulses. The coaxial cables exiting from that station transmit the data to the recording stations in the background. These stations house oscilloscopes that record the data on photographic film.

other weapons effects associated with prompt radiation and blast. While we can't study all of these problems underground, many weapons effects can still be observed. The Defense Nuclear Agency of the Department of Defense funds very complex tests of this nature and Los Alamos participates in these shots, frequently supplying and firing the nuclear explosive as well as making measurements of weapons effects.

From the time of the first nuclear explosion, there was speculation about non-military uses for these devices. Among the first scientific applications were contributions to seismology and meteorology. Knowledge of the exact time and location of nuclear explosions is particularly useful in obtaining information complementary to that from earthquakes. New chemical elements have been produced by nuclear explosions: specifically, the elements einsteinium and fermium were discovered in 1952 in the debris from a high-yield Los Alamos thermonuclear device. Los Alamos scientists have also applied nuclear tests to the measurement of nuclear physics data concerning reactions of nuclei with neutrons, particularly on those isotopes whose self-radioactivity tends to mask the data generated from the lower fluxes available in the laboratory.

When the Limited Test Ban Treaty of 1963 resulted in all of our nuclear tests being conducted underground, the necessary engineering developments were made which produced a line of sight from a deeply buried bomb to the ground surface. This line of sight remained open long enough for neutrons and gamma rays from the bomb to reach the surface, but was closed off by a variety of shutters and valves and ground shock before any radioactive debris could escape. With this system, a very nicely collimated beam of neutrons could be produced that was ideal for study of neutron-induced reactions. From 1963 to 1969, eight of these experiments were performed and produced a mass of useful physics data.

Except for state-of-the-art improvements in solid-state electronics, digitization of data, and miniaturization, some test diagnostics have changed relatively little since early testing experiments. which bears witness to the ingenuity of pioneers at the Pacific and



A fiber optics cable compared to three types of NTS coaxial cable. The two smaller coaxial cables (RF-19 and RF-13) are used downhole and the larger cable (RF-16) is used only for horizontal surface transmission. Each coax cable provides a single data channel; the fiber optics cable provides eight data channels. Depending on the quality of fiber used, the cost per fiber data channel is 1/3 to 1/6 the cost of the cheapest coax (RF-13) shown here. The fiber provides a bandwidth (data capacity) far exceeding that of coax cable. Fiber can provide a bandwidth above 1 GHz for a I km length; RF-13 cable can achieve 1 GHz over a 50 m length. The fiber cable is much lighter and smaller than the coax. Since it is nonmetallic, it precludes coupling of electrical interference from the test into sensitive recording instrumentation. Inside a rugged plastic sheath, layers of stranded Kevlar protect and strengthen the inner bundle of fibers. Each fiber is in a small plastic tube (8 in all) and each tube is filled with a gel material. A central strength member provides most of the tensile strength. This design totally precludes transfer of radioactive gas along the cable while providing excellent protection for the delicate fibers inside.

Interior of a diagnostics recording station with oscilloscopes and cameras.

Nevada proving grounds. It is a tribute of considerable magnitude to realize that some of the gear fielded at Trinity represented a new branch of technology that was born essentially full grown.

Engineering, Construction, and Logistics

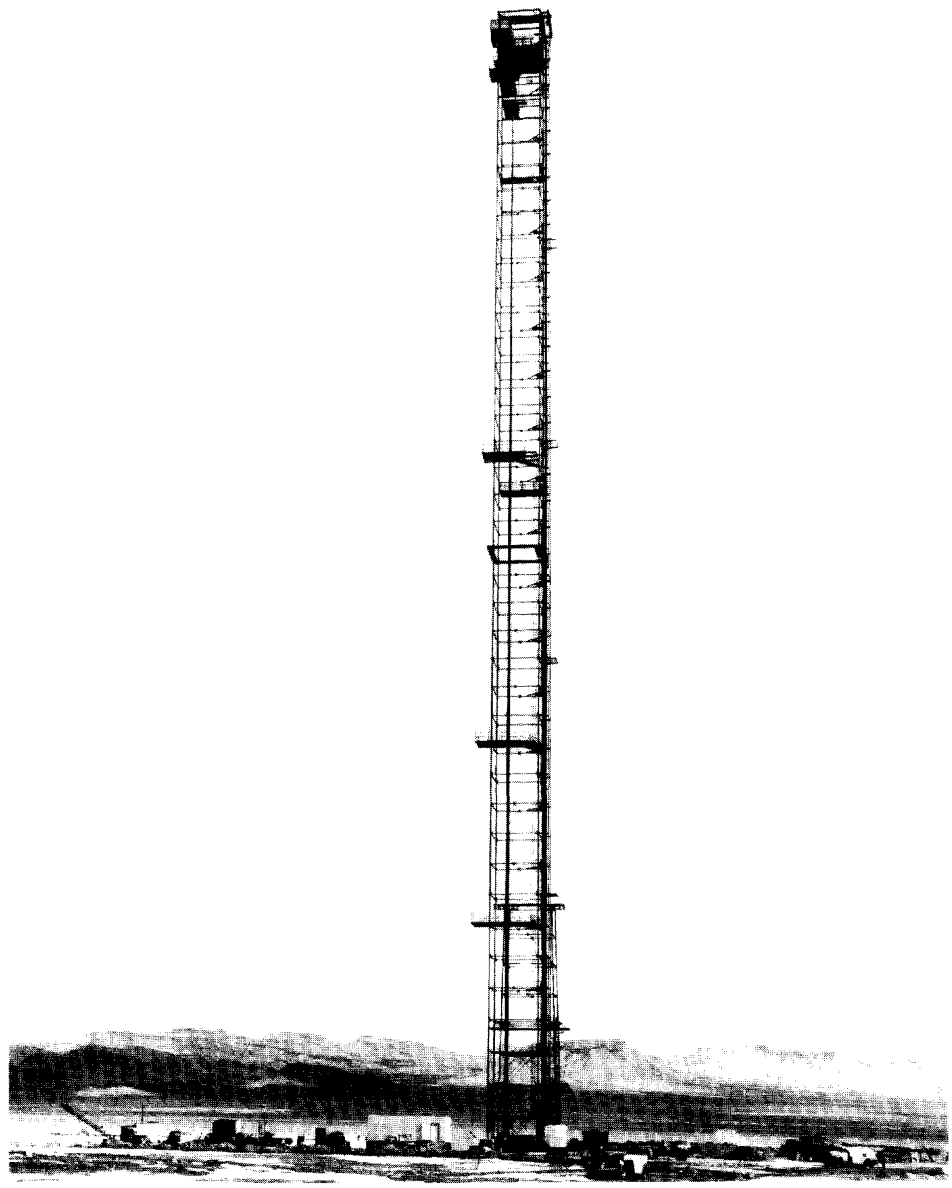
Early testing experience established a mode of operation, largely followed by Los Alamos participants ever since, that grew out of a habit of broad discussions among the experimenters and theoreticians leading to an agreed course of action. The early tests, apart from Trinity, were done on or near isolated islands in the Pacific. It was an enormous task to provide the necessary equipment, laboratory and shop facilities, spare parts, transportation, communications, living accommodations, and everything else needed to conduct test operations under difficult conditions on tight schedules far from home. Pacific operations atypically required planning over a two-year period because they presented extraordinary situations compared to most scientific and engineering undertakings. Some of the *ad hoc* solutions to vexing and unique problems established precedents that have proved admirably sound in the light of subsequent critical examination.

One specific engineering task was the construction of towers to support the test devices above ground. Our appetite for shot towers that could support bigger loads at greater heights was insatiable. Early towers needed only to support the device itself, some firing hardware, and perhaps a few detectors and coaxial cables, but we continued to add shielding and collimator loads as our diagnostics techniques developed. By the end of the atmospheric testing period, we were routinely accommodating tower loads of 100 tons distributed on any two of the four legs. Our desire for higher towers was driven by the operational problems created by the Trinity shot when activated or contaminated particulate matter was engulfed by the fireball and entrained in the resulting cloud. The Trinity shot was fired on a 100-foot tower. We progressed to 200 feet for Sandstone, 300 feet for Greenhouse, 500 feet for Teapot, and 700 feet for the Smoky shot of the Plumbbob series.

There are many true and untrue tales regarding towers. The tower for Greenhouse George was heavily loaded, but the story that you couldn't withdraw a bit after drilling a hole in the tower leg because the weight caused the hole to immediately become elliptical is not true. It is true, however, that users of the taller towers reported very perceptible motion at the top on windy days. which produced little enthusiasm for working under such conditions. People did get stuck in elevators when winds whipped cables about and once technicians even disconnected the power needed to fire the device while they were removing the tower elevator after the device was armed.

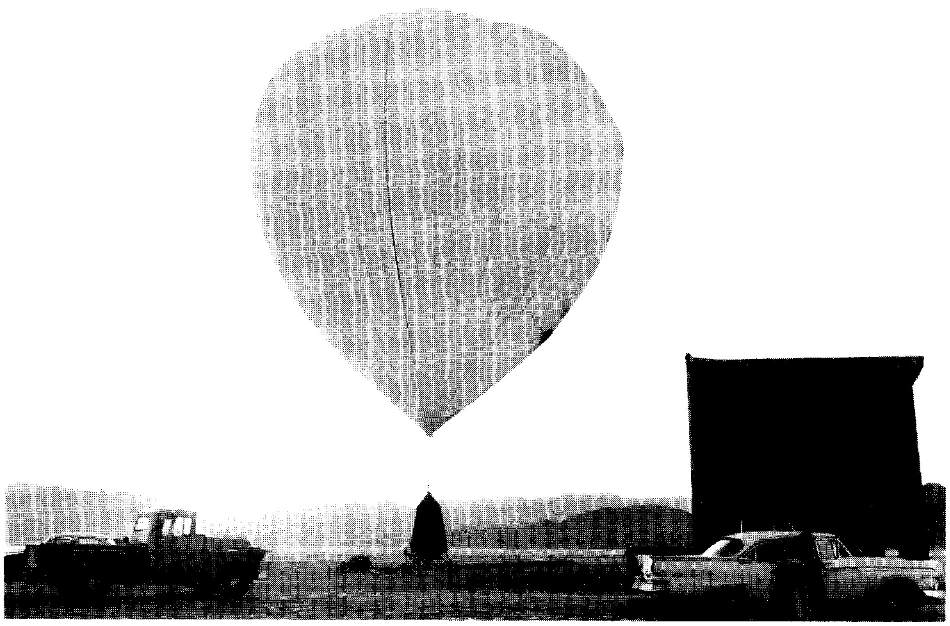
Towers were necessary for shots with elaborate diagnostics. but there were other shots whose purpose could be satisfied by air drops from military aircraft, although we were not always skillful enough to build targets that the Air Force could hit. In the Plumbbob series, several tests were conducted with devices suspended from tethered balloons in a system engineered and operated by Sandia Corporation. The balloons could not be inflated in high winds, but they significantly reduced the operational problem of fallout by allowing us to fire as high as 1500 feet above ground level.

Beginning with the Castle series of 1954, we were able to repeatedly fire large-yield devices in Pacific lagoons near fixed diagnostic stations on land by placing the devices on barges moored at the four corners to anchors on long scope. By adjusting the individual winches on each corner, we could hold barges to within a few feet of their required positions. Mercifully, the tidal variations at Eniwetok and Bikini are slight.

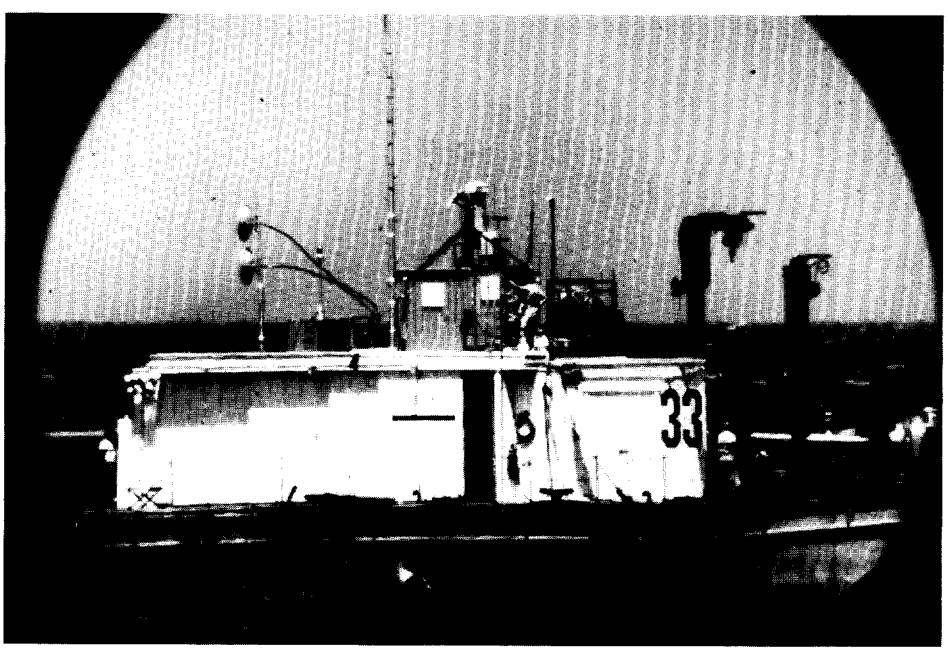


A test device mounted on a 500-foot tower at the Nevada Test Site. Taller and taller towers were built (to as high as 700-feet) to minimize entrainment of ground debris by the fireball and thereby reduce fallout resulting from the test.

