LOS ALAMOS SC
UNIVERSIT
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OFFICE MEMORANDUM

TO : Edward Teller

DATE: September

FROM : N. E. Bradbury

SUBJECT:

SYMBOL : DIR

Unique Document # SAB 200085640000

THIS DOCUMENT CONSISTS
NO. 1 OF 1 COPIES, SERIES A

PUBLICLY RELEASABLE
OS-6 B. Palatin 1/26/94

1. In addition to your responsibilities as Assistant Director for Weapon Development for the Los Alamos Scientific Laboratory, I would like to ask you to assume the following responsibilities in connection with the development of an appropriate device for a large scale test of a thermonuclear explosion:

   (a) With the help of appropriate members of the staff of T Division and other theoretical and computational groups elsewhere, to prepare a complete schematic of an appropriate device to accomplish the above end and to establish the design criteria for its construction.

   (b) To approve, from the point of view of nuclear design, all engineering design details, and, of course, to provide the design engineers with necessary information during the course of development.

   (c) To determine the necessary diagnostic experiments which should accompany the actual test of the device, and to supply schematics of such experiments as required.

N. E. Bradbury
Director

NEB:cjd

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CLASSIFICATION CANCELLED
PER DOC REVIEW JAN. 1973

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DOE/OFFICE OF DECLASSIFICATION
KOLBAY, A.D.C. 11/3/95 DATE:

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penalties under applicable Federal law.

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PROBLEMS ALMOST AT ONCE PD PARA I BELIEVE IT IMPERATIVE THAT WE ESTABLISH A STABLE AND NONEMOTIONAL BASIS FOR THE PURSUIT OF THIS IMPORTANT PROBLEM PD IF TELLER IS WILLING TO SETTLE DOWN AND WORK WITH THE REST OF US TOWARDS THIS GOAL CMA THIS WILL BE FINE PD AFTER THE EXPERIENCE OF THE LAST YEAR CMA I AM NOT PERSUADED THAT THIS IS LIKELY TO BE THE CASE CMA AND I BELIEVE THAT EQUALLY RAPID AND CERTAINLY MORE STABLE AND UNEMOTIONAL PROGRESS WILL BE MADE TOWARDS A SIGNIFICANT EXPERIMENT IF WE STRIVE FOR THE MAXIMUM UTILIZATION OF TELLER AS A CONSULTANT RATHER THAN AS A CONTINUALLY DISSATISFIED AND REBELLIOUS MEMBER OF THE LABORATORY PD I RECOGNIZE THE POLITICAL IMPLICATIONS WHICH CAN FOLLOW CMA ALTHOUGH I WOULD SUGGEST THAT BETHE"S PARTICIPATION CAN BE AN ANTIDOTE PD HOWEVER CMA WE CAN CERTAINLY LIVE WITH TELLER IF YOU WISH TO TRY TO PERSUADE HIM TO STAY AND ENTHUSIASTICALLY

END PAGE FOUR OF FOUR

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FOUR PAGES - PAGE FOUR

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LOS ALAMOS SCIENTIFIC LABORATORY
UNIVERSITY OF CALIFORNIA
LOS ALAMOS, NEW MEXICO

UNCLASSIFIED

OFFICE MEMORANDUM

471.6 Thermonuclear Program

TO : N. E. Bradbury [redacted]

DATE: March 7, 1951

FROM : Edward Teller

HISTORICAL

SUBJECT: Comments on Draft Memo of March 6, 1951

SYMBOL :

With respect to paragraphs 5 and 6 on page 3 of your memo of March 6, I should like to make the following comments. I believe that Darol has conducted things in the thermonuclear field in the most admirable manner, and for personal reasons I would wish that he should continue to do so.

You know that I am not pressing at present for any reoganization or change because that might disturb the harmony of the Laboratory at a time when united effort is badly needed. The present proposal seems to me either a mere clarification or, at best, a slight modification of present arrangements as far as thermonuclear work is concerned.

In particular, with respect to paragraph 5, I do not believe that under present conditions it will be profitable to determine the relative efforts necessary in the fission or the thermonuclear field and then to continue to divide the responsibility of each division according to proportions needed.

With respect to paragraph 6, I do not believe that it will be profitable to continue to conduct thermonuclear work through a committee like the Family Committee.

I should be most eager to discuss these things with you at the earliest possible date. As you know, I have taken your advice and not discussed these things in the Laboratory.

I should most particularly want to emphasize that I would be delighted for the maximum amount of continued collaboration between Darol and myself. I think Darol will bear me out that there has been no lack of this on either side. I do not believe, however, that the thermonuclear work can either be continued or directed on a part-time basis.

OFFICE MEMORANDUM

TO : Norris E. Bradbury

DATE: March 23, 195

This document consists of __ pages
No. __ of __ copies, Series A

FROM : Lothar W. Nordheim

SUBJECT: - Comments on the Organization of the Thermonuclear Work

SYMBOL : ADWD- 288

One of the primary responsibilities of the Los Alamos Scientific Laboratory consists in the further development of fission weapons. Any progress in this respect results in an immediate improvement of the military situation of this country. It is therefore a task of overwhelming importance, particularly in view of the present emergency. On the other hand, we do not know yet what the future of fusion weapons will be. Some, like the booster and the highball, may quite well be of interest for the near future. The large thermonuclear weapons, the super and the alarm clock, definitely fall in a long-range category since their outlook is uncertain and their time of development has to be measured in years. On the other hand, it would be obviously extremely shortsighted to forego the study of this field. If such an attitude had prevailed in the last war, there would not be any atomic weapons now. This country simply can not afford not to know what the possibilities in the thermonuclear field are. In view of this situation, it would seem to be unfair and unwise to ask persons who are heavily preoccupied with fission weapons to devote attention to the new problems. The thermonuclear work can prosper only if there exists a definite separation of responsibility from that for the fission work. The scope of the work ought to be defined in directives from the AEC and not left to chance just how much time can be spared for it by persons busily engaged otherwise.

In the last war, the solution for such a situation was invariably the setting up of an entirely separate organization for the new purpose. This may not be advisable at the present time, for several reasons. The setting up of a new organization would produce considerable time delays, and it is not yet sufficiently clear how large an apparatus would be required. The knowledge of the handling of fission weapons and of uranium and plutonium fabrication is entirely concentrated at Los Alamos. It would be difficult to build up a competent group with this knowledge elsewhere. Finally, Los Alamos possesses already most of the facilities required, particularly with respect to fabrication of parts, handling of explosives and conducting actual tests.

The best solution of this dilemma would seem to be therefore to set up within the Laboratory a special organization which for practical purposes might be called a section, charged with the responsibility for the thermonuclear work, including the setting up of the program for it. The head of this section should have a status as an associate director of the Laboratory so that he would be automatically included in the discussion of all policy

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questions which might affect the work of his group. He would have complete authority over manpower and facilities necessary for his work as approved by the AEC and the Laboratory Director. The physical facilities of the section should include first, all such items which are required for its work but not for the normal fission business. While the general facilities of the Laboratory should be utilized as far as possible, additions which are necessary because the present ones are insufficient are to be placed in the new section. It would be presumptuous for me to go into details since I do not have an intimate enough knowledge of all branches of the Laboratory. The danger that has to be avoided at all cost is that policy decisions are influenced by conflicts of interest at divisional and lower levels.

It has been pointed out, and rightly so, that the setting up of such a new section might produce a split in the Laboratory. This will not be serious as long as the section remains comparatively small. The members of this Laboratory have always shown such a cooperative spirit that I anticipate confidently that there will be a continuation of exchange of ideas, mutual assistance and close cooperation between the new section and the other branches of the Laboratory. If, however, the time should come for the actual construction and testing of a large thermonuclear device, this will obviously demand a great effort and a strong organization. The LASL could undertake such a task within its present framework only at cost of the development work on fission weapons, which in my opinion will not decrease in importance. The new section, on the other hand, would form a nucleus that could be expanded to whatever is necessary without a disrupting influence on other parts of the Laboratory.

DoE b(3)

The foregoing remarks imply by no means a criticism of the previous handling of the thermonuclear work by this Laboratory. The advocacy of a clear separation of the two main tasks of this Laboratory is thus not based on any question of personalities but it is of an axiomatic character. The thermonuclear work has so far been developing in a natural way as a sideline with people pitching in whenever they could. I believe it is now at a stage where the interests of the country demand its reorientation and a clear division and assignment of responsibilities. In this way alone justice can be done to both the fission and the fusion work.

L. W. Nordheim

Distribution:

1A - N. E. Bradbury
~~2A - D. K. Froman~~ destroyed 2/2/55
3A - E. Teller
4A - L. Nordheim File

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DIR -625

1 June 1951

PUBLICLY RELEASABLE
OS-6 [signature] 1/26/94

Dr. J. Robert Oppenheimer
Director, Institute for Advanced Study
Princeton, New Jersey

Dear Robert:

Confirming our tentative arrangements over the telephone a few weeks ago, I shall plan to be in Washington 15 June and return to Princeton with you during the late afternoon or evening. General McCormack's office will know of my whereabouts during the day, and perhaps you can leave word there as to where and when we should meet. Would you also please ask your secretary to make reservations for me for the nights of the 15th and 16th.

I am looking forward very much to discussing the situation with you before the meetings. It is unfortunate that the emotions which the thermonuclear field has engendered are probably out of proportion to the actual problems involved. Basically, I hope that this meeting can give both the General Advisory Committee and the Commission some degree of confidence that the right problems in the field are being attacked with the right emphasis. I also hope that it will give Los Alamos confidence that we are seeing the situation with reasonable clarity and perspective. As you know, I regard this meeting as one in the normal course of the General Advisory Committee's responsibilities and one very properly called after the first experimental observation of deuterium-tritium burning. If a philosophy of "Where do we go from here?" can be maintained, I see every reason to hope that that should be a great degree of benefit to all the participants.

The things which we from Los Alamos would be glad to do in starting things off might be something as follows:

I would then like to have Darol Froman present the proposed laboratory program in this field, including the work which may be done elsewhere for us along these lines. Where at all possible, he will endeavor to put

DEPARTMENT OF ENERGY DECLASSIFICATION REVIEW
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tentative dates into this picture, recognizing, of course, that these are primarily useful in administrative planning and always subject to modification as the result of the turn of technical or military events.

Possibly between Carson's and Darol's remarks, I might make a few general statements myself as to the laboratory philosophy in distributing effort between this field and the fission field; the extent of the effort during the last year; its probable extent during the next year; the philosophy of testing thermonuclear devices in the field; and matters of this sort.

You will note that I have not suggested Edward as a laboratory protagonist in any of these opening remarks. The reason for this is, of course, to permit him full freedom, as a consultant to the General Advisory Committee, to express opinions which may differ in emphasis or enthusiasm from those expressed by the individuals above whom, I suppose, I expect to follow something of a "party line". Actually, I do not believe there is very much difference in actual opinion between Edward and myself in these matters except with respect to degree of emphasis, the philosophy of test preparation and test time scale and the nature of thermonuclear development administration. This latter matter I do not expect to discuss before the General Advisory Committee unless the question of separate laboratories or a separate division of Los Alamos is brought up by someone else.

We will plan to have copies of the program document on hand, and I shall try to see that you receive your copy as far in advance of the meeting as possible - even if this is only the Friday preceding!

With best regards until then, I am,

Sincerely,

NEB/hgs

N. E. Bradbury
Director

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Darol Froman, Carson Mark

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6 June 1951

N. E. Bradbury

Princeton Meeting

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1. Oppenheimer has asked what sort of presentation the Laboratory wishes to make in connection with the Princeton meeting.

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OS-6

Do6 b(3)

[ ] I suggested further that Froman present the proposed Laboratory thermonuclear program and that I might make some general remarks about relative fission-fusion effort, test philosophy, and so on.

2. Oppenheimer regards Nordheim, Teller, Wheeler and so on as consultants to the GAC (the speakers above wearing somewhat smaller hats in putting the case before the jury). This will also permit the consultants to speak as freely as they wish about the situation without any Laboratory strings or restraints attached.

NEB/hgc

N. E. Bradbury
Director

1A - Darol Froman
~~2A - Carson Mark~~
~~3A - Edward Teller~~
~~4A - Lothar Nordheim~~ destroyed 12/20/54
5A - File
6A - File

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95SA20B000244

October 13, 1949

TO: Technical Council Members

FROM: Edward Teller [redacted]

SUBJECT: THE SUPER BOMB AND THE LABORATORY PROGRAM

ADWD-2-7 [redacted]

On Monday there will be a discussion in the Tech Council concerning the Laboratory program and in particular concerning the question whether our effort can be so increased as to make the Super Bomb feasible within the foreseeable future. I should like to present to you my views on this matter in this memorandum. In this way, I hope that more thought can be given to the question before Technical Council convenes.

I would like to outline why it is essential for us to develop a Super Bomb at the earliest possible time or else be able to say with reasonable confidence that the Super is not feasible. The arguments that have led me to this conclusion are of various kinds.

POLITICAL CONSIDERATION: [redacted]

It is my conviction that a peaceful settlement with the Russians is possibl only if we possess overwhelming superiority. We do not now possess such superiority. The most promising prospect to acquire a great lead is by an early development of a Super Bomb. I am sure that such an accomplishment will in itself not solve the problem. Most difficult political questions will also have to be solved. But early possession of the Super Bomb will give us another chance in the political field. Without a Super Bomb such another chance is not likely to arise.

RUSSIAN PROGRESS:

The fact that an atomic explosion took place in Russia at this early a date has considerable significance. It seems that the Russian rate of progress is at least comparable to, if it does not exceed, the rate of progress in this country. The Russians have started working on the atomic bomb approximately in the summer of 1945. They are likely to have given consideration to the problem somewhat earlier but it is hardly probable that the total time of reasonably intensive effort in Russia has exceeded five years. This time is approximately the same as the time that was needed in this country to make and to explode an atomic bomb. Thus the rate of progress in R[redacted] is comparable to the rate at which we have been working during the [redacted] present rate of progress in comparison is far slower than in war[redacted].

It is probable that the Russians did not explore as many po[redacted]es before achieving an atomic explosion as we did. It is also pro[redacted] the Russians have not put into their atomic development th[redacted]horough and elaborate scientific and technical effort as we did. [redacted] therefore claim that their accomplishment is not equal to our wartime a[redacted]omplishment. If this is so, however, it merely proves that the elaborate precautions which we took in our atomic development was not absolutely essential. If the

Russians continue to make actual progress faster and if we lose the atomic armament race, it will make little difference whether the reason has been the particular brilliance of Russian scientists or the exaggerated caution and thoroughness of our own group. [redacted]

A detailed possible picture of the Russian progress can be imagined along the following lines:

The Russians probably were familiar with the plans for the Canadian heavy water pile. The captured German scientists also knew of plans for heavy water piles. There are two major technical difficulties in the construction of such a pile. One is the fabrication of metallic uranium. This the Russians have probably learned from the Germans. The other is the production of great quantities of heavy water. There are several ways to accomplish this. One of the best techniques is the distillation of liquid hydrogen. The necessary low temperature technique is quite well developed in Russia. The Russians may therefore have had a pile of the approximate efficiency of the Chalk River pile as early as 1948 or even 1947. The extraction of plutonium from such a pile is a difficult job but can be more easily accomplished if the precautions taken for protection of personnel are less elaborate than those taken in this country. Such precautions are and must be a paramount consideration in our country but the same is not the case in Russia. A pile of the Chalk River type working steadily at 30 megawatts can produce material for a trinity bomb in less than a year. It can produce material for a gun gadget in two years. It is quite possible that the Russians have made a successful implosion. It is also possible, although perhaps somewhat less likely, that they have used the gun assembly and that the actual nuclear explosion was performed with a relatively inefficient gun gadget.

The above description gives the Russians credit for a minimum of scientific and technical progress. Even if this minimum is accepted, further Russian progress can be anticipated along the following lines. The Russians probably will build further heavy water piles. They may also build small piles working with some of the plutonium which the heavy water piles produce. These aims can be rapidly accomplished if no excessive demands are made with respect to high flux, resistance of materials for irradiation or with respect to breeding properties. The Russian rate of production of plutonium may equal the rate of our production within a year and indeed we have no absolute assurance that our production has not been already surpassed. It is therefore not impossible that the Russians should overtake us even in the matter of the stockpile which is the one item which never has been neglected in the United States.

An even more dangerous situation seems to exist with respect to neutron excess. This neutron excess is much greater for heavy water piles than it is for graphite piles and of course even greater for plutonium piles. [redacted] is reasonable to assume that the Russians either are already ahe[redacted] this respect or will be ahead of us in the near future.

The number of available excess neutrons is of decisi[redacted] in several war time applications of atomic energy. [Among the[redacted]uction of tritium is probably the most important since tritiu[redacted]ction is an important component in the production of the Super Bomb.]

Therefore, it is quite possible that the Russians will possess a Super Bomb within a short time. The reasons for this conclusion can be summarized as follows: [redacted]

1. The Russians have been evidently working with considerable speed and efficiency and their efforts are not likely to slacken now.

2. The Russians are likely to have produced their plutonium in heavy water piles. Thus, they may even at present have considerably more excess neutrons available than we do. Their tritium production may thus surpass ours in the very near future.

3. The Russians had greater assurance than we did during the war concerning the feasibility of the fission bomb. It is therefore not impossible that even during their work on the fission bomb, they may have made thorough preparation for the time after the fission bomb has exploded. This preparation might have included work on the Super.

4. Our discussions have so far concentrated on designs of the Super whose feasibility can be proved theoretically. This has led to complicated--perhaps unnecessarily complicated--arrangements. Simpler ways of detonating a Super may exist and may be discovered by the Russians.

If the Russians demonstrate a Super before we possess one, our situation will be hopeless. In case of war, we can hardly hope to survive and it might even be possible that under such conditions, the Russians may force this country to surrender.