Power was a problem both in the Pacific and Nevada. At NTS, power was generated well away from the shot areas, but both the above- and below-ground distribution systerns were subjected to ground shock which tended to make counting on postshot power a bit risky. In the Pacific, power was usually generated by diesel-driven generators near the point of use. The diesel engines would loaf along for hours under low loads and then die when the required large loads were



This photo of a balloon-carried test configuration was taken around 1957. The device is suspended from the balloon and the balloon is tethered to the ground by steel cables. With the balloon at the desired altitude (perhaps 1500 feet) the device was fired by sending electrical signals through the firing cables that connected the device with the firing system on the ground.



An early barge-mounted test configuration at the Pacific Proving Grounds. The nuclear device is housed in the shot cab (white structure).

imposed minutes before shot time. Experimenters were plagued at both sites by the quality of the power and by the effects of the test-generated EMP carried on the power distribution system. EMP shielding ranged from continuously soldered solid-copper lining of the recording rooms, to screened rooms, to no screening except that provided by reinforcing bars in the structural concrete—each according to the tenets of the individual experimenter. Power and timing signals were sometimes brought in on insulated mechanical couplings (with a motor or relay outside the shielded volume coupled mechanically to a generator or relay inside). Continuity of power was sought by several stratagems that included replacing fuzes with solid wire. Breakers in substations were wired closed to prevent ground motion or EMP from operating them, Automatic synchronizing and transfer equipment was designed to run generators in parallel and pass the load back and forth as necessary. This proved to be unreliable. so we ended up running several generators, each of sufficient size to carry the whole load and each carrying a dummy load, each of which could be dropped if any one or more of the generators running in parallel failed.

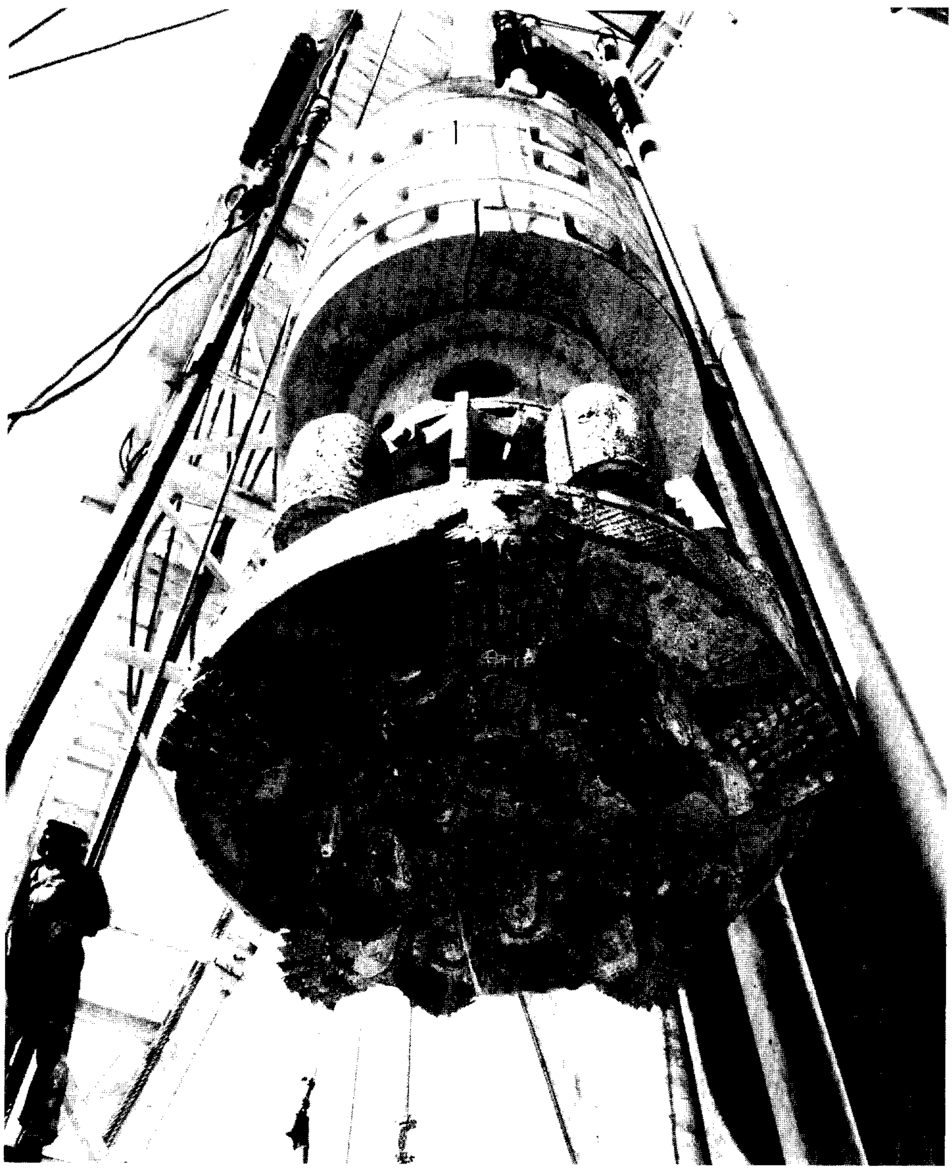
Concrete was a problem in the Pacific, since the only available aggregate was coral and we had to use salt water. Several mixes were invented, some to provide the required strength for recording stations and some to match the strength of normal construction concretes so that we could have valid effects tests on typical military and civilian structures. At both sites we learned to calculate and design shielding for collimators and their recording equipment. The resultant design of massive structures tended to err on the conservative side. The high-density concrete made by loading the mix with limonite ore, iron punchings, and the like gave densities triple that normally encountered, but was rough on mixing equipment and difficult to emplace. On some stations that had to function in close proximity to megaton-class devices, the center-to-center spacing of reinforcing steel approached its diameter and presented a very difficult job for the construction worker. There was a legend, never confirmed. that some iron bars which had been included for shielding in the design of a structure near Ground Zero were omitted in the construction because the superintendent "knew very well that the structure would stand without them."

Our initial experience in drilling the deep emplacement and postshot sampling holes was instructive. It must be the custom in the drilling industry to do whatever the man paying the bills asks, and not proffer any suggestions, for we were permitted to reinvent a number of existing drilling techniques. particularly in postshot drilling for radiochemical samples. Once Fenix and Scisson, Inc., came aboard as drilling and mining architect-engineer (A-E) and REECo took over enough of the drilling previously done by contract drillers to provide continuity, our lot improved. Big-hole drilling techniques were developed which are now accepted throughout the industry. We learned to extract postshot samples of device debris without releasing radioactivity to the atmosphere. Drilling times have improved even though the diameters of emplacement holes have increased from two to eight feet, and postshot operations that once took more than a month are now done in a safer and contained fashion in less than a week.

None of this work could have been done without the complete cooperation of the contracting officers and the sometimes heroic efforts of the architect-engineers and constructors in support of the laboratories. A real "can-do" attitude on the part of all concerned has been the trademark of the weapons testing community since Trinity.

For the earliest tests, namely Trinity and Crossroads, engineering and construction of scientific facilities, camps, utilities, communications, and the like were accomplished by military forces. For Sandstone, Army Engineers were used by the AEC because there wasn't time to obtain private contractors, but much of the building design and specifications were done by the firm of Johnson and Moreland. Liaison between these two parties was done by the Sandia Laboratory. whose engineers handled many details for Los Alamos. The Santa Fe Operations Office (SFOO), Office of Engineering and Construction, employed Holmes and Narver (H&N) as architect-engineer (A-E) and constructor for Greenhouse; and all subsequent Pacific testing and liaison with the AEC and its contractors became the responsibility of a small group, J-6, at Los Alamos. For logistics of construction for Ranger, SFOO employed the Reynolds Electrical Engineering Company (REECo) in a joint venture with R. E. McKee and Brown-

A multimegaton barge shot on Eniwetok in 1958,



A modern large-diameter drill bit, weights, and rigging used to drill device emplacement holes. Holes typically range from 600 to 3000 feet in depth and from 4 to 8 feet in diameter.

Olds. For Buster-Jangle, SFOO employed H&N as A-E although some of the later engineering was done on site by Haddock Engineering. At NTS, Haddock built Control Point Buildings 1 and 2 as well as the required construction work for Buster-Jangle in Areas 7, 9, and 10. For the Tumbler-Snapper series, REECo returned in the same type of arrangement as before, while maintenance work was done by the Nevada Company. a Haddock subsidiary. During this time. Haddock built the first structure at Camp Mercury—plywood hutments. REECo did construction and maintenance on Upshot-Knothole and all subsequent Nevada operations. Silas Mason served as A-E for operations Tumbler-Snapper through Teapot. REECo provided A-E support in addition to doing the construction for Projects 56 and 57. Holmes and Narver returned as A-E for Plumbbob and subsequent operations.

Firms and people have come and gone, but the fact that they sometimes had reason to believe our requests were unusual never reduced their fervor to help us field an operation. They, too, were pioneering to produce the facilities we needed to conduct this totally new business of testing nuclear weapons.

Readiness

Halloween night of 1958 saw an abrupt halt to the weapons tests that had continued more or less regularly since Trinity. During the test moratorium, which was agreed to by the U. S. and the Soviet Union in order to promote arms control and disarmament negotiations, no preparations for test resumption were authorized in the U. S. Nevertheless, when the Soviets resumed testing without notice in 1961, the test organization and the laboratories responded heroically; only ten days later they were able to tire the first United States underground test since the 1958 moratorium.

More difficult to accomplish than the bomber-dropped air bursts that comprised most of the early atmospheric tests after resumption of testing was the renewal of high-altitude testing, which employed rockets fired from Johnston Island to carry a variety of weapons to a wide range of altitudes, mainly to explore the effects that had only been hinted at during the last days of the Hardtack atmospheric operation. In 1963 the Limited Test Ban Treaty (LTBT) prohibited tests in the atmosphere, underwater, and in outer space, but it left under-

The USAF NC135A-369 containing the Los Alamos Airborne Diagnostics Laboratory. This plane, part of the atmospheric test readiness program, was available and ready to measure device performance in the event that atmospheric testing was resumed. Used during the 1960s and 1970s for several test readiness exercises and numerous purely scientific missions (solar eclipse, cosmic ray, auroral, and other), this plane is now retired.

ground testing unrestricted so long as no radioactive debris crossed international borders. Underground testing continues. limited by the Threshold Test Ban Treaty (limiting yields to 150 kilotons) which is observed although not ratified. The present testing activity provides some technological continuity that was not available when it became necessary to resume testing in 1961.

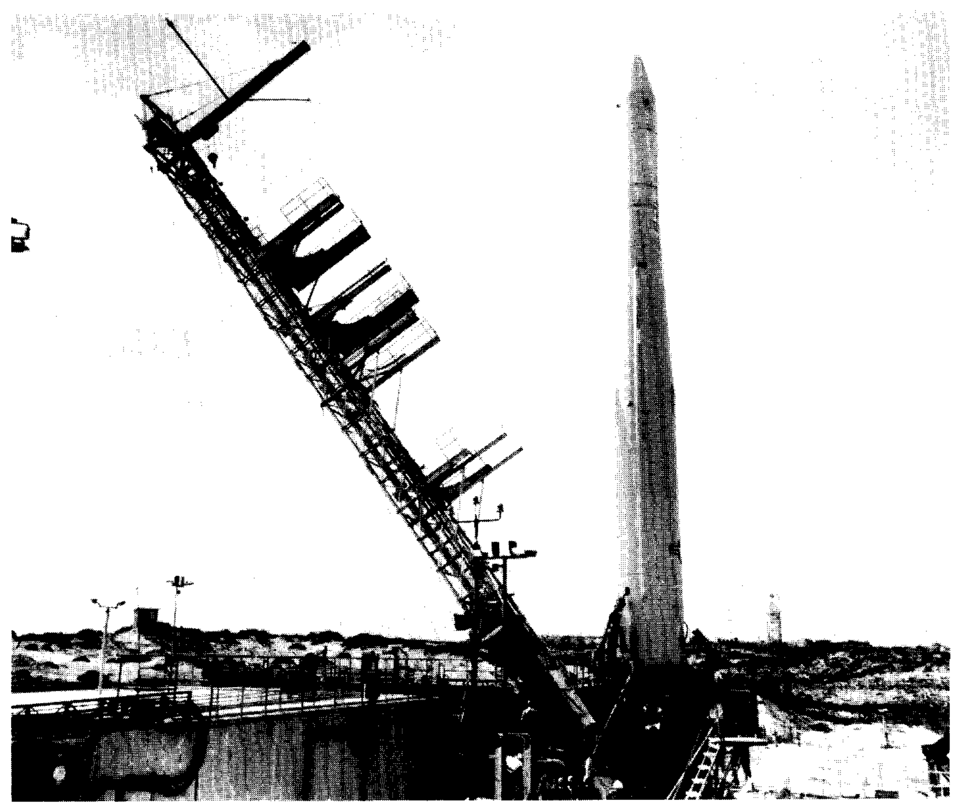
Maintenance of the capability to resume testing in the prohibited environments required not only continued training of a cadre of test personnel but also upkeep and modernization of extensive and sophisticated instrumentation, hardware. and facilities. Capabilities provided. for example, by operation of the NC-135A "flying laboratory" aircraft and the small-rocket range in Hawaii were periodically utilized to address questions about high-altitude detonations that were raised as a result of the 1962 atmospheric tests.