TRITIUM PRODUCTION IN AMERICA:

Some recent developments and discussions have shown that the required amounts of tritium may become available in a relatively short time. These developments are:

1. The techniques of tritium production have been improved by the use of lithium aluminum alloy.

2. Information from Chalk River indicates the immediate feasibility of heavy water production piles.

3. According to recent information received, the production of 1/10 ton to one ton heavy water per day would not be an expensive undertaking. In particular, it seems that a distillation pr[redacted] could be set up at a capital cost of about $7,000,00[redacted] duction cost of about $9,000 per ton of D20 for [redacted] of producing one ton of heavy water a day. [redacted] ple a supply that heavy water production is not li[redacted] become the bottleneck in the construction of heavy water [redacted] les.

4. Early increase of tritium production could be had by collaboration with the Canadians and use of their Chalk River pile. An estimate shows that this pile could deliver 25 grams of tritium per year. [redacted]

5. The tritium production of the Hanford piles could be stepped up immediately by use of enriched slugs.

6. Somewhat later, the construction of highly enriched piles will raise the tritium production.

Thus, tritium could be produced in three steps: Immediately, in the Hanford piles by enriched slugs and in the Chalk River piles, in the near future with the help of more heavy water piles and somewhat later by strongly enriched piles.

LABORATORY PROGRAM:

Considering the arguments given above, it seems quite possible that the bottleneck in making a Super Bomb will be lack of theoretical knowledge and lack of technical knowhow in the Los Alamos Laboratory. As indicated the problem of tritium production is not likely to be the bottleneck and we must thus avoid the possibility that this Laboratory slows down progress. I should therefore urge that we give to these two problems top priority. I should like to recommend the following three immediate steps:

1. Lay plans for an early speed up and reorientation of Laboratory effort.

2. Obtain personnel capable of accomplishing the necessary job in the shortest possible time. I believe that the Laboratory should make an all out effort to try to obtain on a full time basis the help of the people most useful to help the Laboratory in the development of the Super Bomb, such as Enrico Fermi and Hans Bethe.

3. Call a conference in November for the discussion of the detailed steps which have to be undertaken inside and outside of the Laboratory in order to produce and test a Super in the shortest possible time. This conference would try to unite at one place much of the theoretical and experimental top talent from outside the Laboratory who can help and advise on details.

In more detail, I envisage the impact upon the Laboratory somewhat as follows: The Super program would become the No. 1 objective of the Laboratory. However, this would not mean that work on other important phases such as a small weapon, a penetrating weapon, et cetera, would be stopped. Only in cases of an irresolvable conflict between two programs would the Super program take precedence. In the early stages probably the largest burden of work on the Super prog[redacted] would fall on the Theoretical Division. After that various concret[redacted] would have to be explored.

DOE b(3)

One of the first objectives would be to step up the date on which one can hope to produce a successful booster.

TIME SCHEDULE:

If the Laboratory can marshall the necessary support from Washington for a really vigorous program, the problem of construction and detonating of a Super might be attacked with the same speed with which similar problems were attacked during the war. I realize that this program calls for an all out effort. However, I do not believe that anything less than such an all out effort would be commensurate with the responsibility which this Laboratory has undertaken with respect to the ultimate safety of the nation. It will be essential that all members of the Laboratory contribute fully any ideas they may have how to accomplish the technical details of this job.

[redacted]

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No. 120 of 130 Copies, Series A

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October 13, 1949

Unique Document # SAB20008566
1/1B made 12/5/66 and sent to Richard G. Hewlett AEC, Wash.

TO: MEMBERS OF THE TECHNICAL COUNCIL

FROM: J. H. Manley

SUBJECT: THE FUTURE LABORATORY PROGRAM

TAD-73

Whatever statements the National Military Establishment or the AEC have made or may make concerning the effect of the detonation of a Russian bomb, this Laboratory should admit at least to its own personnel that the current Laboratory program has not been geared to such an event in 1949. Rather, it has been tacitly assumed that this event would not occur before 1952, a date beyond the expected 1951 fruition of current programmatic work. At the very least, therefore, the Laboratory should consider that it has lost some three years of time. Nor is this all, for the simplest assumptions as to relative rate of progress of this country and the USSR give strong support to the contention that it would be dangerous to assume that their progress is any less than ours; in fact, it can be quite logically maintained that it is appreciably greater. It is, therefore, imperative that the Laboratory's role in this new situation be examined most thoroughly. This has been in process in portions of the Laboratory but views must now be exchanged on a wider basis among responsible Laboratory personnel, especially since the Laboratory program is to be discussed with the Commission on October 19.

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Certain trends of reactions to the situation are apparent. They range from an all-out effort to make a real discontinuity in the balance of power as rapidly as possible by developing the super-bomb to almost a "business as usual" attitude but involving as much speed-up as possible in the normal type of business, the production of present-model weapons. The Russian achievement should teach us at least one thing: that our state of ignorance of their efforts is so nearly complete that we should no longer assume any time scale for their developments but rather choose our action so as to strengthen ~~our~~ position as rapidly as possible and maintain a rate of progress limited only by our resources for a relatively long period of time. In other words, we can neither jeopardize the future for short-range achievements nor such achievements at a cost of longer-term progress. For this reason it would be unwise to choose a single course of action for the whole Laboratory effort. One may be as important as another, depending on the time-scale of international developments.

Another general comment may be made. It is evident that no one now has the wisdom to foresee the method by which a resolution of the East-West difficulties may be achieved. Nevertheless, the longer the time before war is resorted to in a vain effort at resolution, the higher the probability that an alternative may appear. Since 1946 this country has pursued a policy, among others, of military strength, based on the atomic bomb, as a deterrent to war. No other policy seems likely to be accepted. The position of this Laboratory as the atomic weapon research and development facility of the nation in the instrumentation of this policy has been and is quite clear: to maximize the nation's strength in the applications of nuclear explosives for military purposes.

In accordance with the point above, that no single course will be clearly desirable, a few possibilities and brief comments on each are listed below in order to suggest the type of effort which should receive attention. It is certainly not an all-inclusive listing.

1. Super-Bomb

This possibility has been more thoroughly discussed than any other and is discussed in a memorandum by Mr. Teller. The salient points are:

a. The development may be relatively simple.

b. The Russians may very likely be further along than we are in both the weapon itself and matters of production.

c. Possession by one side would shift enormously the balance of power.

d. A vigorous effort in all departments by this country could possibly result in a test by 1951.

2. Present Program

In the absence of a Russian Super-bomb an acceleration of the present program may be the best protection of our position for the short-term.

a. TX-5 increases military potential through greater deliverability through smaller size and weight.

b. TX-5 developments applied to present size will increase damage per unit, no change in number of units.

c. 7 DoE b(3)

d. Booster increases damage per unit and number of units. Also provides valuable information on thermo-nuclear reactions.

A less thorough experimental program before test would advance the date of test.

3. Extended Present Program

a.

b.

Greater efficiency, larger damage per unit, fewer units. Possible super-bomb initiator.

c. "Bethe-bomb"

Greater efficiency and damager per unit at no great cost in numbers.

d. Alarm Clock

A special type of combination fission-thermonuclear reaction weapon producing widespread radioactive effects.

4. Production of fissile materials

Laboratory strength applied here or elsewhere to problems of production could result in more present-type weapons at cost of new weapon developments.

5. Propulsion

Applications of nuclear energy to propulsion of ships and/or aircraft could increase our military potential.

6. Guided missiles

An acceleration of this program for this type of delivery could be undertaken. For short range use, presently planned missiles can carry

7. Autocatalytic schemes

A successful scheme could appreciably increase deliverability.

8. Other military applications of nuclear explosives

Mines, torpedos or present unconceived uses may increase effectiveness.

9. Radiological warfare

Although some work is underway, great acceleration could take place. Requires special emphasis on production and methods of use.

10. Fundamentals

The long-term position could be improved by more vigorous fundamental research in high-explosives, nuclear physics, chemistry and metallurgy.

J. H. Manley
Technical Associate Director

[redacted] This document consists of 3 pages
No. 1 of 7 copies. Series A

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UNIVERSITY OF CALIFORNIA
LOS ALAMOS SCIENTIFIC LABORATORY
(CONTRACT W-7405-ENG-36)
P. O. Box 1663
Los Alamos, New Mexico

Unique Document # SAB200085610000

IN REPLY REFER TO: ADWD-2-111

December 13, 1949

CRM FILE BC
DD 50997

Dr. John H. Manley
Los Alamos Scientific Laboratory
P. O. Box 1663
Los Alamos, New Mexico

Dear John:

I have considered carefully your suggestion to recapitulate the discussion which took place in the presence of Senator McMahon and Mr. LeBaron on November 15th. I think I can remember accurately the main ideas which I tried to put across concerning the feasibility of the booster and of the super. There is no reason for any change in these ideas. What follows below, therefore, will be both a restatement of my previous opinion and the statement of my present opinion. I understand that you will want to transmit this letter to General McCormack and to LeBaron.

There are two distinct models of the booster which must be discussed in connection with the super. It is this booster which we plan to shoot in the spring of 1951.

by the system of external or internal boosting.

A shot involving either of these two boosters will be a test of the feasibility of thermonuclear reactions. In this respect, it will be a test of a part of the theory on which the super is based. There is, however, little doubt that the TD reaction will actually take place under proper conditions and therefore this test in itself will confirm only a part of the theory of which we feel fairly certain.

It is planned that the test of the booster will be accompanied by a detailed investigation of the 14 million volt neutrons emitted in the TD reaction. It is hoped in particular that we shall be able to follow the production of these neutrons as a function of time. Such experiments would be of extremely great value in the plan of constructing the super and would actually have to be repeated whenever a model of a super should be tested. Success in predicting the time-dependence of the 14 million volt neutrons will give us confidence that the engineering of a planned super has a sound theoretical basis.

1ST REVIEW DATE: AUTHORITY: NAME: 2ND REVIEW DATE: AUTHORITY: ADD NAME:
DETERMINATION (CIRCLE NUMBER) 1. CLASSIFICATION RETAINED 2. CLASSIFICATION CHANGED TO: 3. CONTAINS NO DOE CLASSIFIED INFO 4. COORDINATE WITH: 5. CLASSIFICATION CANCELLED 6. CLASSIFIED INFO BRACKETED 7. OTHER (SPECIFY):

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RESTRICTED DATA

[redacted]

The question has been raised whether failure of the booster test would indicate that the super is not feasible. The answer to the question will depend on the kind of failure which we may encounter. The failure may be due, for instance, to predetonation, in which case the booster will have little bearing on the planning of the super. [redacted] Thus failure to observe the expected amount of thermonuclear reaction in the booster need not imply that the super is not feasible.

Present opinion on the feasibility of the super is necessarily speculative since all discussions rest on theory alone. On the other hand, no serious objections have been raised to the present plans and I feel that ultimate success in construction of a super is probable. I do not like to state probabilities of this kind in a percentage but I do want to imply that the probability of success is considerably better than 50%.

DoE 6(3)

[redacted] In such event, I would not hesitate to attach to this probability a percentage greater than 90%.

DoE 6(3)

[redacted] Further mathematical verification of this expectation will probably be available by 1950.

It can be therefore concluded that the test of an appropriate booster together with supplementary mathematical work will provide an almost complete proof of the feasibility of the super and will at the same time accomplish the major development in the engineering of the super.

Edward Teller

Clear at CRP 4/3/90 [illegible]

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[redacted]e Document # SAB2000856800C0

[redacted]

[redacted]

Drawer 1

47/16 Sup

ADWD-2

February 8, 1950

Dr. Luis Alvarez
Radiation Laboratory
University of California
Berkeley, California

Dear Luis:

Needless to say that I am enthusiastic about the Presidential decision and that all of us here are now busy like monkeys. (I hope you will be kind enough to interpret this as an indication of the quantity and not the quality of our business.)

The main purpose of this letter is again to ask you and the Radiation Laboratory for help.

DOE 6(3)

The great importance of this experiment is that the soft X-rays are likely to play an important role in both the ignition and possible quenching of the thermo nuclear mixture.

I shall not try to make suggestions how this experiment should be done. I have some in mind but probably only such which would occur to you readily. I wonder whether the Radiation Laboratory could take over this job. It would be a very sizable help and would allow us to proceed faster and more efficiently with other phases of the work. I do not know how many people you would need for such a job but my offhand guess would be that the number may be as great as a dozen good people.

I hope that you will not turn down this request without giving it a little thought. Probably you would want to talk it over with Ernest. Would you be good enough after having done so to give me a phone call? With best regards to Ernest and yourself,

1A - Alvarez - Rec. 43623

3A - File

Edward Teller

AUTHORITY: DAOC ... DAOC
NAME: [illegible]
2ND REVIEW DATE: 11/22/95
AUTHORITY: ADD
NAME: H.R. Schmidt

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2. CLASSIFICATION CHANGED TO:
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[redacted]

AH 16

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Lab - T-202

[redacted]

S E C R E T

UNCLASSIFIED

June 24, 1950

CRM FILE BC i

DD 50872

Document # SAB200085690000

To: Carson Mark

From: Hans Bethe

During my ten days stay here, I have considered some of the phases of the calculations concerning the Super weapon. My opinions on this are obviously tentative, both because of the incompleteness of the calculations and because of the short time I have spent on this subject. However, I thought it might be of some value to set down my present thinking.

1) I believe that the last calculation by Ulam and Everett definitely indicates the dying out of the reaction.

DOE 6(3)

This dying of the reaction is to be expected on qualitative consideration.

2) I expect that also the proposed Aberdeen calculation will die out.

DOE 6(3)

I expect, therefore, that it will be impossible to raise any pure deuterium to burning temperature in the Aberdeen problem.

3)

DOE 6(3)

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[redacted]

RESTRICTED DATA

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Seen by Bradbury [illegible] Ret'd to file Aug 16, 1950

I believe there is about an even chance that this amount of tritium will set off a spontaneous propagating reaction in pure deuterium if the inverse Compton effect is neglected.

Proposals were made for a simplification of the calculation of deposition of energy by alpha particles in the TD reaction. It was also proposed to increase the thickness of the zones in order to simplify the calculation. It would be most desirable to think of further simplifications and improvements in the calculation procedure. In particular, a method is needed for taking into account the shock which will undoubtedly develop, and it would be most desirable if this could be done by an integral method.

will be needed because the detonation propagates into larger volumes.

It would be desirable if some simple method of calcuating this problem could be devised. DOE b(3)

[redacted]

DoE 6(3)

7) Efforts will undoubtedly be made to reduce the amount of tritium needed for initiation by implosion. [redacted]

DoE 6(3)

Considering the high efficiencies which are expected from ordinary implosion weapons, it seems to me very improbable that the Super weapon will be justifiable from the economic point of view.

However, once we have gone this far, it seems to me essential to come to a real decision on the feasibility and economics of a Super bomb. I therefore believe that calculations should continue as rapidly as possible in order to find out exactly the minimum tritium requirements for a Super weapon and to decide the question of whether pure deuterium will propagate the thermonuclear reaction at all.

I also think that the 1951 tests should proceed essentially as planned.

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29

THE NEED AND PHILOSOPHY OF FIXING UPON A THERMONUCLEAR DEVELOPMENT PROGRAM

Unique Document # SAB2000857000000

March 15, 1951

The need for such a program, which will, of course, require modification with time, is illustrated by the following points:

1. The general advantage of unifying the effort toward definite goals, with either relative priorities or defined time sequences, in order that finite accomplishments can be attained with a limited effort. Wide dissipation of the effort over a very broad field can result in little accomplishment anywhere. On the other hand, extreme narrowness can result in "missing the boat" and new, important ideas must not be neglected.

2. In order to establish a sensible fraction of our potential to each of our three main technical activities, basic research, fission weapons, and thermonucleonics, fairly detailed programs and objectives for each must be studied and compared.

3. In order to insure that the fraction of the Laboratory effort agreed or decreed to be devoted to thermonucleonics not be stolen by another activity, especially in some of our hectic periods, different sections of the Laboratory must be working upon fairly definite phases so that judgment can be made upon both progress and actual type of work being done.

4. For purely political reasons we need a rather definite program and objectives in order to report progress in a meaningful way and thus avoid, possibly, criticism of making no progress in the field or of not really trying to make progress. Up to the present this need has been satisfied by preparations for Greenhouse tests - a definite goal marking a definite stage in development.

Att 19

~~SECRET~~ UNCLASSIFIED

[redacted]

In formulating such a program the following points should be considered:

1. The present phase of the thermonuclear program was started about a year ago with the single idea of developing a super or H-bomb. If this was not the case in the minds of some of us at Los Alamos, it was for many of us, and is largely true outside the Laboratory. Thus, there is a solid, and to some extent political, reason for expending the major part of our thermonuclear effort toward determining (a), the feasibility of making a deuterium-burning bomb, (b) if feasible, the minimum cost (in terms of neutrons, fissionable material, tritium), ~~and~~ (c) if feasible and sufficiently cheap, the practicable shape, weight and similar characteristics and (d) the military worth and use. Until such work is accomplished, other ideas, e.g. alarm clocks, should receive only minor attention unless they appear upon such cursory study to be very much more practicable, easily attainable or useful. [redacted]

DoE b(3)

The premise here is, of course, that we do not and will not have sufficient power to accomplish work at an acceptable rate in more than one of these difficult fields at once.

DoE b(3)

[redacted] It is not obvious at present what form a succeeding test should take. Under these circumstances it seems wrong to try to manufacture a test, and certainly wrong to choose its date at the present time. Logic would indicate proceeding with theory and other development, including assimilation of the Greenhouse results, until the need for a

[redacted] UNCLASSIFIED

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specific expermental test arises naturally in the course of the work. This philosophy also has the advantage of not dissipating scarce man power upon the details of a test model before general principles are established.