Experiments of a purely scientific nature. such as a series of solar eclipse observations from the aircraft, resulted in original scientific achievements while attracting other Laboratory scientists to the testing environment and preserving the scientific credentials of the base test cadre.

Our mandate to monitor international nuclear testing led to the birth of a space instrumentation and space science capability within the Laboratory. Beginning from design and fabrication of instruments for satellite-based test detection, this activity has evolved over the years to include a broadly based scientific space observation program with worldwide recognition,

Safety Considerations

Throughout the entire history of testing. operational and public safety have always been principal concerns. While the government agencies—first the Manhattan Engineer District, then the Atomic Energy Commission, later the Energy Research and Development Administration, and now the De-



A Thor missile, with gantry to the left, used in an ICBM weapon system simulation test on Johnston Atoll, August 1970. Some Los Alamos personnel served in an advisory role to the Task Force commander, while others aboard the Los Alamos flying-diagnostic-laboratory aircraft observed the missile launching and flight. This readiness exercise served as a very valuable and effective checkout of the missile system.

View of surface Ground Zero during the emplacement operation showing emplacement hardware and diagnostic cable bundle that connects the downhole equipment with the recording trailers. The small cylinders on the cables are gas blocks that prevent the flow of downhole gases through the cables to the atmosphere.

partment of Energy-have the responsibilities for the safe conduct of test operations, the Laboratory has always played an active role in safety matters. Because nuclear energy was totally new. every question related to nuclear hazards had to ‘be formulated before instrumentation could be built to gather the necessary data. This was as true for safety matters as for weapons diagnostics. In retrospect, the effort devoted to public safety. particularly as one notes the profusion of problems and unknowns. is very impressive. Pressure was applied from within the Laboratory to learn as much as possible, but to be very conservative in experimental design. As a result. the testing community has accumulated an outstanding safety record. In fact. the record is unique for a new, evolving technology.

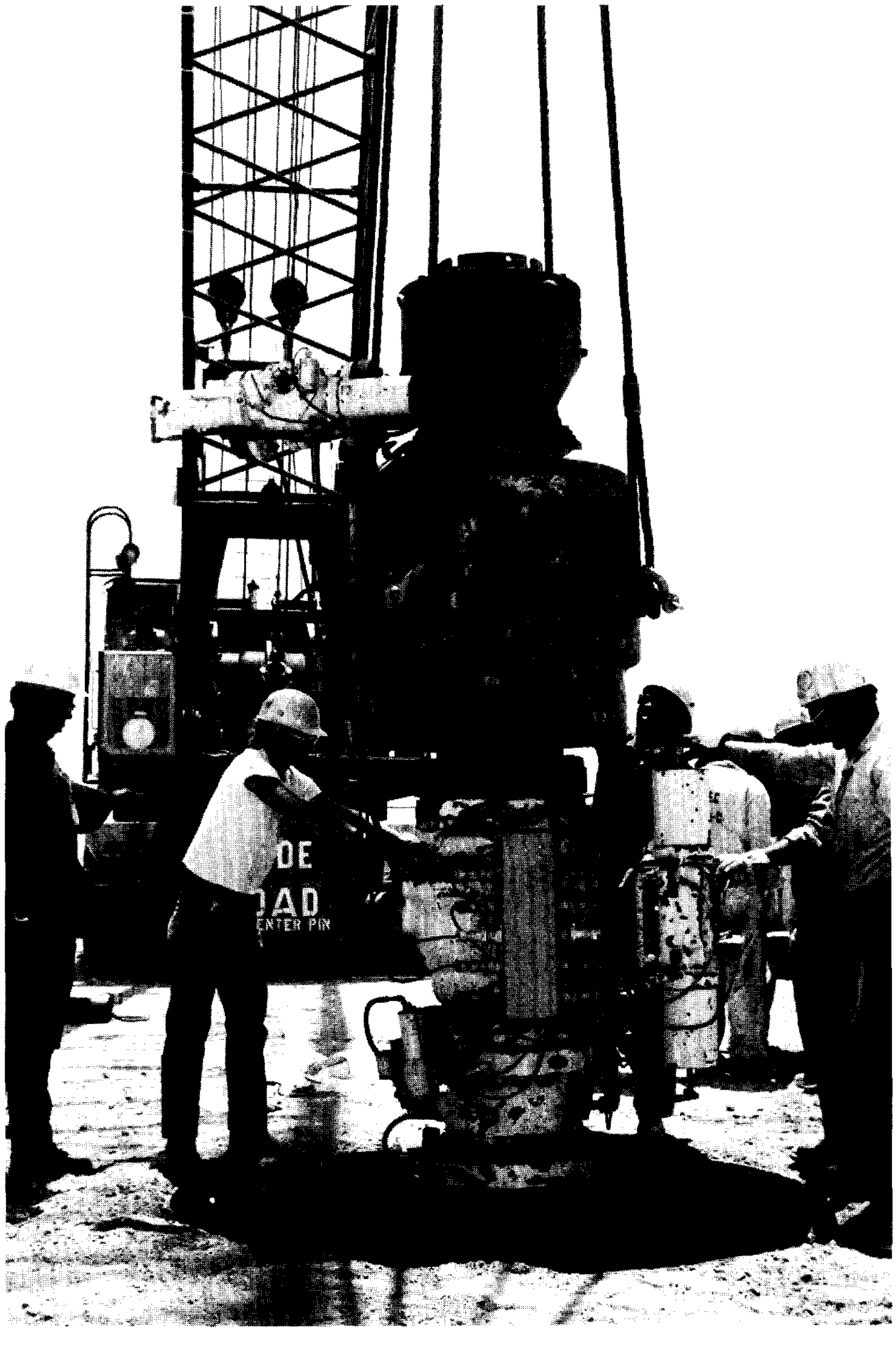
As additional experience was gained, the question “How can we reduce fallout?” became increasingly important for all tests. The first nuclear test at Trinity was conducted near the earth’s surface. but then to reduce fallout we went to taller towers, then air drops. balloons, and tunnels. and now to completely contained underground explosions,

Our first experiments in underground testing were done in 1957, initially using only high explosives. The first underground nuclear test. Pascal A, was in a three-foot-diameter hole at a depth of 485 feet. In lieu of completely filling the hole, a combination plug-collimator was placed near the bottom of the hole. Fired at 1:00 a.m., Pascal A ushered in the era of underground testing with a magnificent pyrotechnic Roman candle! Nonetheless. the radioactive debris released to the atmosphere was a factor of 10 less than what would have resulted had the test been conducted in the atmosphere. Theoretical models were constructed concerning possible containment schemes and 20 underground nuclear tests had been conducted before the intervention of the test moratorium. Theoretical work continued during the moratorium (1958- 1961) and when testing resumed, additional containment experience was obtained from a number of underground tests. By 1963, containment was sufficiently well understood to permit the U. S. to sign the LTBT with confidence that required tests could be contained underground—including those with extended lines of sight. The language of the treaty text prohibits detectable radiation levels beyond national borders.

The U. S.-assumed necessity to prevent even gases from escaping into the atmosphere at test time spawned entirely new disciplines in containment, and prompted the development of a number of special technologies to help achieve complete containment. With the exception of a few releases (none since 1970), the containment record of U. S. nuclear testing has been excellent since the LTBT was initiated in October 1963. No off-site radiation exposures exceeding national guidelines have been experienced.

There were some diagnostic cable related seeps and some sizable leaks associated mainly with LOS pipes. There were also a few prompt ventings; however, in no instance did off-site radiation levels violate guidelines. Only the close-in areas were evacuated for test execution. Containment effort was largely on an ad hoc basis and had little effect on operations.

After the Baneberry event of December 18, 1970, in which a large prompt venting produced off-site radioactivity. but not exceeding guidelines, the admonition became "not one atom out!" A more formal containment program was initiated, and the subsequent containment has been virtually perfect. Containment Evaluation Panel (CEP) procedures are more rigorous and formal. The Los Alamos containment program is extensive and involves about 35 employees in the Laboratory, plus NTS support. There are detailed geologic site investigations. Devices are buried deeper. Gas-blocked cables and impervious stemming plugs are used. All



Postshot drilling blowout preventer, a device used to preclude the escape of radioactive products into the atmosphere during postshot operations. This is a direct adaptation from oil field technology.

Aerial view of the formation of a postshot subsidence crater at the moment of collapse. This collapse may occur from a few minutes to many hours after a shot fired. Note the dust caused by falling earth.

operations are more conservative, and anything new or different that has any conceivable effect on containment must be well understood and justified. Longer lead times are required for geologic studies, document preparation, and the DOE approval process. Emplacement and stemming time and expense have increased. All of the NTS north of the Control Point is evacuated for every event. Although these steps have resulted in added expense and operational complications, they have provided increased confidence in complete containment of radioactive debris and the overall safety of test operations.

Conclusion

Nuclear testing has always been and will continue to be a vital element in the Los Alamos weapons program. Only with full-scale tests can the validity of complex design calculations be confirmed and refined. In a similar manner, only in the nuclear crucible of weapons tests can the physical behavior of weapons materials and components be investigated. Without testing, it would be difficult if not impossible to maintain a complement of knowledgeable weapon designers and engineers. Possible stockpile degradation could go undetected. Innovative solutions to national security problems would remain only paper designs, without proof of their validity in nuclear tests. As long as the United States national security is dependent upon nuclear deterrence, the weapons program will need nuclear tests to maintain its credibility. The Los Alamos history of successful and safe nuclear testing over the past 40 years is strong evidence that the program can remain a vital element of the national nuclear weapons program without detriment to the citizens of the United States or the world.

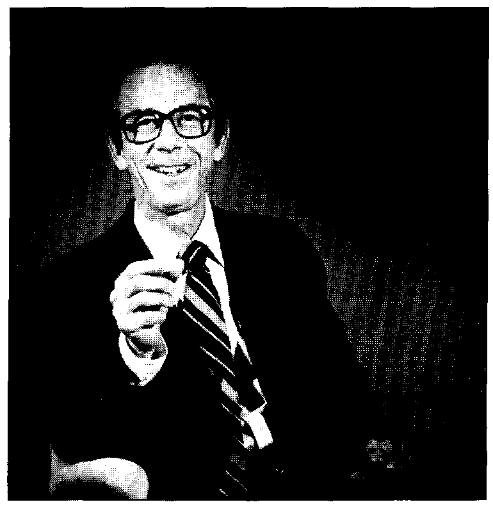
For any participant in the testing program, indelible impressions remain. Among those are the unique elements of romanticism and camaraderie associated with "where it was at'" and the excitement of successfully meeting difficult objectives and schedules. Another is the strong and consistent military-civilian partnership that grew throughout the 1950s to become an integral part of the testing philosophy and operation. Not the least of them, however, is the sense of purpose and accomplishment that comes from the conviction that we are doing something good for our country. ■

The Weapons Program

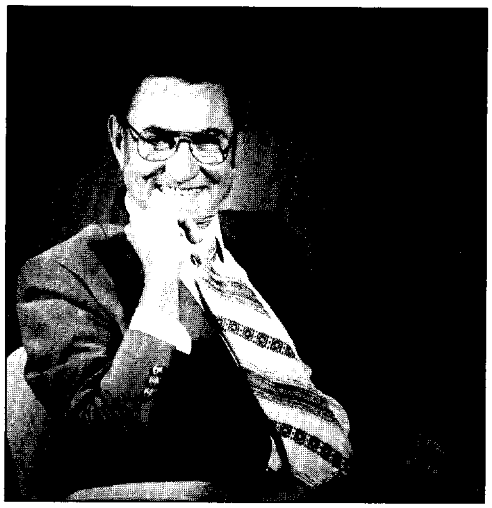
AUTHORS

Richard D. Baker *(Plutonium)* received a B.S. in chemical engineering from South Dakota School of Mines in 1936 and a Ph.D. in physical chemistry from Iowa State University in 1941. He came to Los Alamos from Chicago in 1943 to join the Chemistry and Metallurgy Division. His research and development on the preparation of plutonium and enriched uranium metal led to the patent for the production of plutonium metal on a multigram scale. Because of the importance and challenge of the materials research. he remained at Los Alamos after the war ended. He was a Group Leader *in* the Chemistry *and* Metallurgy Division from 1945 to 1956 and then became Leader of the newly formed Chemistry-Materials Science Division, which was involved in materials research and development for most of the Laboratory's programs. He became Associate Director for Weapons in 1979 and Associate Director for National Security Programs a few months later. He retired from the Laboratory in May 1981 but continues serving the Laboratory as a consultant.