3. Very definite goals in a time sequence should be established. This psychological aid should, to a large extent, supply any need felt for a deadline of the type appearing from test dates.

4. There appear to be enough problems and questions known to constitute the basis for a program. Thus, the plan, at this stage, should be to direct the program toward answering known questions and not to try for several months, at least, to provide facilities or manpower to handle simultaneously problems which have not yet been thought of but which will surely turn up. The program may need revision to meet these problems as they arise.

30 [redacted] 7/26/56 [illegible] 4716 Thermo Prog[illegible]

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[illegible] DSRD 9 [illegible] 4/3/90 SAB2000857100000 3/15/51

Notes for Darol Froman by S. Ulam   Unique Document # [redacted]

Some of the important projects necessary for a rational thermonuclear program.

[redacted] This will be completely set up by Johnny von Neumann, Evans, etc. this summer. If the machine exists by July or August, the problem may be settled once and for all - say before October. If there is no Maniac in sight, it has to be done on another machine with less detail perhaps, but still will be better than the Fermi-Ulam calculation. This is a must for this year.

[redacted] Granting the absence of mixing one should estimate the probable efficiency of this system and the initial tritium expenditure for various assumed compressions. The work of Nordheim, Richtmyer, et al., must be continued at all cost and a calculation on some machine made before the end of the year.

3. To evaluate the feasibility of assumed compressions, a lot of theoretical effort is necessary. It would seem to me that with the greatest effort it still will take several years to evaluate and design

   a). the heterocatalysis

   b). theoretical predictability of mixing of materials

4. The points discussed in 3 could be studied experimentally on scaled down models of various sorts. The theoretical work to devise and interpret meaningful models is difficult and it can hardly be hoped that this will be completed soon.

5. Various cures for ailments of thermonuclear reactions, e. g., walls with wonderful properties, hydronuclear possibilities - interesting study for years to come.

6. For all schemes a little study of military interest of such devices, i. e., the deliverability as a function of estimated size and weight - equivalent cost in fission bombs and the area of destruction.

S. Ulam

1 - Darol Froman
2 -

i

[redacted]

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ROUGH DRAFT [redacted]

ADWD-272

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6/7/51

LOS ALAMOS LABORATORY PROPOSED THERMONUCLEAR PROGRAM

Unique Document # SAB2000857200000

The considerable success of the thermonuclear shots at Eniwetok and the rapid theoretical development of new thermonuclear devices create a rapidly changing situation. It is, in this situation, impossible to propose a realistic program extending over several years. It is, however, desirable to summarize the status and the immediate plans for at least the three devices which have been considered prominently as possible realizations of a full-scale thermonuclear explosion. [redacted]

DOE b(3)

Consideration of booster weapons is omitted. It is expected booster weapons will be useful. It is clear, however, that sound theoretical calculations are needed before effective weapons can be developed and it is furthermore clear that both the effort and the result to be expected from these developments are significant but moderate.

[redacted]

DOE b(3)

No really significant progress in this matter can be expected prior to a full-scale Maniac calculation. This calculation is being prepared by J. von Neumann and some staff of T-Division. No significant expansion of this group is necessary except possibly for the period of the actual run on the Maniac.

In preparation for the work on the Maniac, some theoretical calculations are needed which are carried out by Breit and collaborators. It is recommended that these calculations be continued.

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[redacted]

In order to clarify the behavior of the inverse Compton effect, calculations have been proceeding at Rand, under the direction of de Hoffmann, from the Los Alamos Laboratory, with the help of Stein. These calculations have proved to be useful as preparation and orientation for the proposed Maniac calculations, and it is recommended that these calculations be continued as long as the Maniac group considers them valuable.

Further calculations by analytical methods have been carried out by Bethe and collaborators. It is considered that these calculations have served their valuable purpose and that this most efficient group should be employed on more urgent problems.

Under these conditions, it may be expected that results will be available early in 1952. It is not recommended that any further experimental or developmental programs be carried out for the exclusive purpose of the super calculations. Measurements of DD and DT cross-sections are continuing. These are relevant for all thermonuclear work. The same holds for the opacity work in Chicago.

7

[redacted]

It is probable that such a test will be proposed for a later date or else that no such test will be proposed.

[redacted]

Reasonably complete and conclusive calculations seem to be simpler. Such calculations are now scheduled to start on June 15 and will probably start somewhat later than that date.

An active group consisting of Nordheim, Peck, Richtmyer and collaborators is now working on this problem. This group might be strengthened by the part-time work of Goertzel and possibly by Scalletar in the fall.

[redacted] Marshall Rosenbluth and others have made recently considerable progress in discussing this topic. It will not require great effort to follow up these developments in case they should turn out to justify the promise which they seem to show at present.

DOE b(3)

The necessary high compressions can not be obtained by means of high explosive implosion. It is planned to achieve these high compressions with the help of radiation implosion originating in a primary fission bomb. [redacted]

The potential promise of this device seems at present the highest of any device as yet proposed. On the other hand, least consideration has been given to this device and the possibility exists that a decisive flaw will be found. It is therefore proposed that highest priority be given during the summer of 1951 to a detailed investigation [redacted] Some work has been done on this device by de Hoffmann, Hoyt, Bethe, Ulam, von Neumann, Teller, Wheeler and others. In addition, it is recommended that a considerable fraction of T Division at Los Alamos, a considerable fraction of the summer visitors at Los Alamos, and practically all of the Princeton group concentrate on the problems [redacted] It is hoped that by early fall of 1951 one will have concluded that either the very highest priority be given to the immediate technical development of the [redacted] and that the main effort should be concentrated on other thermonuclear devices.

[redacted] UNCLASSIFIED

No discussion of the test date is justified prior to the presentation of a reasonably detailed theoretical design which can be considered final at least in its main features. Every effort will be made to make the design as simple as possible. In particular, it will be attempted to limit the observational program connected with the tests to essentials. One may hope to obtain in this way a proposal which can be carried out in a period less than a year after presentation of the preliminary model.

It seems therefore advisable to proceed without delay with the necessary jobs of maintenance and expansion of cryogenic equipment on Eniwetok. This last recommendation need not, of course, be followed if an alternative method of liquid deuterium delivery and handling is found.

1RD - N. E. Bradbury
2RD - H. A. Bethe
3RD - D. K. Froman
4RD - J. C. Mark
5RD - L. W. Nordheim
6RD - E. Teller

[redacted]

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[redacted]

[illegible]

July 13, 1951

[illegible]

[illegible] PROGRAM

[illegible]-143

1. [illegible], I am belatedly forwarding you a copy [illegible], if you wish, of the Los Alamos Scientific Laboratory [Thermonuclear] Program as it was finally sent to the Atomic Energy [Commission].

2. The original draft which was discussed at Princeton was very much the same as this except in two significant respects. Since these differences are [rather] revealing of the Washington--General Advisory [Committee] [frame] of mind (at least as expressed in public), the major changes which went into the final form are quoted herewith:

Princeton Draft	Final Form
V. (A) It is presently intended that a device be proposed for test only if such a test is capable of leading in some foreseeable way to a device having weapon utility.	V. (A) It is presently intended that a device be proposed for test only if such a test is required for a significant increase in the understanding of thermonuclear systems, or if such a test is capable of leading in some foreseeable way to a device having weapon utility.
V. (B) [redacted] ~~is made at this time.~~ The Los Alamos Scientific Laboratory does not believe it appropriate to recommend immediate plans to	V. (B) [redacted] Steps are understood to be underway to meet such a rate at the earliest practicable time.

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Page [illegible] - C. L. Tyler July 13, 1951

Princeton Draft

V. (B) Cont.

[redacted] Attention is, however, called specifically to this point since any decision on this is somewhat in the nature of a gamble and the question might well deserve further discussion.

The other changes were minor and did not suggest any significant change in laboratory thinking.

Original Signed by N. E. Bradbury

N. E. Bradbury
Director

NEB:cjd

1A - C. L. Tyler — Rec 68869.
2A and 3A - Files

DISPATCHED JUL 13 1951 Los Alamos Scientific Laboratory P. O. Box 1663

~~SECRET~~ UNCLASSIFIED

SOME IDEAS ABOUT THE LASL THERMONUCLEAR PROGRAM
June 1951

TAD-262

No great gains have been predicted for boosted implosion devices by using thermonuclear reactions over those obtainable by more conventional means using fissionable materials and, in view of the very heavy program of other work, no specific programs in this field are foreseen in the near future.

Two important points should be made clear from the beginning. It is believed that there is insufficient evidence now to assign very different priorities to these systems, but more relevant information will be available in the Fall of 1951 and by that time, or by June 1952 at the latest, meaningful relative priorities can be assigned and the work greatly concentrated. It is also not clear that a system upon which work may be concentrated several months from now is at all accurately represented by the present conception of any of these three devices; it may be a combination object or have presently unimagined features. Second, the target dates given under each of the devices are estimates of the dates at which the development might be at the indicated stage on the assumption that the particular device has a priority over-riding the others after early 1952. In general tests will be limited to the theoretically promising items which appear capable of meeting military worth criteria in the foreseeable future. XXXXXXXXXXXXXXXXXXXXXXXX

I. GENERAL.

1. Greenhouse Results.

TARGET DATE: Rough draft of report scheduled for August 1, 1951.

PRIORITY: A.

PERSONNEL: LASL theoreticians and groups J-1 and J-7.

[redacted]

2. Mixing.

Theoretical considerations of the general problem of mixing as applied to any thermonuclear device. Objective is to determine the feasibility of making significant progress toward the solution of this problem by theoretical and calculational means.

TARGET DATE: JANUARY 1952 for review of progress and determination as to whether or not further effort should be expended along these lines.

PRIORITY: C (low priority because the extreme difficulty of the problem makes it seem not very hopeful for valuable results at an early date).

PERSONNEL: Princeton Group.

3. Radiation Implosion.

Radiation implosion and other heterocatalytic methods.

A. Theoretical.

Appropriate calculations as indicated below under the devices.

TARGET DATE: January 1, 1952 for basic theoretical and calculational methods.

PRIORITY: B, but changing to A if required for an A priority development.

PERSONNEL: Teller and staff and T Division.

B. Experimental.

4. Boulder Cryogenic Facility.

Apart from testing the large hydrogen liquefiers now under construction at NBS and installing one of the plants at Eniwetok, probably the first job to be undertaken is to solve the problem of storage and transportation of liquid hydrogen by trucks or tank car.

[redacted]

[redacted]

[redacted]

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TARGET DATE: Work in the near future is being planned to meet a requirement for large or complicated dewars to be used in a test in the spring of 1954.

PRIORITY: Different for different phases and dependent upon the priorities of the different systems.

PERSONNEL: National Bureau of Standards with advice from LASL.

5. Military Worth.

A continuing study and intercomparison of the economics, deliverability and military worth of each thermonuclear device as envisaged at all times during its development. Criteria are to be developed to be used, in part, to determine at appropriate times whether or not to proceed with further theoretical and calculation work or with a test relating to a particular device. It will be a general principle not to make an early test of a device whose development into a deliverable weapon having acceptable military and economic characteristics cannot be foreseen as practicable. The point of the whole program is to produce a weapon, not merely to demonstrate the feasibility of a large scale thermonuclear reaction.

Personnel carrying out this study will necessarily have to be conversant with the economics and military worth of fission weapons. It is proposed that this group have the responsibility for evaluating military worth for both fission and thermonuclear weapons, and, thus, perform a needed function in the fission weapon field. It is not intended that this group undertake the theoretical work in the field of weapon effects and phenomenology.

TARGET DATES: To coincide with dates of important decisions with respect to the several devices as indicated below. For example, the alarm clock ~~decision~~ schedule calls for a decision in Nov. whether or not to proceed with alarm clock development at high priority and comparisons should be made with the picture of the equilibrium super at that time.

PRIORITY: A.

PERSONNEL: A new LASL group (to be formed).

6. Cross Section Measurements.

Improved measurements of the (D,D), (D,T), Li^6 and similar cross sections at relevant energies.

TARGET DATE: June 1952.

PRIORITY: A.

PERSONNEL: P Division, C. I. T., and other laboratories.

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[redacted]

7. Lithium-Hydrogen Fabrication Studies.

At any time at which it appears very likely that Li^6D and Li^6T may be used in one of the devices put on high priority (for example, at the time a decision is made to build a lithium isotopic separation plant) studies will undertaken of fabrication methods for these compounds. A certain amount of fundamental work upon the physical properties of these compounds is presently under consideration.

TARGET DATE: Acceptable fabrication methods by the time the separation plant is in production, possibly November 1952.

PRIORITY: To conform with that of the device in which its use is contemplat

PERSONNEL: CMR Division.

8. Opacities.

The calculation and tabulation of opacities will be continued, at least throughout the next year, upon substances of interest to the thermonuclear program.

TARGET DATE: July 1952.

PRIORITY: A.

PERSONNEL: Argonne National Laboratory.

II. The Super.

A. Theoretical.

1.

Preparation for and calculation on the Maniac under the following conditions: wall effects neglected, zoning of energy deposition, normal density, best present values of cross sections, non-relativistic inverse Compton effect, hydrodynamics included.

These calculations are designed to determine allowable initial conditions, effect of variation of radius, estimates of the amount of tritium required and fission bomb yield required to ignite the deuterium, ~~etc. Some of these calcu~~lations, or modifications of them, may be pertinent and problems directly relevant to the latter may be undertaken in ~~this series~~. The coding for the super problem is now nearly completed.

[redacted]

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TARGET DATE: Coding to be ready when a Maniac is ready to take the problem. Completion of several Maniac runs by January 1, 1952.

PRIORITY: A.

PERSONNEL: T Division and von Neumann (Rand).

2. [redacted]

Preparation for and calculation on the Maniac of the propagation including wall effects, relativistic effects, etc., and special devices or conditions designed to improve the reaction [redacted]

These calculations follow A.1. above and are designed to go a long way toward determining the feasibility of the super if A.1. has not already done so.

TARGET DATE: To cover the field may be a long job, but the objective is to be prepared to make Maniac calculations including significant improvements and variations over the preliminary calculations during the spring of 1952.

PRIORITY: B, for the present. This priority may be altered either direction depending upon results of the preliminary calculation and military worth studies.

PERSONNEL: Theory by Princeton Group, Breit and Bethe (problems distribute calculations by T Division and von Neumann.

3. [redacted] Decision whether or not to undertake this work seriously is to be made by June 1952 and will depend upon the results of Maniac calculations on propagation and of military worth studies.

TARGET DATE: Completion by January 1953 of the theoretical work upon which to base the design of primer and booster.

PERSONNEL: T Division with probable request for Princeton Group to assist in certain phases.

B. Experimental.

1. [redacted] If the former, experimental studies will be required (possibly some experimental work in either case). This work will be closely tied in with A.3. above, possibly lagging somewhat. [redacted]

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[redacted]

TARGET DATE: Completion of experimental study required for design of test object, April 1953.

PERSONNEL: GMX Division.

2. Experimental studies on equation of state of liquid hydrogen and deuterium if results of calclations indicate by June 1952 that the uncertainties in our knowledge in this field are determining or very important factors. [redacted] No specific programs in this field are planned for the near future but some preliminary experiments are under consideration.

DOE b3

TARGET DATE: JANUARY 1953.

PERSONNEL: GMX and CMR Divisions.

3. Nuclear detonation tests as may be required for significant further progress. At the present time no test other than one of a real super, although possibly one of low yield, appears to be required.

TARGET DATES: Completion of design of super for test, May 1953. Test of super if it meets the various criteria, Spring 1954. [redacted]

PRIORITY: High, if decision is made to test the super.

DOE b3

PERSONNEL: T, W, CMR, GMX, and J Divisions.

III. [redacted]

DOE b(3)

It appears that the NBS SEAC can handle this problem. [redacted] Probably several runs will be made with the general objective of determining minimum acceptable values for compression, tritium content, and initiating fission bomb yield and the relations between these quantities. It is hoped that these calculations will be sufficient to determine whether H. E. implosion is adequate or other means such as radiation implosion are required and to supply reasonably good data for considerations of military worth.

DOE b3

[redacted]

[redacted]

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TARGET DATE: November 1,1951.

PRIORITY: A.

PERSONNEL: Nordheim, Richtmyer and other LASL theorists.

2.

If the results of calculations on the idealized alarm clock are encouraging, calculations relating to actual designs including lattice effects, fission bomb core, and specified implosion systems will be undertaken in November 1951.

TARGET DATE: June 1, 1952 for completion of calculations relevant to designing a test object.

PRIORITY: High if the alarm clock is given a high priority in the Fall of 1951, low if one of the other devices is given a higher priority.

PERSONNEL: Nordheim, Richtmyer, and other LASL theorists.

B. <u>Experimental</u>.

PRIORITY: High if undertaken at all.

PERSONNEL: ORNL.

2. Certain implosion-type experiments using H. E. may be required during design phases.

TARGET DATE: Fall 1952.

PRIORITY: Dependent upon results of calculations.

PERSONNEL: GMX Division assisted by CMR.

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[redacted]

3. Nuclear detonation tests as may be required for further progress.

It appears that any such test for the purpose of learning about mixing or radiation implosion would cost roughly the same theoretical and experimental effort as a test of a real alarm clock. At the present time no test other than one of a real alarm clock, although possibly one of low yield, is proposed.

TARGET DATES: Completion of alarm clock design January 1953. Test of alarm clock if it meets the various criteria, Fall 1953.

[redacted]

PRIORITY: [redacted]

PERSONNEL: T, W, CMR, GMX, and J Divisions.

IV. [redacted]

A. Theoretical.

1. General Feasibility Study.

[redacted] Crude estimates are to be made of the necessary temperatures and compressions required for propagation, the amount of tritium required, the required yield of the initiating fission bomb, the yield required from any fission bomb needed to implode the device, the yield of the device and the timing. These estimates will take into account the hydrodynamics of the expanding and burning phase, the transfer of energy out of the reacting region by radiation, the transmission of radiant energy from the bomb initiating the implosion, and the progress of the implosion. [redacted]

TARGET DATE: October 1951.