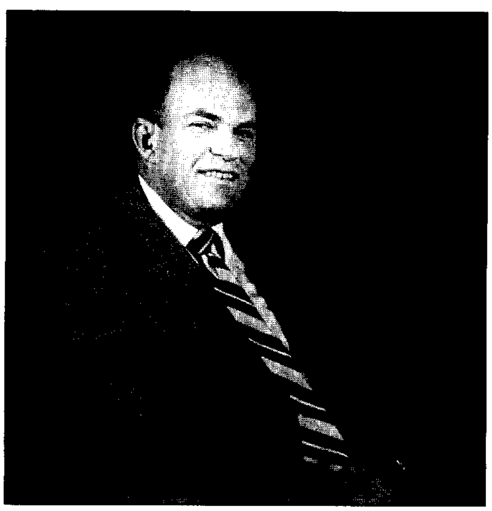
Merle E. Bunker *(Early Reactors)* participated in the Manhattan Project as a chemical technician at Decatur. Illinois. where the diffusion-barrier tubes for the Oak Ridge gaseous diffusion plant were produced. He received his scientific training at Purdue University (B.S. in mechanical engineering. 1946) and Indiana University (Ph.D. in nuclear physics, 1950). He joined the Los Alamos staff in 1950. attracted here by the high reputation of the Laboratory, which he learned about from friends already working at Los Alamos. and by a strong desire to live in the West. immediately after his arrival he participated in the design and construction of the SUPO version of the Water Boiler. He served as Operations Supervisor of SUPO for many years and oversaw its deactivitation in 1974. In parallel with his reactor work. he has conducted research on nuclear structure and nuclear transition rates, resulting in over 50 publications. He is currently Leader of the Research Reactor Group, which operates the OWR.



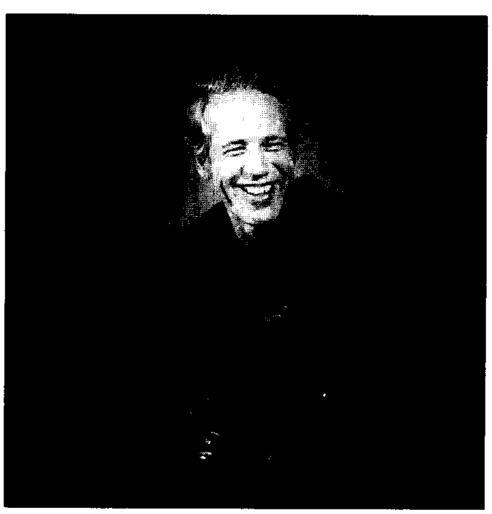
Bob Campbell *(Field Testing)* received a B.S. from Purdue University *in* August 1942. Until July 1947 he worked on field development of underwater ordnance, especially mines. with the Naval Ordnance Laboratory both as a civilian and. from January 1945 to July 1946. as a commissioned officer. He then joined the Laboratory, working first on explosive-driven jets and later on radiochemical samplers for obtaining specimens of bomb debris directly from the fireball. From October 1951 to August 1957, he was Leader of the Test Site Engineering Liaison Group, which saw to it that structures for test equipment and test devices were built according to the needs of the scientific groups. For the next two years he was Test Director for the Rover testing activities in Nevada. He was Assistant and Associate Leader of the Weapons Testing Division from August 1959 to August 1979 and served almost continuously from 1961 to 1982 at various test sites. He is now retired.



Ben C. Diven *(Nuclear Data* and *Field Testing).* As a graduate student in physics at Berkeley, I contributed to the first year of the war as lecture-room assistant in accelerated physics courses for military personnel. By the end of 1942 I decided I should be in the service like most of my friends and intended to join the Navy. Oppenheimer summoned me to his office and said that if I insisted at least I should go where I could do some significant good and that he would arrange a commission in the Army, the proposed home of all Los Alamos staff. Project Y's militarization did not materialize, but Oppie suggested that I come along anyway. I did. I landed in Albuquerque after my first airplane ride on March 13, 1943, and in Santa Fe I joined John Williams, Hugh Bradner, and Joe Stevenson, the only other Laboratory staff on site. We commuted to the Hill every day to see what strange things were being built from the hurriedly drawn-up plans and reported to Oppenheimer each evening by phone from Santa Fe. Soon supplies and staff began to arrive, but the man who was to coordinate the two wasn't due for some time. Oppie promised me that if I would take on that job in a few months he would find *me* work that was interesting and very educational, He kept his promise, and in the summer of '43 I joined Rossi and Staub's group, which developed instrumentation and measured nuclear data. I switched later to the RaLa experiment on implosion systems and in early '45 to preparation for measuring the reaction history of the Trinity device, In January 1946 I returned to graduate study at the University of Illinois. After receiving my Ph.D. in 1950, I returned to Los Alamos and joined Dick Taschek's group. In 1958 I became Leader of a group in the Physics Division, and in 19771 retired. I still come into the Laboratory frequently as a consultant to the Physics Division and Test Operations offices,



Delbert R. Harbur *(Plutonium)* received his B.S. in metallurgical engineering from the Colorado School of Mines in 1961. He came to Los Alamos in 1963 after working for two years on the Polaris missile system at Lockheed Missiles and Space Co, in Sunnyvale, California. Having learned from his colleagues in the aerospace industry that the way to get ahead was to move from job to job every two years, he came to Los Alamos fully expecting to move on in a couple of years, Instead he found working with the complexities of plutonium—a metallurgist's dream—and the enchantments of northern New Mexico irresistible. He has worked on the development of plutonium alloys for both the weapons and reactor programs and helped in the technology transfer that is responsible for placing the Los Alamos-developed weapons alloys into production, He is now Leader of the Plutonium Metal Technology Group, which deals with various aspects of plutonium metallurgy for the weapons program.

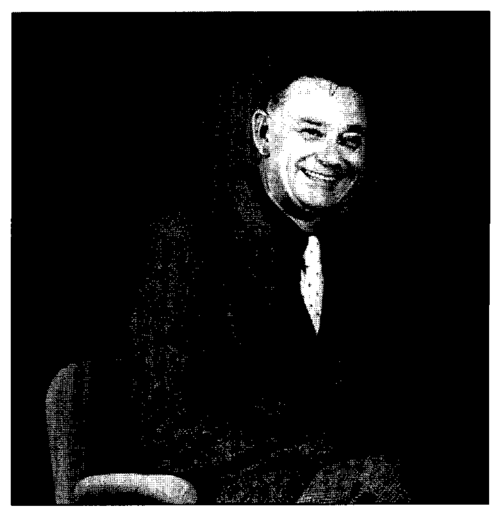


Francis H. Harlow *(Computing and Computers)* came to Los Alamos in September 1953 after receiving his Ph.D. from the University of Washington and has been a physicist in the Theoretical Division during his entire employment at the Laboratory. Special interests include fluid dynamics, heat transfer, and the numerical solution of continuum dynamics problems. He was Leader of the Fluid Dynamics Group for fourteen years and became a Laboratory Fellow in 1981. His extensive publications describe a variety of new techniques for solving fluid flow problems and discuss the basic physics and the application to practical problems. Northern New Mexico has served as a strong stimulus to his collateral activities in paleontology, archeology, and painting. Writings include one book on fossil brachiopods and four on the Pueblo Indian pottery of the early historic period. His paintings have been the subject of several one-man shows and are included in hundreds of collections throughout the United States.

AUTHORS

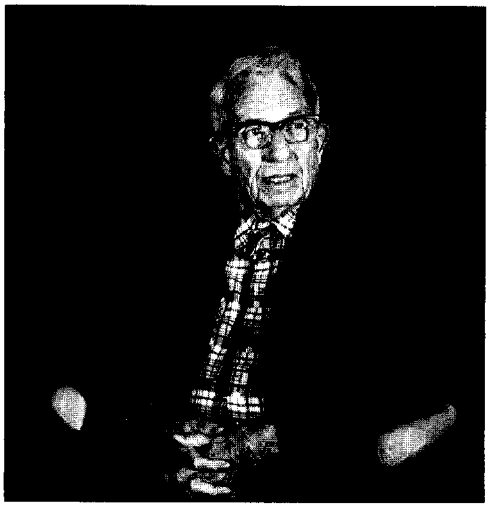
Siegfried S. Hecker *(Plutonium)* received his B. S., M. S., and Ph.D. degrees in metallurgy from Case Institute of Technology (now Case Western Reserve University) in Cleveland, Ohio. He first came to the Laboratory from Cleveland in 1965 as a summer graduate student. The main attraction was the mountains. which brought back memories of his childhood years in Austria. During his stay (which incidentally was also his honeymoon) he recognized the great potential for materials science at the Laboratory with its opportunities for basic research and applied technology. Also apparent were the opportunities for winter sports such as skiing, which was a way of life in Austria but difficult in Cleveland. Hence, he returned as a postdoc in 1968 to pursue basic research in metal deformation. That position was followed by three years at the General Motors Research Laboratories. In 1973 he returned again to Los Alamos to pursue basic and applied materials research. His main interests have been in plutonium metallurgy, mechanical behavior of materials, and materials for radioisotopic heat sources. In 1981 he helped to set up the Center for Materials Science at Los Alamos. He is currently Acting Chairman of the Center and Deputy Division Leader of the Materials Science and Technology Division.

Raymond E. Hunter *(Weapon Design)* was born September 4, 1935 in Moultrie, Georgia. He received B.S. and M.S. degrees in physics from the University of Georgia and a Ph.D. in elementary particle physics from Florida State University. He attained the rank of Captain in the United States Air Force with active duty at the Air Force Cambridge Research Laboratories. In 1961 he received the Air Force Research and Development award. Hunter served as head of the Department of Physics and Astronomy and as Dean of the Graduate School at Valdosta State College. He joined the Laboratory in 1965 and is now Assistant Division Leader for Weapons in the X Division Office. In 1981 he received a Distinguished Performance Award from the Laboratory for design of the W76 (Trident) warhead.

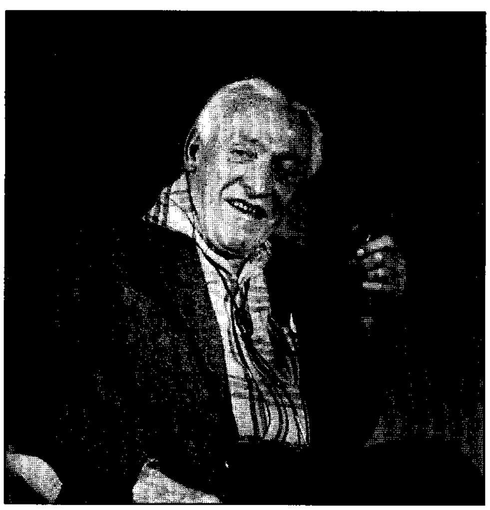


John W. McDonald *(Field Testing)* joined the Laboratory's Weapons Design Group in 1952 as a technical writer/editor to prepare instruction manuals for nuclear weapons and their components, manufacturing procedures. and research reports. He later served as Laboratory representative on the Joint Special Weapons Publications Board and was on loan to the Atomic Energy Commission as Technical Assistant in the Papers Branch of the U.S. Office of Participation in the 1958 United Nations Conference on the Peaceful Uses of Atomic Energy in Geneva, Switzerland. He left Los Alamos in 1960 to join the National Science Foundation's Office of Science Information Service. In 1962 and 1963 he was on loan from NSF to the Department of State and was assigned to the U. S. Mission to the United Nations in Geneva, Switzerland, as technical liaison officer for the 1963 United Nations Conference on the Application of Science and Technology for the Benefit of the Less-Developed Areas. Later, as a member of the North Carolina State University Mission to Peru, McDonald served from 1963 to 1966 as technical information advisor to the Peruvian Ministry of Agriculture. Returning to the Laboratory in 1966, McDonald joined Group D-6 (now IS-6) as leader of the technical editorial section and in 1970 joined the Group's classification staff, where he served as Deputy Group Leader and Alternate Classification Officer until joining National Security Programs in 1981. McDonald holds a degree in physics and mathematics from Utah State University and has done graduate work at the University' of Utah and the University of New Mexico. His experience also includes eight years on newspapers as copy editor and reporter.

AUTHORS



John H. Manley *(Nuclear Data).* I received my B.S. in engineering physics from the University of Illinois in 1929 and a Ph.D. in physics from the University of Michigan in 1934. After teaching physics at several universities, I was persuaded by Leo Szilard to join Arthur Compton's Metallurgical Laboratory at the University of Chicago. Shortly thereafter, in May 1942, Compton asked me to assist Robert Oppenheimer in the experimental part of weapons physics going on at several universities but administered from Chicago. In September Compton, Oppenheimer, and I urged General L. R. Groves to create a new laboratory where all weapons work could be concentrated. That laboratory came into being as Project Y at Los Alamos. I arrived there in April 1945 after working with a contractor on design of laboratory buildings, recruiting personnel, and arranging for four accelerators to be "borrowed" and shipped to the new Laboratory, located, as the recruiting brochure read, "on the shore of a small lake" better known today as Ashley Pond. After investigating the neutron properties of various tamper materials, my group assumed responsibility for blast and earth-shock measurements at the Trinity test. Before retiring in 1972. my Los Alamos duties included the positions of Physics Division Leader and Associate Technical Director. I now serve as a consultant. I served the Atomic Energy Commission for a summer as Deputy Director, Division of Research, as a Senior Responsible Reviewer for declassification, and for four years as Executive Secretary of its General Advisory Committee, chaired by Oppenheimer. In 1958 I was loaned to the State Department as the first Technical Advisor of the U.S. Mission to the International Atomic Energy Agency in Vienna.

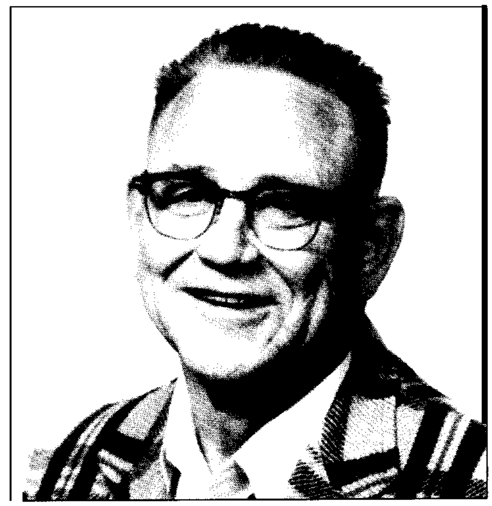


Carson Mark (*Weapon Design),* a native of Ontario, Canada, pursued his undergraduate studies in mathematics at the University of Toronto and the University of Western Ontario. His graduate work, also in mathematics, was carried out at the University of Toronto. He taught at the University of Manitoba from 1938 to 1943 and from 1943 to 1945 worked at the Montreal laboratory of the Canadian National Research Council. In May 1945 he and George Placzek came to Los Alamos as part of the contingent of United Kingdom scientists collaborating on the Manhattan Project, He joined the Laboratory staff in 1946 and was Leader of the Theoretical Division from 1947 until his retirement in 1973. Currently he is a Laboratory consultant and serves on the Nuclear Regulatory Commission's Advisory Committee on Reactor Safeguards.

N. Metropolis *(Computing and Computers)* received his B.S. (1937) and his Ph.D. (1941) in physics at the University of Chicago. He arrived in Los Alamos, April 1943, as a member of the original staff of fifty scientists. After the war he returned to the faculty of the University of Chicago as Assistant Professor. He came back to Los Alamos in 1948 to form the group that designed and built MANIAC I and II. (He chose the name MANIAC in the hope of stopping the rash of such acronyms for machine names, but may have, instead, only further stimulated such use,) From 1957 to 1965 he was Professor of Physics at the University of Chicago and was the founding Director of its Institute for Computer Research. In 1965 he returned to Los Alamos where he was made a Laboratory Senior Fellow in 1980.

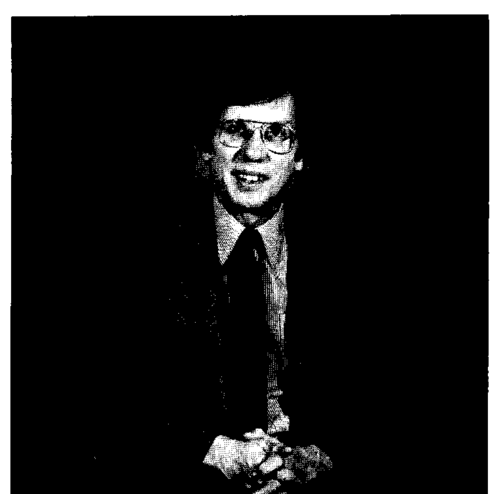
AUTHORS

William E. Ogle *(Field Testing)* participated in almost every field test of this country's nuclear weapons from Trinity through the tests at the Pacific Proving Ground, in Nevada, and on Amchitka Island in the Aleutians. He participated both as an experimenter—measuring neutron outputs, photoneutron thresholds, electromagnetic pulses, and magnetic fields-and as an administrator up through deputy task force commander. In 1945, with a Ph.D. in physics from the University of Illinois, he began work at the Laboratory on implosion dynamics and neutron outputs. From 1950 to 1955 he helped invent a number of weapons diagnostics, including a neutron "pinhole camera" to image the thermonuclear burn region. He was a delegate to the Nuclear Test Ban negotiations in Geneva in 1959, served on the Greenland auroral measurements expedition in 1959, was commander of several eclipse expeditions, and was the subject of a *Time* cover story in 1962. He was Test Division Leader from 1965 until 1972, when he left the Laboratory to form his own consulting firm. Since 1977 he has been president and chairman of the board of Energy Systems, Inc., headquartered at Anchorage, Alaska and contractor to the Department of Energy and the Defense Nuclear Agency. He currently serves as chairman of the Nevada Test Site Planning Board, the Test Concept Working Group. and the Test Net Assessment Panel. In addition. he is a consultant to the Los Alamos and Lawrence Livermore National Laboratories as well as to the Central Intelligence Agency and is writing a definitive history of nuclear weapons testing.



Hugh C. Paxton *(Criticality).* World War II led me away from early experience in nuclear physics consisting of a Ph.D. under E. O. Lawrence at Berkeley (1937) followed by cyclotron technology at the College of France and Columbia University. In 1948 it seemed time to return closer to nuclear physics, and Jerry Kellogg. knowing I had other ideas in mind, said, "Don't be a fool. Come to look at Los Alamos." The spectacular southwestern setting hooked me, and Jean and I remain hooked. Here I was established as leader of the critical assemblies group, a position I held until 1975 when the 10-year limitation caught up with me. The next year there was another limit, the mandatory retirement age. As Group Leader I was stimulated by the challenge of clearing the way for accomplished group members to work effectively. Until the end I felt I was where I belonged.

C. Paul Robinson *(Overview).* I received a B.S. in physics from Christian Brothers College in 1963 and a Ph.D. in experimental nuclear physics from Florida State University in 1967. In joining the Laboratory right out of school, I was convinced that Los Alamos was this country's top applied science laboratory and was "the place to be." My expectations were spectacularly confirmed with experimental work in Nevada on the Rover reactor tests. My job as Chief Test Operator there was superb training for all that followed. In 1970 we moved to Los Alamos with the formation of a group to examine new directions for the divisions involved in the Rover program. This work led to the Laser Fusion Program and the formation of the Laser Division. I participated in the development of both a variety of lasers and the first ideas about laser-induced chemistry. During 1973 we created the Laser Isotope Separation Program, and in 1976 I became leader of the new Applied Photochemistry Division. This was an exciting period of interplay among physicists, chemists, and engineers doing state-of-the-art research in fields ranging from laser photochemistry and high-resolution spectroscopy to laser system development and engineering. 1 am quite proud of the teams of scientists we put together there. as well as the large body of excellent research that resulted. In 1980 Don Kerr asked me to become Associate Director for National Security Programs, The nuclear weapons programs and other defense work at Los Alamos still represent some of the most important technical efforts for our nation's future. One of my chief goals has been to re-emphasize technical leadership of the programs. I am trying to promote activity in wider areas of defense science and technology for the future.



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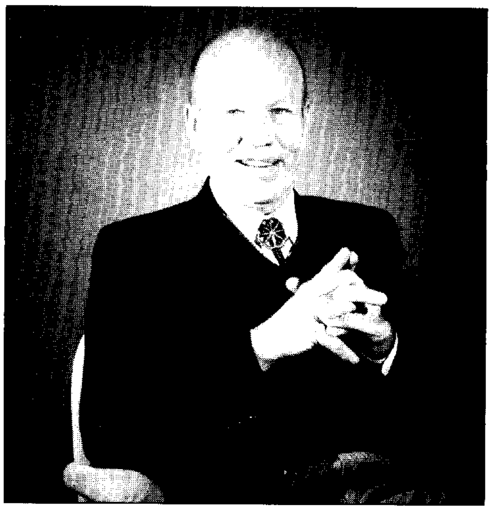
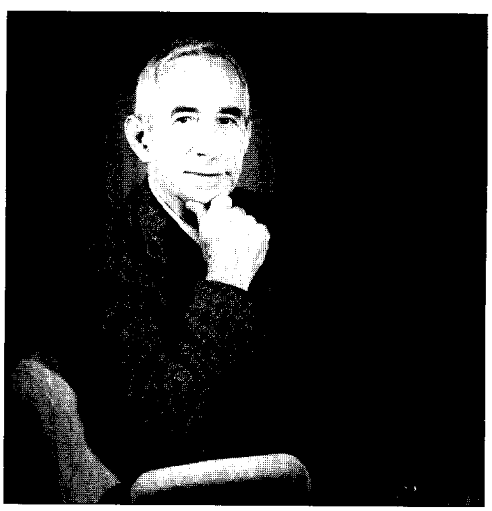
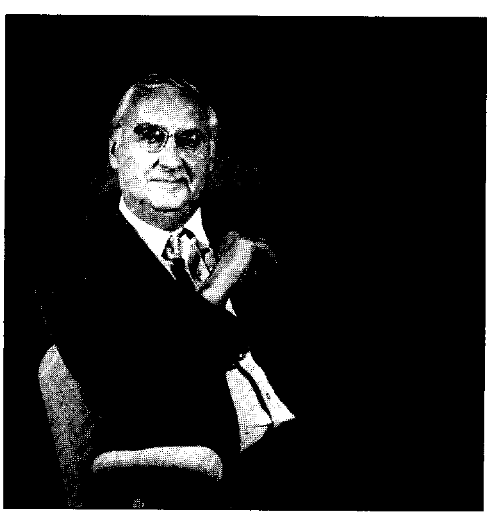


Tom Scolman *(Field Testing)* received his Ph.D. in nuclear physics from the University of Minnesota in 1955. Following the advice of professors and friends acquainted with Los Alamos, he joined the Laboratory's staff in 1956. After working for some years on various aspects of weapons engineering design and production, he became involved in weapons testing, an involvement that has continued to the present. He has directed a record number of tests—over l00-and has participated in all of the underground tests. He is currently the Deputy Associate Director responsible for managing the National Security Programs Test Operations Office.



Richard F. Taschek *(Nuclear Data).* I was born June 5, 1915 in Chicago, Illinois and grew up in Darlington, Wisconsin (population 1500). I received a B.A. in physics and mathematics in 1936 from the University of Wisconsin and an M.S. in physics from the University of Florida. I then returned to the University of Wisconsin in 1938 to do research under Gregory Breit on proton-proton and proton-deuteron scattering at low energies. After receiving my Ph.D. in June 1941, I worked for an electrochemical company until the fall of 1942; then I went to Princeton to work on an isotron project for Bob Wilson and Henry DeWolf Smyth. All project personnel and equipment were transferred to Los Alamos early in 1943; my wife. Inez, our six-week-old daughter, Katrine, and I arrived there in May. I was assigned to the electrostatic accelerator group to measure various fast-neutron cross sections. At the war's end I remained at the Laboratory because of its unprecedented research opportunities and because Los Alamos and New Mexico met all my desires as a place to live, play, and work. For some years I performed and guided research that contributed to the Laboratory's reputation during those years as one of the best research institutions in the world. In the years since about 1960, direct participation in research became quite difficult, even though it remained my first love, because of various administrative assignments, including Physics Division Leader and Associate Director for Research. Since my retirement in 1979, I have continued to serve the Laboratory as a consultant.