PRIORITY: A.

PERSONNEL: Teller and staff with the aid on specific problem by T Division and the Princeton group. The latter may carry a large part of the work.

[redacted]

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2. Advanced and Design Calculations.

Machine calculations, probably on the Maniac, of specific design components to be undertaken if the results of the feasibility and military worth studies place [redacted] on high priority. The time scale for this work is difficult to estimate because the conception of the device is so recent that a great deal is not known about it.

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TARGET DATE: No date specified but the theoretical design criteria are required at least nine months prior to any test of the device. Thus, for example, if the device were simple enough that it could be prepared for test in the spring of 1953, this target date would be June 1, 1952.

PERSONNEL: Teller and staff and T Division, with aid as requested on specific problems by the Princeton Group.

B. Experimental.

[redacted] it is conceivable that a test of the device could be made in the spring of 1953 if it is not attempted to carry a parallel program at the same priority on the super or alarm clock.

DOE b3

TARGET DATE: Determination as early as possible following the general feasibility study whether or not to attempt an early test. This should be done by May 1952, if a test date of spring 1953 is to be attempted.

PRIORITY: High if a decision is made to test [redacted]

PERSONNEL: T, W, CMR, GMX and J Divisions.

2. See Item II.B.2. above.

3. See Item III.B.1. above.

V. [redacted]

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DoE b(3)

PRIORITY: A.

PERSONNEL: W, CMR, and T Divisions (J Division for testing.)

Comments on the Proposed Program.

(a). Target dates for nuclear detonation tests lead to the schedule given below. These dates are mutually exclusive in the sense that each represents the earliest conceivable date for a well planned test if the priority and effort expended on the other devices is relatively small.

Spring 1953

Fall 1953 -

Spring 1954 - a super

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[redacted]

No nuclear detonation tests are planned for auxiliary purposes such as proof-testing a calculated heterocatalytic implosion scheme or as an experiment to study mixing. The reason is that such tests would cost roughly as much effort as the test of a final device and their inclusion would delay development of these devices.

(b). A greatly reduced emphasis is to be placed upon complicated experimental technicques for tests (like Dinex, the X-ray measurements, etc.) because the important experimental results desired from the proposed thermonuclear test shots are obtainable directly from the yield, although certain, possibly simple, diagnostic experiments will be desirable. Thus, the groups which performed these complicated experiments on Operation Greenhouse need not be held intact but should be built up again only when and if it becomes obvious that such measurements must be made in future tests.

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[redacted] This will be sufficient to allow a test of whichever thermonuclear system it is decided to carry to the test stage. Thus, no additional tritium production is requested at this time.

DF:b

Darol Froman
June 12, 1951

four copies typed.
rough draft.
11 pages.

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October 1, 1954

THERMONUCLEAR WEAPONS

Period 1946 to January 1950

(Draft version of a section for a history of technical work at Los Alamos since the war)

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THERMONUCLEAR WEAPONS

I. STATUS AS OF EARLY 1946

On April 18-20, 1946, there was a conference at Los Alamos on the subject of the Super. A large proportion of the persons who had been investigating the possibility of thermonuclear weapons at Los Alamos had continued work on this problem up to the time of this conference. A general description of the device then considered is given in two reports: LA-551[1] (prepared for discussion at the conference), and LA-575[2] (the conference report). The results of work up to that time are indicated or embodied in these reports, the unresolved problems as then perceived are referred to, and the requirements as they were understood at that time of further work along many different lines were listed. A similar summary of the status of the program and work yet required was given in LAMS-290[3].

The qualitative pattern of the weapon in mind at that time was quite specific.

(1) "Prima Facie Proof of the Feasibility of the Super" (April 15, 1946); written by: S. Frankel, work done by: Teller, Konopinski, Frankel, Hurwitz, Landshoff, Metropolis, Turkevich.

(2) "Report of Conference on the Super" (Issued June 12, 1946); written by Bretscher, Frankel, Froman, Metropolis, Morrison, Nordheim, Teller, Turkevich, von Neumann.

(3) "Super-Gadget Program" (October 8, 1945); Teller, Konopinski, Fermi.

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The estimates available of the behavior of the various steps and links in this sequence were rather qualitative, and open to question in detail.

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As it was, the studies of this question had merely sufficed to

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show that the problem was very difficult indeed; that the mechanisms by which energy would be created in the system and uselessly lost from it were comparable; and that because of the great complexity and variety of the processes which were important, it would require one of the most difficult and extensive mathematical analyses which had ever been contemplated to resolve the question -- with no certainty that even such an attempt could succeed in being conclusive.

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The requirements for materials, engineering developments and more detailed understanding of basic physical processes were impressive, and "would necessarily involve a considerable fraction of the resources which are likely to be devoted to work on atomic developments in the next years" (ref. (2), p. 46). An active program to realize such a device was thought at the time to require amounts of tritium beyond the reach of the Hanford plant to produce in any relevant time, so that the building of something like a 100 megawatt reactor for tritium production was probably involved. It was suggested that facilities for the production of uranium 233 and/or the separation of plutonium 239 would be desirable. The need of facilities for the production and storage on a large scale of liquid deuterium and other cryogenic engineering work

[redacted]

were pointed out; but these appeared to be relatively straightforward requirements. Laboratory experiments, measurements of cross sections, and studies of properties of materials and the behavior of fast jets[1] were necessary.

The requirements, however, which were qualitatively most difficult to meet were those involving theoretical study of the behavior of the various steps in the process.

In addition, before the properties of any actual design could be discussed, it was necessary to obtain a much more detailed picture than had yet been developed of the flow of energy from the immediate region of an exploding fission core. A successful treatment of this last problem -- which was also important for the fuller understanding of fission explosions -- itself required the results of laborious calculations on the opacity of materials at the relevant temperatures which were then being conducted by a small group which had recently moved from New York to Chicago. And, indeed, each step in the sequence posed a family of difficult problems[4].

[redacted]

[redacted]

The prospects for realizing a thermonuclear weapon along these lines were problematical. An active program to establish what might be feasible would compete at many points for the resources of effort and materials required for the immediately necessary program to improve and expand our stockpile of fission bombs, and at some points depended on advances in our understanding of fission bombs. It was against this background that it was proposed (in a letter from Bradbury to Groves; November 23, 1945) that at least for the interim period, during which the future pattern of the Los Alamos Laboratory was being considered, the work on the thermonuclear program at Los Alamos consist of: several lines of laboratory experimentation, theoretical studies as practicable conducted in active consultation with Teller, and requests for small amounts of tritium as needed for experimental purposes.

[redacted]

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II. FROM 1946 TO END OF JANUARY, 1950

A. GENERAL

Starting from the stage represented by the Super Conference, definitive progress towards obtaining or trying out a model of the type discussed required as a preliminary step a very great extension and refinement of the theoretical and quantitative considerations involved. Moreover, the possibility of opening up any radically different approach to a thermonuclear weapon also depended almost exclusively on further theoretical insight. (As late as August, 1950, in an appendix to a "Thermonuclear Status Report," prepared at Los Alamos for the GAC, Teller and Wheeler, in discussing the "Scale of Theoretical Effort," made the observation that, "The required scientific effort is clearly much larger than that needed for the first fission weapon. .... Theoretical analysis is a major bottleneck to faster progress .... .") Consequently, the account of the progress during this period will be given with primary reference to the theoretical work on problems of importance to the thermonuclear field.

Of course, some experimental studies (mainly: cross-section studies, observation of behavior of fast jets, investigation of properties of liquid hydrogen) were continued across this period and occupied, on an average, the major part of the attention of something like two of the fifty or so experimental and engineering groups in the Laboratory. Such work, however, was mainly in the nature of acquiring data which was believed would be needed in connection with any attempt to estimate the behavior of a thermonuclear system. It was unlikely of itself to

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reduce the difficulty of undertaking a theoretical estimate, nor to suggest an essentially new approach to a thermonuclear weapon. In addition, although work of an experimental and engineering kind was known to be a necessary and heavy component of any thermonuclear program, it could not rise to the high level of a full attack on the significant outstanding questions until the theoretical understanding of the processes involved in a particular system had advanced to the stage at which such questions could be isolated and clearly defined.

There was also a considerable body of theoretical work which stood in a similar relationship to thermonuclear studies. The work referred to could not be classified as distinctively 'thermonuclear,' nor was it concerned with the details of any specified thermonuclear system; but it was background work which it was recognized would have to be got in hand either before or while undertaking the detailed design of any likely type of thermonuclear system. Among such lines of necessary background theoretical work may be mentioned (i) work on opacities and equation of state of materials, (ii) great numerical refinement of the picture available of the processes occurring in a fission explosion, and (iii) advances in the general area of computational ability, both in the matter of computing equipment and also in the field of computing technique and experience. Very definite progress (some of which will be referred to below) was made along these various lines between 1946 and 1950 and helped provide by the end of 1949, a very much greater theoretical capability with respect to a thermonuclear (or any other) program than was available at Los Alamos in 1946.

 [redacted]

[redacted]

B. RESOURCES FOR THEORETICAL WORK

In this section it is proposed to discuss the growth of the Theoretical Division at Los Alamos (carrying this through to the end of 1953) since this indicates the context in which the particular studies referred to later were undertaken.

At Los Alamos, the Theoretical Division, in addition to the persons who might generally be considered to be trained or capable in some branch or branches of theoretical physics, has always included a considerable number of persons acting in some fairly well-defined supporting role -- such as computers, secretaries, assistants, computing machine operators, and others. Something like two thirds of the total personnel of the Division have normally been in this latter group. Although the conduct of any appreciable program of theoretical work is very heavily dependent on the ability and skill of the persons in this group, the content of the various studies -- their quality, soundness, and degree of novelty -- is almost totally dependent on the ability of those identified as theoreticians. The separation suggested here cannot always be made with absolute precision; but it can be drawn sufficiently closely for the purposes of the following Table, in which the total number of persons in the Theoretical Division, who by training or experience were in a position to help determine the objectives and quality of the theoretical program, is given at the end of each year from 1946 to 1953. By no means all the persons indicated were (or could properly be) ever at one time fully engaged on immediate weapons' problems, since studies similar to the

[redacted]

background type of work referred to above, as well as support of the activities of other sections of the Laboratory, not to mention the occasional partial gratification of the aspiration of everyone trained in pure science to make his own recognized contributions to advances in knowledge in those areas where he feels he has ideas to contribute, together usually occupied something like half of the attention of the group.

TABLE

End of:	1946	1947	1948	1949	1950	1951	1952	1953
Number:								
Los Alamos Staff	8	12	14	22	35	45	45	51
Full Time Consultants at Los Alamos (See Below)	-	-	-	2	3	1	1	-
At Matterhorn (See Below)	-	-	-	-	-	6	10	-

In addition to those holding a 'permanent' appointment to the staff at Los Alamos, the following groups should be mentioned:

(i) Consultants. Ever since the war the theoretical program at Los Alamos has benefited greatly by being able to obtain the services of a large number of able and distinguished consultants. This, for the most part, has been in the form of the consultant working with and among

[redacted]

[redacted]

the regular staff for an extended period of from a few weeks to three months or so during the summer, sometimes with an additional period of a few weeks in December or January, and usually coupled with brief visits at other times either of the consultant to Los Alamos or of Los Alamos persons to the consultant. The exact pattern has, of course, varied between various individuals and from year to year with each individual. To give an indication of the quite impressive assistance obtained in this way, since the time of the Super Conference, the following notable instances are cited (though it would be easy to extend this list):

H. A. Bethe: brief visits 1946, 1947, 1948; about two months each year, 1949, 1940, 1951; about eight months 1952, and three months in 1953.

E. Fermi: visited each year from 1946 to 1953 except 1949; about six weeks per year on the average (between two and ten weeks each year) for these years.

G. Gamow: about twelve months between June, 1949, and September, 1950.

F. Hoyt: eight months between July, 1946, and January, 1948; brief visits January, 1948, to December, 1949. Joined Los Alamos staff in July, 1950.

E. Konopinski: three weeks in 1946; four months in 1950; three months in 1951.

[redacted]

L. Nordheim: one month in 1947; brief visits in early 1949 and 1950; full time September, 1950 - September, 1952.

E. Teller: nine months between July, 1946, and June, 1949; full time from July, 1949, to October, 1951.

J. von Neumann: two months per year on the average (between one and three months each year) from July, 1946, to December, 1953.

J. A. Wheeler: full time from March, 1950, to June, 1951; after which continued to be heavily engaged in the program through Project Matterhorn until March, 1953. Two months at Los Alamos, July - August, 1953.

(ii) Project Matterhorn. In July, 1951, J. A. Wheeler established and directed at Princeton a group known as Project Matterhorn to engage in the program of theoretical studies of thermonuclear weapons in the form then being considered. This group worked in collaboration with the work at Los Alamos. After the formation of the Livermore Laboratory, it made plans to discontinue its operation and the contract was formally terminated on March 1, 1953. Several members of the Project continued work on the terminal and summary reports of Matterhorn work into the summer of 1953. (The present Project Matterhorn, working under L. Spitzer on the problem of controlled thermonuclear reactions, began its work about the same time and was originally called Division S of Project Matterhorn. It was operated under direct contract with the AEC and continued administratively unaffected by the termination of the group engaged on weapons studies.)

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(iii) Opacity Group at Argonne Laboratory. The war-time group working at Columbia under the direction of Maria Mayer on the subject of opacity of materials transferred its operation to Chicago at the beginning of 1946, and continued as a Los Alamos-sponsored program in the Argonne Laboratory from then until 1952. In addition to continuing occasional assistance from Teller and M. Mayer, this program occupied on the average the attention of about two persons (on the scale of the Table above). At least until the advent of modern computing equipment, numerical calculation of opacity values was an enormously tedious undertaking and it was extremely difficult to arouse, and particularly to sustain, the interest of capable persons in this program. This program of study, which is yet by no means complete, was contracted to the Rand Corporation in the middle of 1953, where a considerably larger group is attacking the problem with the aid of modern high speed computing equipment.

(iv) Group at Yale. In March, 1950, arrangements were made to have Gregory Breit direct the part-time work of four or five senior graduate students at Yale in the study of some of the basic interactions between nuclei, electrons, and radiation.

Studies of this general type (required in connection with improved calculations on thermonuclear weapons; but not in themselves involving weapon design data) have been continued under Breit's direction through to the present. Incidentally, among the problems to which Breit

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has given considerable attention under this arrangement, has been that of checking, refining and extending the considerations first applied by Teller and Konopinski in LA-602 (August, 1946) to the question of whether or not the concentrations of energy provided by possible thermonuclear explosions would threaten to ignite the earth's atmosphere or the sea. Such consideration has, of course, continued to show that such ignition is probably impossible and that, even if possible, there are a considerable number of orders of magnitude lacking between anything yet contemplated and the conditions which might be required.

One final comment on the subject of the Table seems appropriate. Of the eight theorists at Los Alamos, at the end of 1946, three had joined the staff after July 1, 1946, and only one (Landshoff) had been preoccupied with work on the thermonuclear program before the time of the Super Conference. By late 1946, however, three of these eight were chiefly engaged on specifically thermonuclear studies.

C. BRIEF CHRONOLOGICAL ACCOUNT

A partial calendar, with notations, is given below to provide a picture of the time sequence of the developments discussed. This "calendar" is largely abstracted from the monthly progress reports of the Theoretical Division during this period, and reference is given only to items which appear to have had a continuing significance in relation to the thermonuclear field. There was, in addition, of course, a large body of theoretical work involved in connection with the developments in the fission weapon field described elsewhere in this account.

(In the following section, the progress of work along a number of specified lines will be traced across this period, and some of the items merely referred to in the present listing will be discussed further.)

May-September, 1946: All the individuals engaged on the studies and calculations discussed at the Conference wind up work underway at that time and prepare final reports, with the exception of Landshoff, who remains at Los Alamos and continues studies of the properties of detonation waves in pure deuterium. (Landshoff remained at this problem until summer, 1947.) From mid July to end of September, Hoyt works on same problem.

September, 1946: Teller proposed the system called the "Alarm Clock," and Richtmyer takes up problem of estimating performance.

October, 1946: Evans takes up study of detonation in deuterium.

November, 1946: LA-610, first Alarm Clock report issued by Richtmyer. Report contains arguments of feasibility in principle, and rough estimates of efficiency and behavior.

December, 1946-January, 1947: Landshoff, Mark and Richtmyer make estimates (embodied in LAMS-560) for use of deuterium (or deuterium and tritium) placed close to the core in a fission bomb test to check predicted features of thermonuclear burning.

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January-February, 1947: Richtmyer starts to develop an improved theory of efficiency of Alarm Clock. Discussion with Teller and von Neumann of possible application of advanced electronic computing equipment (then in early stage of design at Princeton) to Los Alamos problems.

February-March, 1947: Studies of equation of state and related problems, pertaining to thermonuclear as well as fission devices, reactivated at Los Alamos as H. Mayer joins staff. "Monte Carlo" method of computation proposed by Ulam, and LAMS-551, outlining prescription for application of method, prepared by von Neumann, with additional suggestions by Richtmyer.

March-April, 1947: With Teller, planned program for summer, 1947, primary objectives being to: continue studies of detonation in deuterium and Alarm Clock, develop Monte Carlo method, initiate work on obtaining method of calculation of radiation flow in exploding fission bomb, study the proposed experiment to check ideas on thermonuclear burning, and prepare status report on thermonuclear systems. (This last resulted in LA-643, September, 1947.)

May-June, 1947: LA-636: "Improved Theory of the Alarm Clock," issued by Richtmyer.

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July, 1947: Nordheim joined Richtmyer in study of Alarm Clock.