Jacob J. Wechsler *(Weapon Design)* arrived in Los Alamos early in 1944 as an enlisted man in the U.S. Army. He was first assigned to the Physics Division and later to the explosive studies groups of G and M divisions. Before enlisting he had attended Cornell University; he continued his engineering and physics studies at North Carolina State and Ohio State universities while in the service. He returned to Ohio State University in 1947 to teach and do graduate work. Wechsler rejoined the Laboratory in 1948 for the design and construction of the Van de Graaff accelerator and in 1951 returned to weapons engineering, specifically thermonuclear weapons. He participated in many bomb tests and was present for the Trinity, Mike, and early thermonuclear tests. He served the Laboratory in various positions of leadership, including Leader of the Design Engineering Division. Having retired from the Laboratory in February 1982, he now serves as a consultant.

The British Mission

*by Dennis C. Fakley**

News of the discovery in early 1939 of neutron-induced fission in uranium immediately prompted ideas in the United Kingdom and elsewhere not only of a controlled fission chain reaction but also of an uncontrolled, explosive chain reaction. Although official British circles viewed with a high degree of skepticism the possible significance of uranium fission for military application, some research was initiated at British universities on the theoretical aspects of achieving an explosive reaction. Progress was slow, the initial results were discouraging, and, following the outbreak of World War II, the effort was reduced and resources were moved to more pressing and more promising defence projects. The turning point came in March 1940 with the inspired memorandum by O. R. Frisch and R. E. Peierls, then both of Birmingham University, in which they predicted that a reasonably small mass of pure uranium-235 would support a fast chain reaction and outlined a method by which uranium-235 might be assembled in a weapon.

The importance of the Frisch-Peierls memorandum was recognised with surprising rapidity, and a uranium subcommittee of the Committee for the Scientific Survey of Air Warfare was set up. This subcommittee, soon to assume an independent existence as the MAUD Committee,** commissioned a series of theoretical and experimental research programmed at Liverpool, Birmingham, Cambridge, and Oxford universities and at Imperial Chemical Industries. By the end of 1940, nothing had disturbed the original prediction of Frisch and Peierls that a bomb was possible, the separation of uranium-235 had been shown to be industrially feasible, and a route for producing plutonium-239 as a potentially valuable bomb material had been identified.

The first official contact between American and British nuclear research following the outbreak of the war in Europe took place in the Fall of 1940 when Sir Henry Tizard, accompanied by Professor J. D. Cockcroft, led a mission to Washington. The MAUD Committee programme was described and was found to parallel the United States programme, although the latter was being conducted with somewhat less urgency. It was agreed that cooperation between the two countries would be mutually advantageous, and the necessary machinery was established. Even at this early stage the British increasingly recognised that, with their limited resources, they would have to look to the immense production capacity of America for the expensive development work; before long the MAUD Committee was discussing the possibility of shifting the main development work to America.

By the Spring of 1941, the MAUD Committee itself was convinced that a bomb was feasible, that the quantity of uranium-235 required was small, and that a practical method of producing uranium enriched in uranium-235 could be developed. It had also decided that there were no fundamental obstacles in the way of designing a uranium bomb. However, the possibility of a plutonium bomb had been pushed into the background partly because of doubts about feasibility and partly because large resources appeared to be needed for the development of a plutonium production route. The British were unaware of the work on plutonium already carried out by Professor E. O. Lawrence at Berkeley.

The MAUD Committee produced two reports on its work at the end of July 1941. These reports, "Use of Uranium for a Bomb" and "Use of Uranium as a Source of Power," were formally processed through the Ministry of Aircraft Production, the high-level Scientific Advisory Committee, and the Chiefs of Staff to Prime Minister Churchill, but, as a result of a great deal of unofficial lobbying, Churchill had made the decision that the bomb project should proceed before the official recommendations reached him. It was recognised that the project had to be set up on a more formal basis, and the Directorate of Tube Alloys—a title chosen as a cover name—was formed within the Department of Scientific and Industrial Research under the technical leadership of W. A. Akers, recruited from Imperial Chemical Industries, and the policy guidance of Sir John Anderson, Lord President of the Council.

Meanwhile, in the United States Dr. Vannevar Bush, head of the National Defense Research Committee, had asked the president of the National Academy of Sciences in April 1941 to appoint a committee of physicists to review the uranium problem. This committee, which was given copies of the MAUD reports, reached conclusions in November 1941 which were remarkably similar to those of the MAUD Committee, but it was less optimistic about the effectiveness of a uranium bomb, the time it would take to make one, and the costs. Surprisingly, despite the discoveries made at Berkeley, the committee did not refer to the possibility of a plutonium weapon. On the basis of the report of the National Academy of Sciences, President Roosevelt ordered an all-out development programme under the administration of the newly created Office of Scientific Research and Development and endorsed a complete exchange of information with Britain.

Assistant Chief Scientific Advisor (Nuclear), Ministry of Defence, London. The author is indebted to Professor Margaret Gowing, Official Historian of the United Kingdom Atomic Energy Authority, from whose book Britain and Atomic Energy 1939-1945 this outline history has been drawn and to Lord Penney who was kind enough to edit the text.

*****The story of the choice of title for this committee bears retelling. When Denmark was occupied by the Germans, Niels Bohr sent a telegram to Frisch, who had worked in Bohr's Copenhagen laboratory, asking him at the end of the message to "tell Cockcroft and Maud Ray Kent. "Maud Ray Kent was assumed to be a cryptic reference to radium or possibly uranium disintegration, and MAUD was chosen as a code name for the uranium committee. Only after the war was Maud Ray identified as a former governess to Bohr's children who was then living in the county of Kent.***

Although information exchange continued until the middle of 1942, the British were ambivalent about complete integration of the bomb project and expressed reservations which, with hindsight, make strange reading. By August 1942, when Sir John Anderson offered written proposals for cooperation beyond a mere information exchange, the American project had been transferred from the scientists to the U.S. Army under General L. R. Groves. Britain was probably no longer regarded by the Americans as being able to make any useful contribution, and the question of integration was deferred. Further, the imposition of a rigid security system by the U.S. Army led to such severe restrictions on the information exchange that the only real traffic related to the gaseous diffusion process for producing enriched uranium and to the use of heavy water as a reactor moderator.

The change in the United States' attitude toward cooperating with Britain came as a great shock to the British. Prime Minister Churchill took up the issue with President Roosevelt in early 1943 without any early sensible effect. Meanwhile, the British studied the implications of a wholly independent programme and reached what would now appear to be the self-evident conclusion that such a programme could not lead to results which could influence the outcome of the war in Europe.

A breath of fresh air blew over the scene when Bush, now director of the Office of Scientific Research and Development, and U.S. Secretary of War Henry Stimson visited London in July 1943. At a meeting with Churchill, a number of misunderstandings on both sides were satisfactorily resolved, and it was agreed that the British should draft an agreement defining the terms for future collaboration on the bomb project. The draft agreement included a statement of the necessity for the bomb project to be a completely joint effort, a pledge that neither country would use the bomb against the other, a further pledge that neither country would use the bomb against or disclose it to a third party without mutual consent, and recognition of the United States' right to limit whatever postwar commercial advantages of the project might accrue to Great Britain. A mission to Washington by Anderson reached agreement on provisions for establishment of a General Policy Committee and for renewal of information exchange. These provisions together with the points in the draft agreement were incorporated in the Quebec Agreement, which was signed by Roosevelt and Churchill on 19 August 1943.

There were still some minor hurdles to be surmounted before the Quebec Agreement could be implemented in detail, but they were overcome more rapidly than might have been expected by anyone who had experienced the difficult days in the first half of 1943. The increased cordiality of Anglo-American relations was due almost entirely to personal relations built up at the working level. Of pre-eminent importance was the rapport established between General Groves and Professor James Chadwick, senior technical adviser to the British members of the Combined Policy Committee.

With the resumption of cooperation, the first task was an updating one. The British handed over a pile of reports on the progress of their work, and General Groves supplied a copy of the progress report he had just submitted to the President. The British were amazed by the progress made in America and staggered by the scale of the American effort: the estimate of the total project cost was already in excess of one thousand million dollars compared with the British expenditure in 1943 of only about half a million pounds. Chadwick was in no doubt that the first duty of the British was to assist the Americans with their project and abandon all ideas of a wartime project in England. He concluded that this would best be achieved by sending British scientists to work in the United States. Before the end of 1943, Chadwick, Peierls, and M. L. E. Oliphant had taken up indefinite residence in America. Chadwick was occupied mostly in Washington with diplomatic and administrative functions but spent some time in Los Alamos; Peierls worked initially on gaseous diffusion but later at Los Alamos; and Oliphant, with three colleagues, worked at Berkeley with Lawrence's electromagnetic team; a further two scientists were attached to Los Alamos.

The exodus of British scientists to America accelerated in the early months of 1944. However, those who joined the gaseous diffusion programme did not stay long, and all were withdrawn by the Fall of 1944. The British team which joined Lawrence at Berkeley built up rapidly to about 35 and was completely integrated into the American group; most stayed until the end of the war. The British team assembled at Los Alamos finally numbered 19,* and, as at Berkeley, the scientists were assigned to existing groups in the Laboratory (although not to those groups concerned with the preparation of plutonium and its chemistry and metallurgy).

The first British scientists to go to Los Alamos were mainly nuclear physicists. They included Frisch, who led the Anglo-American group that first demonstrated the critical mass of uranium-235, and E. Bretscher, who found a niche in the group already thinking about fusion weapons. As the team built up, most of the British scientists were allocated to work on implosion weapon problems and to bomb assembly in general. Implosion was considered before the British arrived at Los Alamos, but Dr. J. L. Tuck made a significant contribution with his suggestion of explosive lenses for the achievement of highly symmetrical implosions. During 1944 Dr. W. G. (now Lord) Penney was recruited to assist with the

E. Bretscher, B. Davison, A. P. French, O. R. Frisch, K. Fuchs, J. Hughes, D. J. Littler, W. G. Marley, D. G. Marshall, P. B. Moon, R. E. Peierls, W. G. Penney, G. Placzek, M. J. Poole, J. Rotblat, H. Sheard, T. H. R. Skyrmes, E. W. Titterton, J. L. Tuck.

Members of the British Mission entertained their guests at the celebratory party with a skit depicting the adventures that befell Good Uncle Winnie's "Babes in the Woods" during their attempt with Good Uncle Franklin's forces to outwit Bad Uncles Adolph and Benito. At left a devilish security officer (J. L. Tuck) harasses an unhappy scientist (P. B. Moon), who wears his footprint as identification. In the skit's finale (below) a makeshift tower supports a gadget that was detonated with remarkable sound and light effects. Identifiable are (left to right) Winifred Moon (in pigtails), O. R. Frisch (costumed as an Indian maid), that security officer again, P. B. Moon (behind the ladder), E. W. Titterton (in background), W. G. Marley, and G. Placzek.

explosive side of the programme, as were Dr. W. G. Marley and two assistants. Eventually six British scientists (Bretscher. Frisch, P. B. Moon, Peierls, Penney, and G. Placzek) became the heads of joint groups and a seventh, Marley, became head of a section.

Further, two highly distinguished consultants were made available under British auspices, namely Professor Niels Bohr and Sir Geoffrey Taylor. Bohr's visits to Los Alamos were inspirational; Taylor was able to contribute significantly to the work on hydrodynamics.

There is no objective way of measuring the contribution made by the British to the Manhattan Project at Los Alamos and elsewhere. General Groves often acknowledged the importance of the early work in the United Kingdom and the substantial contribution made by her scientists in America, but he added that the United States could have got along without them. The British presence, though small, certainly had a beneficial effect on the morale of the Project. It served a function not otherwise available in that closed community—as a centre of second opinions by scientists whose reputations were generally admired.

Whatever the variations in the opinions of the British contributions to the Manhattan Project, there is no dispute that their participation benefited the British considerably. The course of the British nuclear programme in the postwar period would have been very different had it not been for the wartime collaboration. While United States law prohibited international cooperation on nuclear weapon design, the British were able to undertake a successful independent nuclear weapons programme, which, despite its small scale relative to that of the American programme, succeeded in elucidating all the essential principles of both fission and thermonuclear warheads and in producing an operational nuclear weapons capability. When the two countries came together again in 1958, following a critical

THE BRITISH MISSION

INVITES YOU TO A PARTY IN CELEBRATION OF

THE BIRTH OF THE ATOMIC ERA

FULLER LODGE

SATURDAY, 22ND SEPTEMBER, 1945

DANCING, ENTERTAINMENT,
PRECEDED BY SUPPER AT 8 P.M.