August, 1947: Efficiency calculations for a number of possible Alarm Clock configurations completed with the scheme worked out in LA-636. Landshoff takes up work on radiation flow in fission bomb.

September, 1947: Further Alarm Clock examples calculated (LAMS-625). LA-643[5] issued.

(As the thermonuclear principles involved in the Booster were not essentially different from those embodied in LAMS-560, consideration of the Booster soon superseded further consideration of the LAMS-560 type of proposal.)

October, 1947: Richtmyer starts to plan a fully-detailed machine calculation of the course of a fission explosion. (This turned out to be a two year program, and the first example was actually calculated only early in 1950.)

December, 1947: Work started separately by Landshoff et al. on simpler and, hopefully, faster fission explosion calculation. (Since Richtmyer's problem came to be known as

[5] LA-643: "On the Development of Thermonuclear Bombs." (September 30, 1947.) Written by: E. Teller; Word done by: F. Evans, F. Hoyt, R. Landshoff, M. Mayer, L. Nordheim, R. Richtmyer, E. Teller, E. Zadina.

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"Hippo," the work by Landshoff was known as "Baby Hippo.") Preliminary consideration given to preparing a detailed calculation of the propagation of burning in deuterium for handling on the electronic computer expected to be completed at Princeton within a couple of years.

January, 1948:

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(From January through April a considerable amount of effort was required in connection with preparations for the Sandstone tests and consideration of results.)

February, 1948: First automatic machine calculation of Monte Carlo type prepared for handling on the ENIAC. (Monte Carlo calculation techniques were expected to be required in the detailed calculation of deuterium burning, as well as other types of problems.)

March, 1948: Richtmyer (and von Neumann) introduce so-called "viscosity treatment" of shocks, LA-671. (This technique, which was devised to meet needs arising in connection with Hippo, reduced the problem of calculating

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the progress of shock fronts in explosion (and implosion) calculations to manageable proportions on automatic computing machines, and was of profound value in very many of the calculations undertaken subsequently.)

July, 1948: Detailed study begun of behavior of a Booster system (considered either as a test of thermonuclear principles or a possible weapon). Work begun on equation of state of paraffin (wanted in connection with possible experimental gadgets to test ideas in the thermonuclear field).

August, 1948: Study of the scattering of neutrons by light elements to obtain data required in connection with various calculations (Booster, hydrides, and deuterium burning).

September-October, 1948: LA-704, [redacted] Also LA-713, "Further Booster Calculations." These were the first detailed studies relevant to the proposal to test such a device in the tests then scheduled for 1951. [redacted] From this point on, the planning

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and preparation of this calculation, which naturally came to be known as the Super problem, was continually kept in view, with the objective of having it ready by the time the computer at Princeton should be ready to accept it. Dr.and Mrs. Evans were mainly responsible for the preparation and eventual execution of the Super problem. In this tremendous undertaking, they had, of course, the benefit of suggestions and assistance from many persons on many aspects and details of the work. In particular, they relied on the continuous and pervading interest of von Neumann, who advised on almost every detail in the problem. As it turned out, the completion of the machine was much later than had been expected in September, 1948, and it was not in shape to take this problem until about the end of 1952. The first two examples were calculated in Princeton between February and July of 1953.)

November-December, 1948: [redacted]

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[redacted]

LAMS-831, outlining such a calculation, prepared by Evans, von Neumann and Ulam.

January, 1949: Metropolis authorized to proceed to form group to build an electronic computer at Los Alamos along same general lines as computer at Princeton. (Actual work on the machine was under way by spring, 1949, and the computer began effective operation in the spring of 1952.)

January-June, 1949: Work continued on many of the problems mentioned above; in particular: Hippo and Baby Hippo, the detailed preparation of the Super burning problem, and various features of the Booster.

July, 1949: Work started on equation of state for hydrogen, required in connection with the Booster and hydrides.

August, 1949: Several calculations concerning details of behavior of D-T mixture in Booster begun.

September, 1949: Worries raised about possible damaging effects on Booster performance of mixing between uranium and D-T.

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Bethe and others study problem of determining progress of mixing with intention to use results to guide Booster design.

October, 1949: Work begun in July on equation of state for hydrogen is completed[6], and data made available for implosion calculation of Booster design.

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Baby Hippo calculation reaches stage at which it begins to give results. These are of interest both in connection with preparation of Hippo and details required in consideration of Booster.

November, 1949: Implosion calculation of proposed Booster model gives disappointing results, indicating need of seeking improved design.

Many discussions by Teller et al. of details connected with the Super problem.

[6] LA-1094: "The Equation of State of Hydrogen;" written by J. Reitz
Work done by: Bethe, Longmire, M. Mayer, Reitz, M. Rosenbluth, Sternheimer, Teller.

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December, 1949: Preparations of the calculation of the Super problem far enough advanced that remaining detailed work can be completed much faster than the computing machine required to handle the problem. Work on problem preparation consequently suspended until such time as machine more nearly available.

Detailed work started on preparation of a machine calculation of the proposal of LAMS-831 to study the ignition of a sphere of deuterium. Simplified hand calculation of same problem begun by Ulam and Everett to provide information sooner, even though this information would be less precise.

[redacted] Discussion started of particular designs to be computed. DOE b3

First model of IBM Company's CPC delivered to Los Alamos. (This machine represented an enormous advance in capacity, flexibility and speed over any computing equipment available at Los Alamos up to this time. It required, of course, several months to obtain experience needed to make full use of its capabilities.) DOE b3

Consideration of controlling time of initiation of Booster indicate ways to relieve difficulties met in November.

January, 1950: [redacted]

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Baby Hippo calculation reaches stage about half way through explosion. Hippo calculation almost ready to start in New York.

Spherical Super machine calculation trimmed to fit on the ENIAC, with plan to prepare for first calculation during spring 1950. First example of hand calculation continued, with results expected before the end of February.

H. Mayer completes "The Super Pocketbook" (LAMS-1066), chiefly a summary of two lectures delivered by Teller to the Technical Council of the Laboratory a couple of months previously. The report outlines principles of the Super and gives basic formulae and up-to-date physical data, as well as estimates of damage from an assumed 40 megaton Super.

(January 31, 1950: President Truman's announcement concerning work on thermonuclear weapons.)

D. SUMMARY OF PROGRESS ON SOME PARTICULAR PROBLEMS

In this section it is intended to identify the more significant problems or programs considered, indicate the progress made in the period 1946-1949, and describe the stage reached by the end of January, 1950.

i) Calculation of Details of Fission Explosion. The need of such calculation was clearly stated in the Super Conference Reports of 1946.

In a "Program for the Theoretical Division," drawn up by Fermi, Richtmyer and Teller in August, 1946, this problem is put in the foremost position as being necessary to provide the basis for improving fission bomb designs, [redacted] The advent of the Alarm Clock in September, 1946, placed an additional, and even more specific, emphasis on the need for detailed understanding of the processes involved in a fission explosion.

Starting in the late summer and fall of 1947, two major calculations were undertaken on this problem. These calculations came to be known as "Hippo" and "Baby Hippo." Baby Hippo was conducted by Landshoff, and relied on the computing facilities at Los Alamos -- which at the time consisted of a group of computers using desk calculators and another group using the IBM equipment then available. Hippo was conducted by Richtmyer, with the intention of making as effective use as possible of advanced computing equipment. It was expected that Hippo would require about a year to prepare (since much new ground in mathematical computing technique would have to be broken), but when ready to run would go much faster than the other calculation and provide much more detailed results. Baby Hippo, on the other hand, would get started sooner, probably give some results faster, but most particularly give a foretaste of the nature of the difficulties not foreseen at the start which could help guide the planning of Hippo. Things happened pretty much as expected except that each approach was about twice as hard as imagined at the start and

[redacted]

required twice as long to accomplish. In January, 1950, Baby Hippo had given a picture of the events in the core and tamper of the Trinity bomb up to about half way through the explosion. Early in February, 1950, Hippo was checked out on the IBM Company's SSEC in New York and Baby Hippo was discontinued. (By June, 1950, two Hippo problems were completed, and details of behavior provided by these were used as guides for estimates required in studying designs of experiments proposed for Greenhouse.)

ii) Calculations on the Super. [redacted] In all of these studies some of the relevant effects were neglected so as to allow analytical, or simple numerical, treatment. It then became clear that only a full-scale treatment in which all the important processes were simultaneously taken into account could give significant information. This could only be approached by an elaborate numerical calculation of a magnitude which would obviously tax the resources of the most advanced machines then being designed. No adequate machine was expected to be operating in less than a couple of years from that time, and in fact it was over four years before the first of these appeared. In the meantime, work continued on preparing the Super problem so as to be able to take advantage of the machines as soon as they became available. By January, 1950, these preparations were in a stand-by status, still waiting for the difficulties in machine building to be overcome.

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In December, 1949, detailed work was started (chiefly by Calkin, Dr. and Mrs. Evans, Dr. and Mrs. von Neumann) on the preparation of a machine calculation which was expected to require about six months to get ready. (This calculation was started on the ENIAC at the beginning of June, 1950, and continued into the summer.) In December, 1949, also, Ulam and Everett started a simplified version of this calculation by hand. This would give less detailed results, but give them sooner, and the difficulties encountered would provide guidance in the preparation of the machine version. Lacking calculations of this type, estimates of the amount of tritium required to provide possible ignition conditions for a charge of deuterium were necessarily on a somewhat subjective basis.

By the end of January, 1950, this first calculation was about half complete.

(iii) Alarm Clock Studies. The Alarm Clock system was proposed by Teller in September, 1946.

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This provides more heat to continue the process.

Work started immediately to obtain estimates of ignition conditions and available efficiencies. In LA-610[7], issued November 15, 1946, the conclusion is given, [redacted]

[redacted] Measurements of relevant neutron reaction cross sections were undertaken, and improvements in the theoretical treatment were developed (LA-636, June 26, 1947) to enable specific

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[7] LA-610: "A New Thermonuclear System;" written by Richtmyer, work done by Richtmyer, Teller.

[redacted]

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calculations of assumed models to be made. These would indicate the size of system required to produce a stated yield, and, of more immediate interest, allow one to determine the size of the explosion required to get the reaction well started.

By the end of September, 1947, calculations had been made on several models. The results are discussed by Teller in LA-643[5].

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In proposing a program of research and development, Teller suggested a number of cross section measurements; bomb tests of the sort described in LAMS-560; an attempt to make use of high speed computing equipment when it should be available (then expected to be about two years off) to improve the calculations of Alarm Clock behavior; and continued (LA-643, p. 37), "I think that the decision whether considerable effort is to be put on the development of the Alarm Clock or the Super should be postponed for approximately two years; namely, until such time as these experiments, tests, and calculations have been carried out."

After September, 1947, in consideration of the enormous difficulties of igniting an Alarm Clock system of the type considered, or of achieving a practically useful object by any means then envisaged, further study of the Alarm Clock was soon laid aside. One of the persons, for instance,

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who through most of the preceding year had participated in studies related to the Alarm Clock, turned his attention to work required in connection with Operation Sandstone. As mentioned earlier, Richtmyer, who had conducted the detailed study of the Alarm Clock, took up the problem of obtaining a realistic calculation of the behavior of a fission explosion. Among other things, experience with such calculations was a prerequisite to the improved calculations of Alarm Clock behavior referred to by Teller in LA-643.

At the end of January, 1950, therefore, the understanding and prospects of the Alarm Clock were in essentially the same state as indicated above. Systems of this kind were believed to be feasible in principle, and capable of providing arbitrarily large yields.

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(iv) The Booster. The Booster, or the boosting principle, refers to the notion of a fission bomb whose efficiency is increased by neutrons obtained from the thermonuclear D-T reaction, the D-T being in or close to the exploding core and heated to the point at which the D-T burns at some sufficiently early stage of the explosion.

Possibilities of this general type were recognized at least as early as November, 1945, when they were included in a patent application filed at Los Alamos. The designation "Booster" only became general after its use in LA-643 in September, 1947.

(A note on the Hydride: The neutronic properties of an exploding uranium hydride assembly had never been calculated properly at that time (September, 1947). This was a very difficult undertaking, with the techniques, experience and equipment then available. Starting early in 1948, efforts were made to obtain realistic calculations of the time-constant (alpha) of an unboosted hydride device by use of the Monte Carlo method on the ENIAC. From some time in 1948 up to the end of January, 1950, a model of a Hydride was considered to be one of the objects to be tested in the 1951 Pacific test operation. A very considerable amount of calculation was devoted to the problem of determining a favorable design

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and estimating its performance. This study derived a large part of its impetus from Teller's deep and persistent interest in this type of system, which seemed in principle to offer the possibility of obtaining at least a small explosion from a smaller amount of active material than would explode at all if used in the form of metal. This possibility, and the possibility of combining this feature with some use of the boosting principle, made it attractive to explore the situation in detail. By the end of 1949, however, the results available were distinctly disappointing and the Hydride was dropped from the immediate Los Alamos program in February, 1950, to permit the substitution of one or more experiments involving thermonuclear principles in addition to, or in place of, the test of a Booster which was already scheduled. The properties of hydride systems continued to receive sporadic consideration from individuals at Los Alamos, without anything of sufficient promise to require immediate attention being uncovered. The study was only taken up again in a concerted way by the Livermore Laboratory in 1952. It was still a difficult problem.)

In the summer of 1948 a detailed study was begun of the application of boosting to a device of the general nature of the Sandstone Zebra model. Such a proposal, it was believed, would lead to a relatively clean experiment on the progress of the thermonuclear burning and the efficacy of the boosting process since the fission part of the system would not be too different from systems already tested. A full scale test of the model which would ultimately result from this program of study was put on the list of shots to be made in the next overseas test operation which was planned to be held in 1951.

These studies, which were in general directed by Teller, were carried out in their first stages by Rosenbluth and Reitz. By the fall of 1948, a number of points had been checked (LA-704, LA-713):

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Study of the Booster was continued through 1949 and, starting early in the summer, was greatly intensified. A number of unanticipated problems were turned up, and overcome:

A large number of people necessarily became involved in obtaining the information required for the many different aspects of the study of the Booster: Landshoff, because of his experience with Baby Hippo, to calculate the transfer of heat from the fissile materials to the D-T; Evans, on the ignition and progress of the D-T burning; Reitz and others[(6)], on the equation of state of hydrogen, expected to be needed in connection with the implosion calculation; the members of the "implosion group," under Hammer, to calculate the progress of the implosion;

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the various persons who had experience with the neutronics calculations required for standard fission bombs, to provide data needed to resolve the timing question mentioned above; Bethe, Longmire, and others, to devise methods of obtaining estimates of the probable progress of mixing and to make these estimates; many others outside the Theoretical Division, to measure cross sections and other quantities required for the calculations, and to solve the mechanical problems involved in handling D-T under very high pressures. Almost all of these aspects of the problem had been taken up before the time of the first Russian test in September, 1949. By about the end of January, 1950, this work was far enough advanced to allow the choice of a model for which each step, starting with the HE implosion through to the end, was to be calculated. This chain of calculations was expected to be completed sometime during the summer of 1950, and at that time, provided no major surprises were encountered, it was hoped to freeze the fine details of the design. (In the event, things proceeded very much in this fashion except that it took a little longer than expected. The last details of the design for the Greenhouse Item Booster model were frozen late in October, 1950.)

v) Calculational Requirements. Already during the war, the Theoretical Division at Los Alamos had been forced to make very heavy use of extensive numerical calculation. There was a large group of computers using desk calculators and there was an installation of IBM accounting equipment, which was being run twenty four hours a day

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calculating implosion problems. At that time, this last was probably the largest and most complex calculation being handled on a routine basis anywhere. This computing effort, which was considered very large in those days, was required for the problems arising in connection with the design of the first fission bombs and with rather schematic calculations of the explosion of those devices. As mentioned above, a detailed calculation of the progress of a fission explosion (Baby Hippo) using these computing resources required many many months to complete, even with the use of a number of severely simplifying assumptions. To calculate this problem in noticeably more realistic (though still far from complete) detail was probably physically, and certainly psychologically, impossible without the aid of computing devices such as the (now obsolete) SSEC which only began to appear about 1948.

It was recognized very early that theoretical work on thermonuclear systems would, for comparable realism, require enormously more arithmetical labor than had the design of fission weapons, and that it would also be necessary to rely much more heavily on the information obtained by dead reckoning. This made itself evident in many ways. The first step in any thermonuclear explosion system yet considered is a fission explosion. Somewhere in the middle of its history, it provides the heat required to induce the thermonuclear burning; that is, the starting conditions for an estimate of thermonuclear behavior require a picture of the state of things in an advanced stage of a fission explosion,

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which picture can itself only be obtained by calculation such as Hippo or Baby Hippo. Thus, all the calculation normally required for design of a fission gadget, plus more advanced calculations not absolutely required, simply bring one to the start of an estimate of the behavior of a thermonuclear system. No analogue of the experimental checks which were available with respect to fission weapons, such as critical mass measurements, nor the techniques used to explore the progress of an implosion, such as measurements of detonation velocity in high explosive, pin-shot studies, or RaLa measurements can be brought to bear in this field short of almost impossible measurements on a full scale nuclear detonation. Carrying on from there, the processes involved in the progress of any thermonuclear reaction are in all respects at least as complicated as those in a fission device -- involving the interplay of hydrodynamic motions, transport of energy by heat flow and other processes, and neutronics -- and the variety of the details of thermonuclear reactions is in many respects much more complicated than the details which have to be taken into account in connection with the fission reaction in estimating the properties of an explosion. In the case of the 'classical Super,' these latter considerations are orders of magnitude more severe than in thermonuclear systems such as the booster or Alarm Clock or even the present day two-stage devices.