Mr & Mrs C. Critchfield

R.S.V.P TO MRS. W. F. MOON
Room A-211 (EXTENSION 250)

The social triumph of the collaboration between the British and the Americans on the Manhattan Project was a celebratory party hosted by the British Mission. All aspects of the celebration had a properly British flavor: formal invitations, a "footman" to announce the arrival of the guests, an entree of steak-and-kidney pie, a dessert of trifle, and the best port for ceremonial toasts to the King, the President, and the Grand Alliance.

amendment to the 1954 United States Atomic Energy Act, the developments in nuclear weapons technology over the previous eleven years were found to be remarkably similar.

It is also of interest to note the similarities between the wartime cooperation on the development of the first nuclear weapon and the cooperation which has ensued over the past 25 years under the 1958 U.S.-U.K. Agreement for Mutual Cooperation on the Uses of Atomic Energy for Mutual Defense Purposes (as Amended). It is possible to identify very many of the same strengths and weaknesses that were evident in the 1940s. Those who have been intimately connected with the collaboration on nuclear defence subscribe to the view that it works in the overall joint defence interests of the two countries. ■

The Laboratory's opinion as to the value of the British contributions to Project Y is well illustrated by the following excerpt from a letter, dated July 18, 1949, to Carroll L. Wilson, General Manager of the United States Atomic Energy Commission:

> **For the work of the Theoretical Division of the Los Alamos Project during the war the collaboration of the British Mission was absolutely essential . . . It is very difficult to say what would have happened under different conditions. However, at least, the work of the Theoretical Division would have been very much more difficult and very much less effective without the members of the British Mission, and it is not unlikely that our final weapon would have been considerably less efficient in this case.**
>
> **Hans A. Bethe**

Seven Hours of Reminiscences

by Edward Teller

When Shakespeare presented the life story of Henry VI, he wrote it in three parts, and the plays in their entirety ran more than seven hours. The BBC drama compressed the life of J. Robert Oppenheimer into seven hours, a considerable accomplishment since to my mind Henry VI was not nearly as unique, ingenious or self-contradictory a character as Oppenheimer. Most of us probably imagine ourselves and our closest associates to be simpler than we are. However, the complexity of the man whom I knew and worked with makes the television representation seem almost one-dimensional.

The film merely indicates the important contrast of the two historical poles of Oppenheimer's life—his work at Los Alamos and his loss of security clearance. The inadequacy in describing his work is related to the uniqueness of Oppenheimer's accomplishment as wartime director of the Los Alamos Laboratory. Comparable roles outside the scientific community are rare. Much of my life has been spent in laboratories of similar size and nature. I have known many of the directors intimately. For a short time, I was even a director myself. I know of no one whose work begins to compare in excellence with that of Oppenheimer's.

Throughout the war years, Oppie knew in detail what was going on in every part of the Laboratory. He was incredibly quick and perceptive in analyzing human as well as technical problems. Of the more than ten thousand people who eventually came to work at Los Alamos, Oppie knew several hundred intimately, by which I mean that he knew what their relationships with one another were and what made them tick. He knew how to organize, cajole, humor, soothe feelings—how to lead powerfully without seeming to do so. He was an exemplar of dedication, a hero who never lost his humanness. Disappointing him somehow carried with it a sense of wrongdoing. Los Alamos' amazing success grew out of the brilliance, enthusiasm and charisma with which Oppenheimer led it.

A different perspective on Oppie started to appear in June 1945, a few weeks before the Alamogordo test of the first atomic bomb. I had received a letter from my good friend Leo Szilard* containing a petition from Chicago together with a request that I sign and circulate it among my colleagues at Los Alamos.

The Chicago laboratory, headed by Arthur Compton, had worked on devising the means of production of material for the bomb. Their work had been completed some months earlier, and Szilard, James Franck (nicknamed Pa Franck) and several scientists in the project had some time to consider the political and moral issues related to the bomb itself, a development I knew about since I had recently been in Chicago. The petition they drew up, addressed to the President, pointed out that scientists began work on the atomic bomb because we might have been attacked by this means, but that this danger had been averted. It noted that the ruthless annihilation of cities would be further increased if the bomb were used, as this would set a precedent and open "the door to an era of devastation on an unimaginable scale." The petition asked the President "to rule that the United States shall not, in the present phase of the war, resort to the use of the atomic bombs."

I was inclined to sign the Chicago petition, but I also could not circulate it at Los Alamos without checking the matter with Oppenheimer, first because he was the director but also because I had considerable respect for his opinion. I arranged to talk with him at his office. While the film suggests that other people accompanied me, only Oppie and I were present at this conversation. I began by showing him the petition.

Oppenheimer immediately offered several uncomplimentary comments about the attitudes of the involved Chicago scientists in general and Szilard in particular. He went on to say that scientists had no right to use their prestige to try to influence political decisions. He assured me that the right decisions would be made by the 1eaders in Washington who were wise people and understood the psychology of the Japanese. I have the vague impression that he referred to George Marshall as an example of such leadership. My predominant emotion following our conversation was that of relief—I did not have to take any action on a matter as difficult as deciding how the bomb should be employed.

Years later I learned that shortly before this interview Oppenheimer not only had used his scientific stature to give political advice in favor of immediate bombing, but also put his point of view forward so effectively that he gained the reluctant concurrence of his colleagues. Yet he denied Szilard, a scientist of lesser influence, all justification for expressing his opinion.

In the late spring of 1945, four scientists were asked to serve as an advisory panel on the use of the bomb: Arthur Compton from whose laboratory the petition originated, Ernest Lawrence from the isotope separation laboratory at Berkeley, Enrico Fermi (whose sense of political discretion was carried to the point of hardly ever expressing an opinion that differed from the majority),** and Oppenheimer from Los Alamos. Only Fermi and Oppenheimer were aware of the mechanics and expected effects of the bomb itself. Only Oppenheimer advocated immediate use of the bomb.

Secrecy was an unseen member in this group. The flow of information within laboratories, as well as between laboratories, was strictly controlled. Compton and Lawrence favored prior demonstration, but their information about the mechanics of the bomb, particularly those that would affect the possibility of a demonstration, was incomplete. Lawrence held out longest for prior demonstration, but on June 16, 1945 the panel presented a unanimous recommendation for use without prior warning.

I owed Szilard an answer, but I felt it inappropriate to mention my talk with Oppie as I did not feel that he had authorized me in any way to repeat his opinions. Correspondence at Los Alamos was censored, and I believed it highly likely that Oppie would see my letter. I therefore sent him a copy of my letter to Szilard with a handwritten note:

Dear Oppie,

You may have guessed that one of the men "near Pa Franck" whom I have seen in Chicago was Szilard. His moral objections to what we are doing are in my opinion honest. After what he told me I should feel better if I could explain to him my point of view. This I am doing in the enclosed letter. What I say is, I believe, in agreement with your views. At least in the main points. I hope you will find it correct to send my letter to Szilard.

Edward

I had several reasons for wanting to avoid any further controversy on this issue: as an immigrant, I was particularly aware of my political ignorance; I had not taken sufficient time to think through or discuss the future implications of use versus non-use; and I sincerely wanted to be on friendly terms with Oppie. I have long regretted the fact that I allowed myself to be so easily persuaded.

Immediately after the bomb was dropped on Hiroshima, the feeling of jubilation among many people in Los Alamos as well as Oppenheimer's dramatic quote from the Bhagavad-Gita, "I am the destroyer of the world," made me most uncomfortable. I eventually felt strongly that action without prior warning or demonstration was a mistake.

1 also came to the conclusion that al-

Szilard's Letter was dated July 4, 1945, while my reply, dated Ju/y 2, was written a number of days after I received his. In addittion, Szilard had not bothered to fill in my name in his form letter. The explanation is simply that Szilard was a man of many idiosyncracies.

*****Fermi had lived many years under Fascism, and I suspect this may account for his reticence.***

OTHER PERSPECTIVES

July 4, 1945

Dear

Inclosed is the text of a petition which will be submitted to the President of the United States. As you will see, this petition is based on purely. moral considerations.

It may very well be that the decision of the President whether or not to use atomic bombs in the war against Japan will largely be based on considerations of expediency. On the basis of expediency, many arguments could be put forward both for and against our use of atomic bombs against Japan. Such arguments could be considered only within the framework of a thorough analysis of the situation which will face the United States after this war and it was felt that no useful purpose would be served by considering arguments of expediency in a short petition.

However small the chance might be that our petition may influence the course of events, I personally feel that it would be a matter of importance if a large number of scientists who have worked in this field wont clearly and unmistakably on record as to their opposition on moral grounds to the use of these bombs in the present phase of the war.

Many of us are inclined to say that individual Germans share the guilt for the acts which Germany committed during this war because they did not raise their voices in protest against those acts, Their defense that their protest would have been of no avail hardly seems acceptable even though these Germans could not have protested without running risks to life and liberty. We are in a position to raise our voices without incurring any such risks even though we might incur the displeasure of some of those who are at present in charge of controlling the work on “atomic power.”

The fact that the people of the United States are unaware of the choice which faces us increases our responsibility in this matter since those who have worked on “atomic power” represent a sample of the population and they alone are in a position to form an opinion and declare their stand.

Anyone who might wish to go on record by signing the petition ought to have an opportunity to do so and, therefore, it would be appreciated if you could give every member of your group an opportunity for signing.

Leo Szilard

though the opinions of scientists on political matters should not be given special weight, neither should scientists stay out of public debates just because they are scientists. In fact, when political decisions involve scientific and technical matters, they have an obligation to speak out. I failed my first test in Los Alamos, but I have subsequently stood by this conviction.

It is a remarkable coincidence that with few exceptions (Leo Szilard is the most outstanding), those who favored a prior warning to Japan later argued for continued development of weapons, while those who recommended immediate use of the atomic bomb argued after the war for cessation of all further development. One scientist who withdrew from weapons work and became a tireless opponent of the development of the hydrogen bomb advocated during his Los Alamos years a plan under which the United States would not use any atomic bombs in Japan until the number collected was great enough to bomb several large centers on the same day, thus bringing the war to a sure, immediate end.

On the other hand, Lewis Strauss, a Washington-based Naval officer during the war, knew of the bomb and personally suggested to Secretary of the Navy James Forrestal that the bomb be demonstrated over a forest after warning the inhabitants to evacuate. * In his memoir he devotes a whole chapter to the last days of the war and calls it “A Thousand Years of Regret.” However, he became the strongest single supporter of a program to develop the hydrogen bomb.

The film correctly indicates the sharp contrast of Oppenheimer’s enthusiastic leadership of the Laboratory prior to the bombing and his distress following the bomb’s actual use. In early fall 1945, Oppenheimer passed me on the way to the laboratory. “Touch me,” he said. “I just resigned as director.” Quite a few of us knew that Oppenheimer was eager to return to the study of physics and that he was talking about “giving Los Alamos back to the Indians.” The future of the laboratory was very much in question.

A few weeks later the decision was made to continue the laboratory at Los Alamos. and when Norris Bradbury took over as the new director, he asked me to stay on as head of physics research. I explained that I would stay under one of two conditions: if we were to have a vigorous program for refining fission weapons which included at least twelve tests a year, or if we were to concentrate on the hydrogen bomb. In other words, I was fully willing to participate if our work could make a comprehensive contribution to the nation’s continued military strength.

Bradbury explained that he wished that he could promise to fulfill either set of conditions, but taking political realities into account, he could not do so. I thereupon answered that I would return to Chicago to work on physics with Fermi. (But even then I felt that I should be trying harder to participate.)

That same evening, Oppie and I were at a party at Deke Parsons’** house. Chatting with Oppie, I repeated my afternoon exchange with Bradbury almost verbatim. Op-

The Navy generally opposed the use of the bomb without warning, and Strauss, in every way a man who loved his country, was also too honest not to expose all the details of what he considered a tragic error. The Japanese peace overture instructions (identical to the terms of surrender achieved a few weeks later) to Prince Fumimaro Konoye, who was negotiating in Moscow, were decoded by the Navy. Strauss in his memoirs ignores no part of this confusing and for him extremely painful period.

*****Captain Parsons was the scientific representative from the Navy to the Los Alamos Laboratory, and the party celebrated his promotion to Commodore.***

July 2, 1945

Dr. Leo Szilard
P. O. Box 5207
Chicago 80, Illinois

Dear Szilard:

Since our discussion I have sprint some time thinking about your objections to an immediate military use of the weapon we may produce. I decided to do nothing; I should like to tell you my reasons.

First of all let me say that I have no hope of clearing my conscience. The things we are working on are so terrible that no amount of protesting or fiddling with politics will save our souls.

This much is true: I have not worked on the project for a very selfish reason and I have gotten mucsh more trouble than pleasure out of it. I worked because the problems interested me and I should have felt it a great restraint not to go ahead. I can not claim that I simply worked to do my duty. A sense of duty could keep me out of such work. It could not get me into the present kind of activity against my inclinations. If you should succeed in convincing me that your moral objections are valid, I should quit working. I hardly think that I should start protesting.