The final major indication of the qualitative shift of emphasis towards calculation in going from fission to thermonuclear studies is the following. In the case of a fission explosion, a modest number of

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experimental facts which could be determined in the laboratory (and had mostly been roughly ascertained before the Manhattan District was formed), along with rather elementary theoretical considerations, sufficed to show that a fission explosion was feasible. The major part of the war-time theoretical work at Los Alamos was required to ascertain the details of a favorable design, the mechanics of its assembly, and estimates of its performance. With respect to the classical Super in particular, the very proof of feasibility (since it turned out to be marginal) required the fully detailed calculation of its behavior during an explosion. Without this, no conclusive experiment was possible short of a successful stab in the dark; since a failure would not necessarily establish unfeasibility, but possibly only that the deuterium system chosen was too small, or too large, or that the required ignition conditions had not been met. (Since by now, it is very strongly indicated -- particularly due to the studies made in 1953 -- though not absolutely shown, that the classical Super as envisaged in 1946 will not work, it is almost certain that any early experimental test of such a device would have resulted in failure and probably have lead to premature abandonment of the field.)

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[redacted] These were conducted on the ENIAC, the most advanced computing machine in the country at that time and, indeed, until about 1948. To trim the calculation to the capabilities of the machine as it then was, a number of effects were omitted. [redacted] The results of the calculation were promising; but chiefly because of having ignored these effects, any one of which would have overloaded the calculation with respect to that machine.

Between the time of that first ENIAC calculation and the present, there has been a major revolution in the facilities and technique of computing. No qualitative change from the war-time situation in the resources available for Los Alamos work occurred until early in 1948, at which time Metropolis, of the Los Alamos staff, supervised changes on the ENIAC at Aberdeen Proving Ground which transformed it from a somewhat inflexible machine to one of the modern type, capable of handling a long series of coded instructions without the need of physical adjustments on the machine to take account of each separate step. By modern standards, the ENIAC was of very limited capacity. The SSEC (IBM in New York City) appeared the same year; but it was somewhat slow and cumbersome. The SEAC (Bureau of Standards, Washington, D. C.), a couple of

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years later. The UNIVAC, in 1951. The Los Alamos MANIAC, and the Princeton machine in 1952. The IBM 701, in 1953. Los Alamos problems were put on all these machines soon after they became effective. At the present time, the major computing equipment at Los Alamos consists of the MANIAC and two 701 machines each running from eighteen to twenty four hours a day.

The effect of this revolution can be indicated in several ways. For example, it has been estimated that in the course of running the Super problem at Princeton in 1953, which involved about three or four months of effective computing time for eight hours a day, the number of basic arithmetic operations (multiplications, additions, and so forth) performed were of the same order of magnitude as the total number of such operations performed at Los Alamos (excluding the arithmetic done on the Los Alamos MANIAC) from its beginning in 1943 up to that time. A similar indication is given in the following Table. There, the times required to compute an example of the implosion problem are indicated at various periods. This problem, though improved in many respects and adapted to conform to the requirements of the machines used, is basically the same as it was in 1945 in that it is a calculation of the same physical process, although in rather more detail now than then. (It should be noted that this calculation is quite similar to <u>one</u> of the basic calculations required in connection with the design of a two-stage assembly device.) The Table indicates the 'elapsed time' (time from deciding to calculate a particular example until the results are available), the 'personnel time' (total man-months, etc., required to prepare and

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handle the problem) and the equipment used. The change between 1945 and 1947 reflects improvements in technique of handling the problem, and not improvements in equipment.

Date	Equipment	Elapsed Time	Personnel Time
1945	IBM 601	3 months	9 months
1947	IBM 601	2 months	5 months
1950	IBM 602	1-1/2 months	2 months
1950	IBM CPC	3 weeks	1 month
1952	MANIAC	2 days	2-3 days
1954	IBM 701	1-2 days	2-3 days

All through the period from 1946 to 1950, the phrase 'when high speed computing machinery becomes available' keeps reappearing in reports, usually in connection with thermonuclear problems. By the time (1951) when the present thermonuclear program began to emerge, the log-jam in computing resources was rapidly breaking. There was a period in 1952 when the Los Alamos MANIAC, a model of the UNIVAC in Philadelphia and the SEAC in Washington were all engaged essentially full-time on Los Alamos (and Matterhorn) calculations for the new thermonuclear program.

The essential points in this matter of computing requirements are that: thermonuclear studies required undertaking many more calculation and much more complicated calculations than had been attempted in 1945 or could be sensibly handled with the equipment then available; and, due to the revolution in computing facilities which began to become effective

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around 1950, the proposition of undertaking any particular complicated calculation has radically changed in character, in some cases to the extent of being possible rather than impossible, and in all cases to being manageable in a time of the order of 10 to 100 times less than before the appearance of these machines. The calculations made in connection with the design of the Mike shot were essentially all made in the year between mid-1951 and mid-1952. With the computing resources available a couple of years before, it would have been impossible to compress the same amount of work into anything like as short a period.

It cannot be said that the present thermonuclear program could not possibly have been handled without the revolution in computing equipment, or before the revolution. It is clear, however, that it would have required many more years than it did to accomplish the same progress.

vi) Effort. If one omits the work on Hippo and Baby Hippo (whose results were used at least as much in connection with thermonuclear considerations as others), there was about as much time devoted in the Theoretical Division during this period to studies of thermonuclear problems as to studies of fission weapons. This situation did not apply to the other major Divisions of the Laboratory, with the possible exception of the Experimental Physics (P) Division. The other Divisions had established programs underway in connection with fission weapons and there were not, until the Booster began to take shape, specific objects proposed in the thermonuclear field requiring engineering or hydrodynamic

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[redacted]

studies or development of processes or techniques. Some work related to the thermonuclear field (the cryogenic group in CMR, the work on jets in GMX, for example) did proceed; but for the reasons indicated, it was a small fraction of the work of the larger Divisions of the Laboratory. Indeed, it was felt by sections of the Laboratory which had fission weapon work to accomplish but did not yet have work in connection with thermonuclear devices, that they were not receiving as much detailed assistance from the Theoretical Division as they needed; so that in the fall of 1948 a new group was formed in W-Division specifically to carry out detailed analysis of problems arising in fission weapon engineering.

As to the individuals on the Los Alamos staff who contributed, many names have been indicated in previous sections in connection with particular studies. This list is by no means exhaustive. In particular, all the members of the computing group directed by Carlson have at one time or another been involved in executing calculations referred to. Their time would naturally be divided between various programs roughly in the same proportion as the time of the theoreticians proper.

With respect to consultants, some -- Hoyt and Nordheim, in particular -- worked only on thermonuclear problems. Others -- Fermi and Bethe, for example -- took an interest in any and every thing, fission or thermonuclear, that came to their attention. von Neumann, also, followed many different problems; but partly because of his great interest in advanced computing techniques, he gave most of his attention to problems where the computing difficulties were severe. This naturally meant that he was called on in connection with nearly every thermonuclear

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[redacted]

investigation undertaken, at least in this period. His contributions to this work were direct and of enormous value, and, indeed, at many points may be said to have made it possible to undertake the calculations required at the time they were done.

Finally, very special mention must be made of Teller's contributions to this work. Although he took a direct interest in every aspect of the program, fission as well as thermonuclear, his most distinctive influence was in the thermonuclear field. He discussed nearly every physical detail of almost every problem undertaken. He proposed many, though not all, of the problems. He called attention to possibilities. He resolved difficulties, elucidated complicated phenomena. His speculations induced speculation in others. The main thermonuclear studies would have continued even had he not kept in touch with them, not only because the theoretical problems themselves were so challenging and interesting that people simply couldn't leave them alone, but also because there was never a time at which the moral responsibility of determining whether or not the Super was feasible was not strongly felt at Los Alamos. However, they would probably have proceeded with less ingenuity, and possibly at a slower pace, without the benefit of his keen physical insight and contagious enthusiasm.

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IN REPLY REFER TO: AN-43

March 11, 1958

LOS ALAMOS
01954936

Dr. H. A. Bethe
P.O. Box 451
Ithaca,
New York

Subject: DETECTION OF VIOLATIONS OF A NUCLEAR TEST MORATORIUM. (u)

[redacted] I have read the excellent summary [redacted] and have the following comments to offer. b(1)

The Ad Hoc Panel on Nuclear Test Limitation is in a good position to evaluate the chances of detecting violations carried out inside the USSR. [redacted] b(1) The possibilities for evading detection are large and each one would need thorough analysis if the Ad Hoc Panel were to make any conclusive statement as to what measures would insure good chances of detection. b(1)

For violations occurring inside the USSR I would conclude that surface and air bursts involve too great a risk of detection to be attractive, but that possible sub-surface violations would require the use of inspection teams. The investigation of 100 events per year (for the USSR) might be feasible. [redacted] b(1) The use of inspection teams does not guarantee discovery of violations, of course, but the increased risk of detection seems great enough so that I would doubt that Russia would allow inspection teams and then attempt sub-surface shots within the USSR.

[redacted] b(1)

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b(1)

In my ignorance I must conclude only that successful violations probably could be carried out, that some detection systems would (at a cost unknown to me) provide some unknown risk to the violator, and that the risk of detection increases with the number of successive violations, so that one violation may well be carried out without getting caught but a series of violations is much riskier.

b(3)

R. W. Spence
Alternate N Division Leader

RWS:vjh

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ESTIMATE OF USSR AND USA CAPABILITIES OF WEAPONS DEVELOPMENT (U)

H. A. Bethe

LOS ALAMOS
01954933

Introduction

I was asked to write a report on this subject, without being given specific instructions on the terms of reference; therefore, I am using my own judgment in defining the subject. In my opinion it would be misleading to confine the estimate to USSR weapons development capabilities in the case of a moratorium. To put these capabilities in perspective, it is necessary to compare them with development possibilities under unlimited testing, and also to compare USA and USSR capabilities. Furthermore, it seems to me important to begin with an assessment of the present status of weapons development in the two countries.

DOE (b)(3)

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[redacted]

There are thus considerable uncertainties in our assessments of the present USSR capability. There are far greater uncertainties in estimating Russian plans for future testing, either legal or clandestine. In my opinion the only reasonable way to proceed is to make the most likely extrapolation of past performance and past interest of the USSR, rather than to assume that they will try to do everything we can imagine.

This report will be divided according to various yield classes. The present status and the likely future development is very different in these various classes.

Various possibilities should be considered regarding the testing. One possibility is unlimited testing, another is testing limited to underground and outer space but not limited in yield. When I have made estimates of time required for development I have always first considered unlimited testing, and have assumed that it would take the USSR about the same time to make a given amount of progress. If testing is confined to underground and space but not limited in size, the times have to be increased, probably by about two years for weapons which can be tested underground, and more when space testing is required.

The third possibility is the Eisenhower proposal of 11 February 1960 according to which all atmospheric and underwater tests would be prohibited, while underground tests would be allowed as long as they cause an earth disturbance equivalent to an earthquake of less than magnitude 4.75. [redacted] The Eisenhower proposal would also prohibit tests in outer space which can be detected from a ground-based detection system. [redacted] Accurate numbers must await further investigation of ground observation possibilities.

DOE b(1)

DOE b(1)

The fourth alternative is the Eisenhower proposal plus a moratorium on smaller tests, both underground and in space, for a limited time (2-5 years). This is the Russian modification of the Eisenhower proposal. [redacted] the Russian delegate, Mr. Tsarapkin, has proposed that smaller seismic signals observed by the detection system could be subjected to an on-site inspection. If the source of the signal were in the USSR, the decision whether to inspect would presumably rest with the West, but the total number of inspections would be limited by a quota (McMillan proposal). Thus it would be reasonably hazardous for the contracting parties to cheat on the moratorium, except if they use effective decoupling schemes (the Latter hole).

DOE b(1)

I shall refer in the following to possible development of weapons under any one of these four alternative testing schemes.

DOE b(3)

A. Large Thermonuclear Weapons [redacted]

1. Status

[redacted]

DOE b(3)

DOE b(3)

2. Military Uses

Very large H-bombs, 10,000 lbs and more, can be carried by planes. Because of the development of rocket defenses and fighter planes, direct bombing missions by planes will probably decrease in importance in the future. Planes may continue to carry out missions by means of missiles fired from the plane. In any case the importance of the very heavy warheads will greatly diminish.

Large missiles are available, both intercontinental and intermediate range. The Russians' missiles probably can carry very large warheads; [redacted] DOE b(3)

In fact our estimates of the carrying capacity of Russian ICBM's has been based largely on the estimated weight of their largest warhead, which is rather a circular argument.

[redacted] DO b(3 DO b(3 DO b(3

Moreover, it does not seem to me a reasonable military aim to destroy hard enemy missile sites if the enemy

[redacted]

3. Development Possibilities

a. USA. [redacted]

DOE b(3)

DOE b(3)

[redacted]

DOE b3

4. Test Requirements

[redacted]

DOE b(3)

B. Small Thermonuclear Weapons [redacted] b(3)

1. Status

[redacted]

DOE b(3)

2. Military Uses

This class of weapon, in my opinion, is the most important of all. The weapons may be used as warheads for mobile missiles, either of the Polaris or the Minuteman type. Both of these missiles use warheads [redacted] Mobile missiles, either on rails or underwater, constitute the best safe retaliatory power we have been able to conceive. Such a capability is especially important for us, making us reasonably safe against a surprise attack by the USSR. However, in my opinion, the USSR also must plan for a safe retaliatory capability because they cannot know in what manner war might start. With possible improvements in the aiming accuracy of missiles, and with the possibilities of aerial survey from satellites, even hardened stationary missile bases cannot be considered as very safe against surprise attack, whereas mobile launching facilities are almost invulnerable.

DOE b(3)

A group of industrial contractors lead by the Allison Division of General Motors is engaged in this investigation.

A military use for light thermonuclear warheads which is often mentioned is the anti-ICBM. In this area, however, further development of our small warheads does not seem important to me. The problem in AICBM is not ultimate reduction of warhead weight. Instead, the problem is the discrimination between an enemy nose cone and a decoy. This discrimination is [redacted] DOE b(3) In any case, AICBM is an area which does not require further nuclear warhead development.

3. Development Possibilities without Test

[redacted] DOE b(3)

DOE b(3)

4. Development with Testing

DOE b(3)

b. USSR.

DOE b(3)

[redacted] (An attempt to do so, even if feasible and successful, would in any case multiply the number of tests required and greatly protract the time scale.) Thus, under the Eisenhower proposal, tests would be required in space with all the attendant complications and delay.

DOE b(3)

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c. Time Scale

DOE b(3)

ii. If a system of observation satellites is installed, and if the test ban agreement is extended to outer space tests which can be observed by the detection satellites, and if then the Russians still wished to carry out (unobservable) tests in space, they would need to use elaborate shielding around the tested weapon, and also to conduct the test at extreme distance from the earth, such as 100 million kilometers.

DOE b(3)

DOE b(3)

I conclude that under the Eisenhower proposal, plus a moratorium, underground tests would very likely be of no substantial use to the USSR for the development of small hydrogen bombs. Space tests would be useful but would be very time-consuming if they must be designed to escape detection.

C. Large Fission Weapons

DOE b(3)

1. Status

DOE b(3)

DOE
b(3)

Our existing designs give a good coverage of the interesting spectrum of size and yield. Only minor improvements could be made in cost of material.

DOE
b(3)
b(1)

[redacted]

DOE b(3)

2. Military Uses

[redacted]

DOE b(3)

3. Development Without Test

a. USA. Modest improvements may be possible in size and cost.

[redacted]

DO(E) b(3)

4. Developments Possible With Test

a. USA. Modest improvements may be possible. [redacted]

DOE b(3)

DOE b(1)

iii. With the Eisenhower plan, plus a moratorium on testing. If the Russians observe the moratorium their progress will be very limited, as stated in Section 3b (Development Without Test).

[redacted] However, Tsarapkin's proposal that under a moratorium some seismic events which appear to be below 20 kT should be subjected to on-site inspections would make testing [redacted] hazardous. The additional precautions needed in the case of cheating, the restrictions of the areas in which tests can then be carried out, the requirement to remove all signs of testing before completing the test, the making of new test plans, etc., would surely slow down testing considerably, [redacted]

DOE b(1)

DOE b(1)

DOE b(1)

DOE b(3)

DOE b(3)

DOE b(3)

D. Small Fission Weapons [redacted]

1. Status

[redacted]

DOE b(3)

2. Military Uses

There is likely to be some use in the field for warheads of very small yields, e.g., when hostile and friendly troops are very close together. [redacted] Application will be limited by the necessarily high expense.

DOE b(3)

3. Future Development

DOE b(3)

b(1)

DOE b(3)

DOE b(3)

E. Very Small Fusion Weapons

DOE b(3)

Novel ideas like this should, in principle, be encouraged. However, on the basis of any evidence I have seen (and I have given this particular question some study recently) it is my opinion that the development of such a device would be extremely difficult, and most probably impossible

DOE b(3)

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1 of 10A -- Warren Johnson (GAC)
2 of 10A -- G. B. Kistiakowsky (PSAC)
3 of 10A -- A. D. Starbird (DMA)
4 of 10A -- N. E. Bradbury (LASL)
5 of 10A -- H. A. Bethe (Ithaca)
6 - 10A -- T-Division Files

UNCLASSIFIED Unique Document # SAA 20004033 0000

Ref: JDNO-79-16 2277

[redacted]

7/27/79 WRD

HISTORY OF STOCKPILE PROBLEMS (u) THIS DOCUMENT C... 16 PAGES
NO. 2 OF 2 COPIES, SERIES D

Briefing for the Defense Science Board CTB Panel, July 19, 1979

I. Case studies, experience during the 1958-1961 moratorium.

Background

A. In response to a growing Soviet nuclear weapon capability the United States in 1954 tested and began deployment of several thermo-nuclear weapons as an "emergency capability." [redacted] Reduing-technology weapons began entry into stockpile in 1959, after the moratorium had started on an announced year-to-year basis. It is useful to compare the strategic stockpiles in FY '58 and FY '61:

DOE b(3)

[redacted]

DOE b(3)

Derivative Classifier:
Donald R. Westervelt, J-DOT SM

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CNSS: REF: 16

In the years following FY '61 numbers of the newer weapons deployed increased rapidly, as the older ones were retired. There had been thirty types of weapons introduced into stockpile by 1958; of these 12 had been retired at an average age of 3.8 years, and those remaining had an average age of only 2.8 years. Clearly this country was, in 1961, heavily dependent on a very new technology for its defense.