But I am not really convinced of your objections. I do not feel that there is any chance to outlaw any one weapon. If we have a slim chance of survival, it lies in the possibility to get rid of wars. The more decisive a weapon is the more surely it will be used in any real conflict and no agreements will help.

Our only hope is in getting the facts of our results before the people. This might help to convince everybody that the next war would be fatal. For this purpose actual combat use might even be the best thing.

And this brings me to the main point. The accident that we worked out this dreadful thing should not give us the responsibility of having a voice in how it is to be used. This responsibility must in the end be shifted to the people as a whole and that can be done only by making the facts known. This is the only cause for which I feel entitled in doing something: the necessity of lifting the secrecy at least as far as the broad issues of our work are concerned. My understanding is that this will be done as soon as the military situation permits it.

All this may seem to you quite wrong. I should be glad if you showed this letter to Eugene and to Franck who seem to agree with you rather than with me. I should like to have the advice of all of you whether you think it is a crime to continue to work. But I feel that I should do the wrong thing if I tried to say how to tie the little toe of the ghost to the bottle from which we just helped it to escape.

With best regards.

Yours,

E. Teller

OTHER PERSPECTIVES

pie said, "And don't you feel better now?" I said, "No." I also remember that on the same occasion Oppenheimer said: "Our accomplishments in Los Alamos have been remarkable, and it will be a long time before anyone can reproduce them." I felt less optimistic and could not agree with Oppie's attitude. To Bradbury belongs the great credit of having kept the Laboratory alive through difficult years.

On one point, I have always agreed with Oppenheimer in a most enthusiastic manner: the need for openness of information for the American people. Recently I secured copies of my correspondence with Oppenheimer from his archives.* I discovered that as early as March, 1943, I was already bending his sympathetic ear on the question.

Once a course of action is established, it becomes particularly hard to undo. During the wartime work on the atomic bomb, secrecy seemed imperative. Scientists, whose work is based on openness, urged that their findings not be released lest they fall in the hands of the Nazis and Hitler gain the atomic bomb. But having begun in such a way, how does one rid oneself of the cancer of secrecy? I believe that only a drastic measure can now remedy the situation, and I have repeatedly proposed that after the period of one year, all classified material (with a few exceptions such as the routes of our submarines and blueprints for equipment) should be released to the public. To continue classifying anything of a scientific nature for a longer period should require detailed Presidential orders, a practice which would surely limit the number of exceptions.

That the American people—who in a democracy should and do create our policy of defense—have a *need to know* seems to me to be a truth beyond any question. Yet this truth is contradicted by laws which forbid open discussion, laws which as citizens we are bound to obey. The issue never gained the stature in Oppenheimer's lifetime that it deserved. Today there can be no doubt of the crippling effects of secrecy.

Before leaving the war years, I want to correct a minor historical inaccuracy in the BBC production. Introducing Oppenheimer's opposition to the hydrogen bomb at the Berkeley summer conference in 1942 enables the producers to suggest future developments but results in a skewed perspective. The hydrogen bomb was the main topic at that conference, and unlike the television portrayal, there was no difference of opinion about the propriety of discussing the subject.

Oppenheimer, I was told, actually used this topic in a conversation with Arthur Compton to point out the surprises waiting in the nuclear field and the consequent necessity of establishing a separate laboratory at Los Alamos. One of the first pieces of equipment (for cryogenic work) built at Los

**** All my correspondence prior to 1952 was lost when I left Los Alamos, and Z have only recently begun piecing it bock together from other sources.***

Alamos was related to work on the hydrogen, rather than the atomic, bomb. It was only after we were all at Los Alamos that a strong difference of opinion arose on the advisability of working on the H-bomb *at that time.* The need to pursue this research in the long run was not called into question until after the end of World War II.

Because the United States held a clear monopoly on the atomic bomb in 1945, Oppenheimer began working on and for a plan which the television drama slights. With Lilienthal and Baruch, he drew up a proposal to place all information about control of atomic weapons in the hands of an international agency. Baruch presented the plan to the newly created United Nations. The Soviet delegation insisted that before any discussion of how to assure compliance with the plan could begin, the United States must destroy its nuclear weapons, Since the Soviets were clearly not willing to come to any reasonable agreement on inspection, the Baruch plan was ultimately dropped.

Today the failure is easy to understand. What we thought we were offering-the secrets of atomic explosives—the Soviets had already gained through their very efficient spy system.

In 1949, I returned to Los Alamos on a full-time basis. The political climate had not improved, and few people seemed to share my concern about the possible progress in development of nuclear weapons in the Soviet Union. However, I had decided to make whatever contribution I could to our own defense.

In September of that year, I was in England and visited with Sir James Chadwick, who had been the leader of the British delegation to Los Alamos. I made an unflattering comment about General Groves, and Chadwick, ordinarily a most reticent man, became effusive. According to him, I did not properly appreciate General Groves' dedication and efficiency. Without Groves, insisted Chadwick, the project would never have been successful. American scientists (but not the British and not American military leaders) had no sense of what it meant to have one's home and family truly endangered by a war. Their determination and dedication were apt to be too little and too late. He ended by insisting that I recall his advice: "I might have need of it."

A few hours after I was back in the United States, it dawned on me that during our conversation, Chadwick probably had known what I had just learned—that the Soviets had exploded an atomic bomb. (An interesting footnote to this event is the fact that without the detection system that was introduced shortly before at the insistence of Lewis Strauss, the United States might have remained in ignorance of the Soviet bomb.) It was then that I called Oppie and was advised, as the film described, that I should "keep my shirt on." This was not the first time since the war had ended that Oppie had made it clear that he was uninterested in using his great talents on defense research problems again.

The BBC production contains to my mind only one major historical flaw. This important point concerns the position of Lewis Strauss, who at the time of the Oppenheimer hearings was the Chairman of the Atomic Energy Commission.* Strauss appears in the film as one of Oppenheimer's main antagonists, but the facts contain a different tragic drama than was conveyed.

Clearly Strauss disagreed with Oppenheimer's belief that new weapons development should be curtailed, and Strauss would have been happy to have a Presidential advisor with a different perspective. However, his role in Oppenheimer's loss of security clearance was quite different than the BBC production suggests.

Early in December, 1953, I went to Strauss' office for a prearranged meeting on some laboratory-related matters. He had been unexpectedly called away so I waited. He returned in uncharacteristic agitation and led me immediately into his office. Pledging me to discuss the issue no further, he told me of the cause of his late arrival and distress.

I kept Strauss' confidence for many years, but any obligation for silence lapsed long ago. Strauss was appalled because President Eisenhower had called him to the White House and told him to institute official proceedings to review Oppenheimer's security clearance. Strauss told me with real fervor of his hope that the President's decision would be reversed or at least modified. He foresaw disastrous consequences should Oppenheimer's clearance be called into question.

My experience leaves me no room to doubt that Lewis Strauss, far from bringing about these proceedings, wanted to prevent them. Whether Strauss merely foresaw difficult times for the Atomic Energy Commission or whether he had an insight into the future effects on the scientific community, I have no way of knowing.

The film's mistaken sequence—where removal of classified material from Oppenheimer's home occurs before Oppenheimer knows that he had lost his right to retain classified material—and the portrayal of an imaginary meeting of Strauss and Nichols to plot against Oppenheimer create a particularly misleading picture of Strauss. In reality, Lewis Strauss was a sensitive man with a most demanding code of honor. He did not disturb Oppie during his European vacation but, as soon as Oppie returned, called him in to discuss the problem. Strauss explained that a high-ranking official had written the President accusing Oppenheimer of disloyalty, that Oppie had the choice of resigning or having a hearing, and that his clearance would be temporarily suspended either way.**

After the hearings Oppie remained for many fruitful years as director of The Institute for Advanced Study. Strauss was the chairman of the board of that institute and had earlier been instrumental in securing the directorship for Oppie.

*****The details of this meeting on December 21 are included in Strauss' memoirs, Men and Decisions (Doubleday, 1%2), pp. 275-9 and 443-5.***

OTHER PERSPECTIVES

Oppenheimer asked how long he had for his decision on resignation or hearing, and Strauss explained that because he had already delayed some weeks, he would appreciate the decision on the next day. The classified papers were picked up after this interview. Strauss had not specified that this would occur, but given Oppenheimer's years of experience with security practices, Strauss' omission had many more reasonable explanations than malice.

There is another detail in this section of the film which is in error. When I was called to testify at the hearing, I was, as is shown, met by the attorney for the Atomic Energy Commission, Roger Robb. However, Robb did not give me the FBI file on Oppenheimer. That I never saw. Instead, Robb asked me how I would testify—for or against Oppenheimer's clearance. I had no difficulty with my reply: I would testify for his clearance. Robb then said that he wanted to read a part of the hearing testimony to me. I was a little uncomfortable about this, but an earlier incident seemed to me to have a bearing on what was now appropriate.

Early in 1954, when the question of Oppenheimer's clearance had become public knowledge, I had met Oppie at a small scientific meeting. I expressed my regrets at the nature of his problem. He asked me whether I believed he had behaved in a "sinister" manner. I said that I certainly did not. He then asked me as a favor to go and talk with his lawyer. I agreed to do so and did. Oppenheimer was not present at the interview, and his lawyer told me no novel facts.

However, having been briefed by Oppenheimer's lawyer, I could find no grounds to refuse Robb. Robb then read Oppenheimer's sworn testimony concerning the Chevalier affair from the hearing transcript to me. As the film suggests, this issue proved to be the turning point of the hearing. Oppenheimer testified that he had voluntarily gone to Army security officers with a distorted story which in the end ruined a friend's life. He had told the intelligence officers that Chevalier had asked three scientists to provide information to the Soviets about the atomic bomb project. When asked why he had done so,* Oppie replied, "Because I was an idiot."

I will never forget the shock that this portion of the testimony produced in me. Robb asked me again, "Should Oppenheimer be cleared?" I could only tell him that I did not know.

My reluctant testimony, given minutes later, was that I definitely considered Oppenheimer loyal, but that because his actions appeared confused and complicated, I would personally feel more secure if public matters would rest in other hands. I was convinced then and continue to believe now that the hearing should never have occurred.

The historical importance of the Los Alamos years are comparatively easy to grasp because of their clearly visible consequence—the use of an incredibly powerful weapon and the end of a terrible war. However, the consequences of Oppenheimer's security clearance are difficult to discern outside the scientific community. They are hardly hinted at in the television drama. Oppenheimer's loss of security clearance partly introduced and partly solidified a deep division among the ranks of American scientists.

After the two events—the use of the atomic bomb and Oppenheimer's loss of clearance, the great majority of scientists felt that it was wrong to work on new weapons. A small minority of scientists, to which I belonged, believed it imperative to work on such weapons if the United States were to be able to defend itself and the free world. For this minority, the events of the past thirty-five years have demonstrated that while the danger from a ruthless adventurer named Hitler was more immediate, the danger from the patient, unrelenting leaders in the Kremlin is in reality greater,

Furthermore, scientists were discouraged from involving themselves with work which would place them under the vagaries of the security system. Many scientists have never forgiven the damage that was done to a great scientist's reputation. While the origin of the feeling of distrust may have vanished from memory, the residual effect in the scientific community remains. The Oppenheimer hearing was truly as tragic as Strauss feared and combined with the bombing in Japan have resulted in some people today crying, "A plague on both your houses." But distrust of our nation seems about as justified as evaluating one's own bad case of acne as equal in seriousness to a neighbor's case of bubonic plague.

There is one incident depicted in the film which is true in spirit but lacks any factual basis. I could very honestly have said on many occasions to Oppie, "I wish I understood you better. " However, I failed ever to do so. Since reading Haakon Chevalier's books about Oppenheimer,** I have wished for understanding even more intensely. These books give evidence that Oppie's early left associations should not be used to interpret him as a dangerous Soviet sympathizer, At the same time, these books provide a hint of the unknown depths that were Oppenheimer's personality. I remain totally unable to form an opinion of what his values and motives were.

The BBC film does not reveal the truth, nor does it offer explanations. But it gives a glimpse into some of the causes of the confusions and divisions from which people in the free world suffer. I hope through these reminiscences to offer a little insight into the contradictions and painful events surrounding that most remarkable person, J. Robert Oppenheimer. ■

**** Chevalier has stated that he told Oppie about a scientist, Eltenton, who was trying to obtain information about the bomb since he believed that Oppie should know this in order to prevent such activities from damaging him or the project. To my knowledge Oppenheimer never contradicted nor validated Chevalier's version.***

*****Haakon Chevalier, The Man Who Would Be God (Putnam, 1959), and Oppenheimer: The Story of a Friendship (Braziller,, 1965).***

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