B. The 1956 Suez and Hungarian episodes, the 1961 Soviet massive resumption of testing, and the 1962 Cuban missile crisis are indications of the international tension during the period surrounding the moratorium.

C. In spite of the moratorium, 13 phase threes were initiated in the period 1958-1960 in response to DOD requirements. In addition to theater weapons these included warheads for Polaris A1 and A3 and Minuteman I and II, and for Atlas, Titan I, and Titan II, as well as new bombs for the Air Force B52. Although the year-to-year nature of the moratorium implied that testing would be resumed to develop the weapons in phase three, no preparations were made for test resumption until the Soviet series began in September 1961.

[redacted]

DOE b(3)

• • •

Against this background, it is appropriate to review the history of stockpile problems during the moratorium and the years following. The discussion will be limited to cases in which nuclear testing was either indispensable in the discovery of a problem or in the solution of a problem or both; cases capable of discovery and solution by alternative administrative actions [redacted] are omitted, even though in several such cases nuclear testing has been strongly indicated as the most cost-effective and reliable course of corrective action. It must be recognized, however, that national policy rendered some possible administrative actions impermissable, so that it is impossible to divorce the subject of stockpile problems from policy issues entirely as is sometimes attempted.

DOE b(3)

A. [redacted]

DOE b(3)

[redacted]

DOE b(3)

The experiments and essential post-moratorium tests described here served to delineate the risk/benefit situation with regard to one-point safety, providing a basis for subsequent administrative judgments; while not all weapons affected were retrofitted to the most inherently safe configurations, the decisions made were informed by the test results as well as other considerations such as deployment modes and economic factors.

[redacted]

DOE b(3)

[redacted]

DOE b(3)

In summary:

- [redacted]
- [redacted]

DOE b(3)

- A suitable candidate replacement primary was believed to be available, pending confirmation by nuclear test.
- Nuclear testing of the replacement confirmed the modification.

[redacted]

DOE b(3)

DOE
b(3)

DOE
b(3)

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It should be noted that the decisions to change the HE in case (D), and to deploy the B57 without test of its intermediate yield, if necessary, were made by experienced, knowledgeable design personnel, informed by the results of extensive non-nuclear testing, and these decisions were ratified by senior management. The changes were deemed to be minor extensions of tested experience. If testing had not resumed, moreover, several of the weapons in phase three during the moratorium were planned for deployment without test, in response to stated DOD requirements for those weapons.

• • •

[redacted]

DOE b(3)

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[redacted]

DOE b(3)

F. Summary

Had testing not resumed in 1961, there would have been the following effects on the stockpile of that period:

[redacted]

DOE b(3)

• Weapons effects tests at the NTS have been used to assess the reliability of our weapons against enemy defenses, including effects of neutron-caused fission heating, X-ray caused material blowoff and impulse, X-ray caused thermal loads in structures, X-ray- and gamma-ray-caused internal electromagnetic pulse currents in detonators, cables, and other electronic components. The experiments have often revealed deficiencies in our designs and have allowed corrective measures to be taken and verified. Most of these experiments require a nuclear weapon source, and most of our knowledge regarding system hardness/vulnerability would not be available had testing not resumed in 1961. In the past, hardness requirements have changed as the perceived threat changed.

It should also be noted that, had testing not resumed, a number of major weapon systems would have been deployed without test in response to pressing DOD requirements. Many of these would have been subject to difficulties similar to those described above, but we would not have known this. That few troubles have arisen in the post-moratorium systems is a direct result of the knowledge gained after the resumption of testing.

6. Parallels and Differences

In order to relate the moratorium experience to the present time it is necessary to examine the similarities and differences between the situations then and now. A major difference is that, at the time of the moratorium, many weapon requirements remained to be fulfilled involving major strategic and theater weapon systems. Today, most known requirements are nearly in hand with tested devices. Future requirements, however, are not ruled out in the Nuclear Weapon Development Guidance, and the DOD injunction that DOE must be prepared to meet them in a CTB environment must be a cause for great concern.

A major similarity between 1958-61 and the present, which also may cause concern, is that the nation is again embarking on the deployment of many newly designed and tested weapons, affecting most of its strategic and many of its theater systems. The new weapons in many cases are as technologically advanced, compared with those they replace, as were the weapons deployed during and shortly after the moratorium. The testing of some of the new weapons was curtailed by the early impact of the Threshold Test Ban Treaty. Some involve new and different high explosives, for which there is no stockpile experience. Design intent is now in the process of translation into production engineering in several cases. In these respects the parallel with 1958-61 is striking.

There is another parallel. In the '60's one-point safety was the dominant environmental concern and caused many problems in design later to haunt us. In the '70's plutonium scattering became a problem, resulting in the development of insensitive high explosive. In the '80's the intrinsic radiation (INRAD) problem may become severe. None of these three problems have been fully resolved throughout the stockpile; all may be expected to lead to pressure to further modify existing weapons. Such modifications are not safely undertaken in the absence of nuclear testing; resumption of testing after a three-year CTB would make them possible.

A final difference between the moratorium era and today is that the laboratories now have ten times the experience in the design of boosted weapons than they then had. In the process of accumulating this experience, however, they have learned one lesson beyond doubt: [redacted]

DOE
b(3)

2. Reliability/Confidence Testing Statistics

It has been observed that only rarely has a weapon been taken from stockpile and fired for assurance, and that it is "rare to the point of non-existence" for a problem revealed by the surveillance program to require a nuclear test for its resolution. [redacted]

DOE b(3)

The first statement is partially true, in that only a few weapon tests are correctly described by these words. However, this fact must be weighed against the background of the entire test program over many years. In the course of this program, a very large number of the tests conducted contributed directly to the confidence this country has in its nuclear weapons.

This fact was elucidated in a recent study prepared at the request of the Senate Armed Services Committee. Tests in several categories, all directly related to stockpile weapons, were enumerated:

Category	Number of Tests/Interactions
• Confidence or Operational System Tests	6/6
• Source for Weapons Effects	21/27
• Seismic Calibration	5/6
• Other	6/6

All of these tests involved war-reserve weapons drawn from the stockpile. But, in addition, many other tests relevent to stockpile confidence were identified:

Category	Number of Tests/Interactions
• Stockpile Primary (War Reserve)	9/12
• Stockpile-type Weapons	24/24
• Stockpile-type Primary	69/101

In addition, there have been 44 Vulnerability and Effects tests that affected confidence in stockpile weapons in 116 instances. (The number of test-weapon interactions is greater than the number of tests simply because many tests involve components of more than one stockpile weapon.)

The striking result of the study referred to is the large number of post-proof-test nuclear events that confirmed (in the great majority of cases, but not all) the correctness of designs, particularly of primaries, in stockpile, and the continued reliability of the nuclear weapon product. Results of these tests have been monitored carefully by the laboratories, and the tests have served as a partial substitute for stockpile reliability testing that might otherwise have been considered necessary.

This raises two legitimate questions: What concerns about the reliability of the stockpile might have arisen in the absence of this large body of tests; would strict "reliability testing" have been more common? And, what will substitute for this broad testing base in the case of the systems about to enter the stockpile under a CTB, for which only development and proof-testing, in general, is in hand?

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TO : Distribution Date: April 27, 1979

FROM : D.R. Westervelt, K.F. Famularo, and R.K. Osborne

SUBJECT : THE RELATIONSHIP BETWEEN NUCLEAR WEAPON TESTING AND STOCKPILE RELIABILITY: THE LOS ALAMOS EXPERIENCE (U)

SYMBOL : JDWO-79-5

MAIL STOP : 668

This document Consists of 51 Page(s)
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INTRODUCTION

This document tabulates and summarizes in several ways all of the nuclear weapon tests related to Los Alamos Scientific Laboratory (LASL) stockpile weapons, both past (retired) and present. Tests directly contributing to the development and proof of each weapon are listed, as are relevant tests conducted after the final proof test.

The initial high confidence that the laboratory has in the reliability of its weapons is based on an extensive nuclear test program leading to final yield certification, followed by production, with careful scrutiny of the production process to ensure that the design intent is fully complied with. Tests after the final proof test of many weapons have also been conducted, for a variety of reasons; aside from their main purposes, such tests have contributed in an important way to continued high confidence in the reliability of the weapons in the stockpile.

The report presented here is a result of action taken by the Senate Armed Services Committee and the Committee on Energy and Natural Resources. In May 1978, these committees submitted a report (No. 97-961), together with additional views, to accompany the Department of Energy (DOE) National Security and Military Applications of Nuclear Energy Authorization Act of 1979 (S2693). Included in that report was a request for a joint report from the Secretaries of Energy and Defense on the reliability of nuclear weapons, and the nature, extent and results of past reliability testing. The text of the request is reproduced as Appendix A.

JDWO-79-5

[redacted]

April 27, 1979

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Copy 31A

The Relationship Between Nuclear Weapon Testing And Stockpile Reliability: The Los Alamos Experience

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[redacted]

The report also stated that "the study should present the judgment of the directors of the laboratories responsible for assessing and or qualifying the reliability of nuclear weapons on the potential importance of conducting nuclear detonations for establishing and maintaining confidence in the reliability and performance of those weapons." The statement submitted by Dr. Harold M. Agnew for inclusion in the joint report to the Senate is attached as Appendix B.

In response to the Senate request, the weapons laboratories were tasked by DOE Military Applications (MA) to identify all nuclear weapon tests relating to the reliability of the weapons in stockpile, both past and present. Those tests were to be broken down into six categories. Definition of the categories has of necessity been adjusted during the study. Summaries from both LASL and the Lawrence Livermore Laboratory (LLL) have been combined to provide the basis for the joint DOE-DOD report to the Senate Committees.. Only a tabular numerical summmary was used in that report; the purpose of this document is to provide the detailed backup tabulation, weapon by weapon and test by test, on which the LASL numerical summary, submitted in November 1978, was based. The tabulations presented here are augmented versions of those that were used earlier.

Because of the stringent ground rules we established, many tests that contributed to technology development, but not directly to a stockpiled weapon design, were not listed in the compilation, nor were many LLL tests that contributed in some way to the LASL technology base (e.g. Surfer tests). The synergism among all tests should not be overlooked.

The results of the study are presented, first, in the form of aggregate numerical summaries in which total numbers of tests and of test-weapon interactions before and after final proof tests are displayed; second, in the form of a tabular numerical summary of tests in each of the six categories for every LASL weapon (the form in which the study results were provided to MA for the Senate report); and third, in the form of the detailed backup tabulation that lists all tests as they relate to each weapon. To aid readers having varied objectives, the backup tabulations have been listed in several ways: by weapon number in numerical order; chronologically; and alphabetically by event name.

DEFINITION OF THE CATEGORIES

In the development of this report we adhered to the following test category definitions (not identical in wording to, but not inconsistent with, the definitions in the final report to the Senate):

1. Development tests. These are tests (development, safety, proof) performed during the development of the weapon prior to and including any proof test. In Category 1 we have listed all tests in the main stream of the development (but not all tests mainly done to develop a technology base), even though some of those tests resulted in dead ends. [redacted]

DOE b(3

2. Stockpile tests. These are tests of weapons taken from the War Reserve (WR) stockpile or the WR production line. Components external to the physics package were changed in many cases for the test [redacted]

DOE b(3)

3. Stockpile Primary Tests. These are tests of primaries obtained from WR weapons or WR production. Again, components external to the physics package were often changed, but no change was allowed in the nuclear components.

4. Stockpile-type tests. These tests involved nuclear components the same as those in WR weapons, but they may not have been completely fabricated in the WR production complex. In these tests the device tested was considered to be equivalent in every significant respect to the stockpile device, without reference to the actual beginning of phase 5 (the First Production Unit or FPU), and without reference to the primary gas fill.

The reason for the first (phase 5) is that on some occasions tests have employed devices finally proof-tested but for which the FPU had not been delivered. The reason for allowing gas-fill changes is that this happens between proof-test and production specification with some regularity.

5. Stockpile-type primary tests. These tests involved nuclear components equivalent to those in the stockpile primaries, but they may not have been fabricated entirely in the WR production complex. Any primary test not in Categories 2, 3, or 4, subsequent to the primary proof test, is counted in this category. Again, gas changes did not disqualify otherwise valid tests.

6. Vulnerability and effects tests. The purpose of these tests is to gather fundamental information on system and component response to hostile environments, to develop hardening procedures and verify their effectiveness, and to satisfy requirements that the weapon system will operate satisfactorily despite hostile attack as defined under stockpile to target sequence conditions. Such tests are a vital factor in assessments of stockpile reliability. Other effects tests that do not fall in the V&E category are included by placing them in other appropriate categories, when they involved a WR or stockpile-like weapon.

AGGREGATE NUMERICAL SUMMARIES

(A) Actual test events (explosions)

For the case of actual test events (explosions) it is not convenient to separate the events according to whether the weapon affected by the test had or had not been retired from the stockpile, because many tests affected both kinds of weapons (weapons now retired were affected during their stockpile life by tests that also were related to weapons still in the active inventory). However, total event numbers in each category for all weapons, retired as well as current, are tabulated in Table I.

TABLE I

TEST EVENT SUMMARY

Category 1:	197
Category 2:	28
Category 3:	2
Category 4:	20
Category 5:	55
Category 6:	25

These numbers when totalled exceed the actual number of events (276) because 36 events appear in two categories, six in three categories, and one in four categories.

(B) Test-weapon interactions

We can also summarize the numbers in each category that represent test-weapon interactions; a single test may have had implications regarding the design - or confidence in the design - of several weapons. This summary is obtained by totalling the following detailed tabulations, and is more relevant than bare event numbers when one is assessing the extent to which post-proof-test experiments have contributed to stockpile confidence. The result is given in Table II.

TABLE II

TEST INTERACTION SUMMARY

Category 1:	298
Category 2:	35
Category 3:	2
Category 4:	21
Category 5:	85
Category 6:	38

In contrast to the event summary, the interaction summary can be separated into numbers for retired and current inventory. The total number of test-weapon interactions in categories 2 through 5 (that is, post-proof-test experiments) for the 26 retired LASL weapons is 55; the same total for the active inventory of 18 LASL weapons is 88. The largest single category in the post-proof-test set is Category 5, with 85 entries, of which 51 involve primaries of weapons still in the active inventory.

WEAPON-BY-WEAPON NUMERICAL SUMMARY

The numbers of tests in each category for every LASL weapon are presented in Table III; retired weapons are identified by the letter R.

TABLE III

NUMERICAL SUMMARY OF TESTS OF ALL LASL WEAPONS

DOE b(3)

Some of these tests, of course, have been unsuccessful to a greater or lesser degree, or have revealed stockpile problems that resulted from design errors later corrected, sometimes on the basis of additional tests. The above numerical summary, as it existed in November 1978, combined with similar data provided by LLL, is the basis for Table I in the joint DOE-DOD report to the Senate Committees. That report describes in detail the methods used - including nuclear testing - to assure continued high confidence in the reliability of the U.S. nuclear weapons stockpile.

The striking result of this study is the large number of post-proof-test nuclear events that confirmed (in the great majority of cases) the correctness of primary designs in stockpile and the continued reliability of the nuclear weapon product. Results of these tests have been monitored carefully by the laboratories, and the tests have served as a partial substitute for stockpile reliability testing that might otherwise have been considered necessary. This raises a legitimate question: What concerns about the reliability of the stockpile might have arisen in the absence of this large body of tests? Reliability tests as such have been conducte for very few weapons (e.g. the LLL W62 MMIII warhead), but several are planned in the near future.

BACKUP TABULATIONS

The detailed data base from which the numerical summaries were drawn is presented in tabular form. As noted earlier, the tabulations are listed in three ways: by weapon number in numerical order (Table IV), by date of the test events (Table V), and alphabetically by event name (Table VI).

Many tests contributed to the development or post-development assessment of reliability of several stockpile weapons and therefore are listed repetitively; to facilitate the counting process, each test is identified by an asterisk in the date column the first time it appears in each listing. The Greenwich Civil Time of each event is given as yymmdd, where yy is the last two digits of the year, mm is the month, and dd is the day. In Category 1, entries X, X1, or X2 in the development or proof columns indicate that the test was relevant to the entire weapon, the weapon primary, or the weapon secondary, respectively.

DOE b(3)

The COMMENTS on V&E tests generally describe the components exposed. The column headings for each category carry a mnemonic aid as follows:

Category

1	D	Development
1	S	Safety
1	P	Proof
2	WRW	War Reserve Weapon
3	WRP	War Reserve Primary
4	RW	Reliability of the Weapon
5	RP	Reliability of the Primary
6	V&E	Vulnerability and Effects

In the COMMENT field of the tables, the word EFFECTS has been included if the shot was a DOD or joint DOE-DOD effects test; Category 6 (V&E) events may or may not have this indication. The identification of effects tests was a requirement of the original DOE/MA request that led to this report. The letter F in the COMMENT field indicates that a shot was a clear failure.

Other comments listed were limited by the space available and are primarily of use to the compilers. In many cases the shorthand used is not self-explanatory. No attempt was made in the comments to systematically describe the purpose of each test.

The initial version of the tabulation, JDWO-78-27, contained information about the diagnostic measurements made on each test, in order to allow a judgment regarding the test's contribution to knowledge about the characteristics of the test device. The diagnostic information is omitted from this listing, but it may be included in future versions. In any case, the information is available.

It is not unlikely that the tables will require revision in the future, but we expect such revisions to be minor. Periodic updating may be desirable if the document is considered to have intrinsic value beyond its original purpose.

Compilation of the data base was not straightforward. A basic reference was the Test Information Index published by the Defense Nuclear Agency (HQDNA 119M-11, 15 November 1977), but that document alone was insufficient for the task. Also helpful were the DNA Nuclear Weapons Characteristics Report (HQDNA-48M, 15 July 1978) and A History of the Nuclear Weapons Stockpile, FY 1945 to FY 1975 (USERDA, TID 26990, August 1976). However, using these as basic guides, it proved necessary to exhaustively comb the sources of local documentation and the memories of participants in the weapon program to establish what we believe is a consistent list of all tests in each category for every weapon. The tabulations maintained by B.A. Wellnitz were indispensable to this task.

The following individuals contributed to the assembly of this document; the authors assume responsibility for any errors that remain.

G.O. Allshouse, C.I. Browne, A.W. Charmatz, R.S. Dingus, D.D. Eilers, E.H. Eyster, T.R. Gibbs, B.P. Ginzberg, R.M. Henson, G.G. Hill, J.C. Mark, W.L. Mudd, W.E. Ogle, R. Pollock, J.C. Porter, W.E. Preeg, J.L. Richter, R. Rochester, H.H. Rogers, J.E. Sattizahn, T.L. Talley, M.T. Thieme, P. Vander Maat, D.W. Watkins, J.J. Wechsler, P.P. Whalen, D.R. Worlton, and R.E. Williamson. Without an heroic effort by L.M. Button we could not have met deadlines set by DOE/MA in November 1978.

TABLE IV

WEAPON NO. SORT

[redacted]

DOE
b(3)

DOE b(3) [redacted]

DOE
b(3)

[redacted]

DOE b(3) [redacted]

[redacted]

DOE b(3)

DOE b(3)

[redacted]

DOE
b(3)

DOE
b(3)

TABLE V

CHRONOLOGICAL SORT

DOE b(3)

[redacted]

DOE
b(3)

DOE b(3)

[redacted]

DOE
b(3)

DOE
b(3)

TABLE VI

EVENT ALPHABETIC SORT

[redacted]

DOE
6(3)

DOE
b(3)

[redacted]

DOE
b(3)

DOE
b(3)

DOE
b(3)

[redacted]

DOE
b(3)

JDWO-79-5

April 27, 1979

DOE
b(3)

THE DOCUMENT THAT CONTAINS THE TASKING IS:

Calendar No. 893

95TH CONGRESS 2d Session	SENATE	REPORT No. 95-961

DEPARTMENT OF ENERGY NATIONAL SECURITY AND MILITARY APPLICATIONS OF NUCLEAR ENERGY AUTHORIZATION ACT OF 1979

JUNE 26 (legislative day, MAY 17), 1978.—Ordered to be printed

MR. STENNIS, from the Committee on Armed Services and the Committee on Energy and Natural Resources, submitted the following

REPORT

together with

ADDITIONAL VIEWS

[To accompany S. 2693]

THIS IS PART OF PAGE 10:

Reliability of nuclear weapons

In conjunction with the fiscal year 1980 budget requests, the Secretaries of Energy and Defense are requested to submit to the Congress a joint report on the reliability of nuclear weapons currently and formerly in the U.S. arsenal. Such report should document the nature, extent and results of past reliability testing and the amount and associated costs of such testing as may be planned for fiscal year 1980. In addition, the study should present the judgment of the directors of the laboratories responsible for assessing and or qualifying the reliability of nuclear weapons on the potential importance of conducting nuclear detonations for establishing and maintaining confidence in the reliability and performance of those weapons.

The President is requested to review and comment on this report, drawing on the views of other technical and scientific agencies or entities as he may choose.

JDWO-79-5
Appendix B

April 27, 1979

UNIVERSITY OF CALIFORNIA
LOS ALAMOS SCIENTIFIC LABORATORY
(CONTRACT W-7405-ENG-36)
P.O. BOX 1663
LOS ALAMOS, NEW MEXICO 87545

REPLY
REFER TO:
MAIL STOP: 100

November 17, 1978

MajGen Joseph K. Bratton
USDOE/MA
Germantown, MD

Dear General Bratton:

The Senate Committee on Armed Services have asked me to express my judgment "on the potential importance of conducting nuclear detonations (nuclear tests) for establishing and maintaining confidence in the reliability and performance" of nuclear weapons currently and formerly in the U.S. arsenal. I am happy to do so.

I should state at the outset that I believe that the nuclear weapons we now have in the U.S. stockpile are entirely reliable. We initially acquired this high confidence in the performance of each nuclear weapon as a result of the careful testing we conducted to certify the correctness of the designs. At present there is a large body of data from our continuing test program that allows us to maintain that confidence. I shall summarize some of the relevant information later in this letter. The importance of this data base cannot be overemphasized, although few outside observers seem to be aware of its existence. Thus the phrase "reliability testing" is often misused, in that its meaning is frequently limited to the description of tests in which weapons are literally extracted from the stockpile and fired to see whether they will go off with the right yield. That is almost never done, for reasons that will become clear, yet testing is absolutely essential to our continued high confidence in the reliability of our weapons.

Before I turn to the specific data base that supports our confidence in the stockpile, past and present, let me review again briefly the overall importance of the continuing test program: (a) The test program increases our understanding of nuclear weapon physics, which remains incomplete even after many years of theoretical and empirical study; (b) It maintains the competence of weapons designers and engineers so that they can provide professional judgments when questions arise - as they do - about stockpile weapons, or when those weapons must be rebuilt; (c) It allows the discovery as well as in some cases the resolution of problems in the stockpile. As I have said many times, I do not believe that the weapons technology base, on which our confidence in the stockpile ultimately rests, can be maintained indefinitely without the essential empirical element provided by nuclear testing. If testing is halted - without the clear and assured prospect of resumption within a very few years - a rapid decline in our ability to make correct judgments about the stockpile is inevitable. Stockpile maintenance is a dynamic, not a static process. There will be pressures to make "minor" changes in stockpiled systems that will force design judgments. We will be unable to confirm the validity of those judgments by testing. This will either lead to a significant loss of confidence over the years or, alternatively, false confidence that is a result of our growing ignorance about what can and cannot be done with nuclear weapons.

It is clear that there is an important synergism among all of the tests that we do. Equally - if not more - important, however, to our confidence in the reliability of the stockpile are the many tests that involve either key nuclear components of weapons in the stockpile or the stockpile weapons themselves. Such tests are not, in general, performed because of misgivings about weapon reliability; they may be weapon effects tests, seismic calibration tests, operational system tests, or tests serving a number of other primary purposes. In the process of achieving their primary objective, however, they additionally provide confirmation that the weapon or key weapon component used in the test does function properly. This is not accidental - most tests of this kind call for sources having predictable characteristics, and the sources we know best reside in the tested stockpile. Every such test is in a real sense a reliability test, and it is against the background of these tests that the relative infrequency of "reliability tests," narrowly defined, must be examined.

The exact numbers of tests that have thus contributed to our confidence in the reliability of the stockpile will be provided and analyzed in a classified attachment.* I can summarize some of the information here, however. The number of LASL weapon designs that have been retired from the active inventory over the years exceeds the number of designs now in the stockpile by about 40%. Many of the older weapons were in the stockpile for only a few years, having been replaced by newer designs well before any aging concerns arose. Yet, during their lifetime in the stockpile, there were no fewer than 53 instances in which tests involving either stockpile (War Reserve - WR) weapons directly or the key nuclear components of such weapons, those on which their reliability depends, confirmed the proper functioning of the weapons or components (or, in a few cases, revealed problems with the weapons). The actual number of tests involved was somewhat smaller, because some tests confirmed the reliability of more than one stockpile weapon. For the weapons still in the active inventory the number of such confidence-building incidents is larger: 89. All of these confirmations of reliability have testified to the continued operability of the LASL weapons in the present stockpile. Over 20 of the tests in recent years have involved WR weapons, while the rest have involved the most important (from the standpoint of reliability) nuclear components of the stockpile weapons, and speak directly to the reliability of the weapons themselves. All LASL weapons now in stockpile have had the benefit of at least one, and in most cases several such "reliability tests," as shown by weapon type in the attachment.*

*The data in the classified attachment is included as part of the total data in Table I of this report.

It is, I believe, generally understood that modern weapons have become more sophisticated and safer, less vulnerable to hostile environments and to terrorist activity, and more constrained by economic and delivery system requirements. As the duration of their retention in the stockpile has grown compared to that of the earlier weapons, the number of tests that, among other things, demonstrate their continued reliability has naturally also grown. This signals a special problem in connection with the newest designs now entering the stockpile, replacing older weapons and providing new capabilities. There simply does not exist a backlog of reliability test experience with those weapons to attest to their reliability five, 10, 20 years hence. If testing continues, past experience suggests that in the normal course of events such a data base will be developed for each of the new weapons, as it has for the present stockpile. Without testing this will not happen. Further, problems that may develop will not be disclosed by testing, as they sometimes have been in the past. Combined with the erosion of our expertise, this will seriously jeopardize our long-range confidence in the reliability of some of the most important strategic systems in the U.S. arsenal.

I hope that these facts will contribute to a better understanding of the essential role testing has played in maintaining our continued high level of confidence in the reliability of the nuclear weapons in our stockpile, and of the necessity for continued testing in the future so long as we depend on these weapons for our security and that of our allies. We have established within the nuclear weapons community what the minimum necessary level of testing would be for the narrowly defined purpose of maintaining the stockpile. Without the minimum testing that is necessary we will either lose confidence in the long run, or be lulled into false confidence without knowing that it is false. Neither result, in my view, would be acceptable.

Sincerely,

Harold M. Agnew
Director

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SUBJECT: U. S. WEAPONS EXPERIENCE

SYMBOL: TD-9:78-6

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U.S. WEAPONS EXPERIENCE (u)

IAF270

The following six examples show the consequences of the 1958-1961 Moratorium on U.S. weapons in stockpile during the period and also on Phase 2 and Phase 3 programs then under development.

[redacted]

DOE b(3)

LOS ALAMOS
01713299

He-3 Effects

Before the Moratorium of 1958-1961, all LASL tests of hollow-boosted primaries had been conducted with zero age gas; i.e., gas mixtures in which He-3, the decay product of T, was present only in minute quantities. At that time, it was the opinion of management and most designers that the actual test of primaries with aged gas was predictable and therefore of low priority.

[redacted]

DOE b(3)

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...

I9 0-4222 2/28 *

TD-9:78-6

January 25, 1978

[redacted]

DOE b(3)

Nuclear testing was necessary, however, to certify this. We simply did not know what the problem was.

[redacted]

From examination of the testing record, we believe that LLL experienced similar surprises after deployment of the W45. However, we believe that LLL would have the more vivid memory of the details, and we recommend that LLL be invited to comment.

[redacted] Without the benefit of testing, the error very well might have propagated, undetected. [redacted]

DOE b(3)

One-Point Safety

In 1958, first production units were completed for four weapons systems, B39 Mod 1, W28, B36Y1X2 and W34. One-point safety tests for their implosion systems had been conducted in 1955-58 and safe pit designs had been envolved for stockpile. [redacted]

DOE b(3)

Each of the four above systems had been tested for safety by firing a normal detonator, and had been found to be safe. [redacted]

DOE b(3)

In the case of the B28 we were able to retrofit with a safer pit only as the result of a happy coincidence. [redacted]

DOE b(3)

The 1959 discovery (of sensitivity to location of the point of detonation) was made as the result of continued study of the anamolous test data collected in 1955-58. [redacted]

DOE b(3)

TD-9:78-6

January 25, 1978

Cyclotol vs 9404

Implosion systems were developed during 1957-58 for the W30, B41, B53, W53, and W52. [redacted] DOE b(3)

During the 1958-1961 Moratorium two accidents involving the explosive 9404 occurred at Los Alamos. As a result, the laboratory decided to discontinue use of this explosive in systems of large size [redacted] and to substitute the less sensitive and less energetic explosive cyclotol in its place. DOE b(3)

[redacted] DOE b(3) New pit designs were released in 1960 and 1961 and pit FPU dates occurred during 1960-1962. Nuclear testing was resumed underground at NTS on 9/15/61. [redacted] DOE b(3)

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TD-9:78-6

January 25, 1978

[redacted]

DOE b(3)

W45

[redacted]

DOE b(3)

W47 and W56

[redacted]

DOE b(3)

It is of interest to recall that the Eisenhower Moratorium of 1958-1961 was instigated by the statement of the USSR that they would stop testing. The U.S. followed suit. [redacted]

DOE b(3)

Beginning September 15, 1961, the two U.S. weapon laboratories and the Department of Defense took more than one year to conduct 45 tests. Thus by this measure of the number of tests, the strategy of moratorium during which they continued extensive preparation (and eventual sandbagging) gained more than one year in development time for the USSR, relative to the U.S.

Because of this experience, it has always been very difficult for us to believe that the Eisenhower Moratorium contributed one iota to either U.S. security or to a reduction in the arms race.

We believe that the task of listing the benefits of this moratorium should be imposed upon ACDA. The history is there. The lessons should be delineated.

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[redacted]

TD-9:78-6

January 25, 1978

In addition to the above six examples of U.S. problems connected with the moratorium, there have been many post moratorium design problems that were only discovered through nuclear testing. Those connected with nuclear design will be covered in a separate memo. We are, however, including six examples of vulnerability and effects problems that have only been revealed by nuclear testing.

<u>Proposed Paragraph for Executive Summary</u>

Experience has shown that our warheads and reentry vehicles must survive in hostile environments that frequently change as our perception of Soviet defense capability improves. Weapon effects tests at NTS are used to assess the reliability of our warheads against these evolving enemy defenses. The experiments have revealed major deficiencies in our designs and allowed corrective measures to be taken. Many of the defects were unexpected and could not have been revealed except by full scale testing. Because we made corrections, we have an increased confidence in the ability of our tested systems to perform their required missions. In attack scenarios, x-ray fluences are too high for simulation except with a nuclear weapon as the source. Although many neutron attack scenarios can be simulated using pulse reactors, other engagements involve neutron pulse widths that are orders of magnitude too narrow for any foreseeable pulse reactor to simulate. Thus we have to this time been quite dependent upon nuclear testing to display the Achilles' Heels of the survivability of our weapon systems.

<u>Discussion</u>

In our support of Phase 3 programs (especially), it is clear to those who do V & E work that the using services do take survivability seriously. They consider assurance of a design yield under benign conditions necessary but not sufficient.

It is true that we know a great deal about the physics of neutron and x-ray interaction with materials; so also does TD-Division know how bombs work.

However, just as the weapon designers need experiments to display the unexpected results, so do the scientists who work in the area of vulnerability and effects. Just as TD-Division and their counterparts at LLL have put warheads into the stockpile that didn't perform as expected, so have there also been close calls in the area of V & E.

[redacted]

These effects were all unexpected and wouldn't have been found except by means of NTS tests.

It is true that we can pay a weight and performance penalty and provide additional hardening, for a conservative design, but only for effects and phenomena that we already know about. In the community, we call the unexpected disasters "Achilles' Heels." We've met them before and, without testing, we cannot guarantee that we won't meet them again.

Memo TD-4:78-3 includes a brief (two pages of 17) summary of V & E and a CTB. However, in the latest document (over 40 pages long) the V & E section has been deleted and a single V & E table (with errors) has been substituted. This seems an unfortunate failure to describe an important area. We believe a brief two pages of text on V & E (such as those in TD-4:78-3 and here) could be accommodated in the supporting document and would describe better the importance of NTS testing to design and certification of reliability of our hardened weapon systems.

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BROWN AND I WILL BE AT AFOAT ON FRIDAY /AND PRESUMABLY MARK AND COWAN/ PD SUGGEST THE WHOLE PROBLEM BE DISCUSSED THERE SINCE POSSIBLE PERMUTATIONS AND COMBINATIONS SEEM TO BE GROWING RAPIDLY PD

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Rockwell International

November 26, 1986

CD86-6100

Albert E. Whiteman
Area Manager
DOE, RFAO

FINAL ACHIEVEMENT CALCULATION ON GAO-86-2 AND GAO-86-3

1. "Oxide Reduction Initiative"

Performance on the "Oxide Reduction Initiative" was determined as prescribed in GAO-86-2 and the Production Control Procedure, dated June 5, 1986. Achievement and award were determined by the amount of reduction acheived in the percentage rate of oxide generation. The percentage rate of oxide generation was defined as the ratio of the "Total Nuclear Weight of Oxide Shipped from the 707 Foundry" divided by the "Total of the Charge Weights as Recorded on the Casting Run Sheets". The Production Control Procedure established the criterion for matching oxide shipments to the "accounting month" reports of total casting activity.

Figure 1 shows a tabulation (and matching) of weekly oxide shipments and total casting activity for each of the accounting months in the five month performance measurement period.

The GAO-86-2 stipulated that achievement (for award purposes) would be determined by calculating the mean of the three-month moving averages (rounded to the second decimal place) for the months of July, August, and September. Figure 2 shows the calculation of the three month averages for July, August, and September, and the calculation of the average of those three averages. [redacted] Figure 3 shows the award scale and the actual achievement.

DOE b(3)

[redacted]

DOE b(3)

2. "Pipeline Reduction Initiative"

Performance on the "Pipeline Reduction Initiative" was determined as prescribed in GAO-86-3, and the Production Control Procedure, dated June 5, 1986. [redacted] Performance (and award) was determined by the amount of reduction achieved in the "Issue-to-Ship" ratio. [redacted] DOE b(

The GAO specified that three weight ratios were to be calculated. [redacted] DOE b(

[redacted] The quantities shown in the "Total" column were used to calculate the "Baseline" and "Goal" weight ratios. DOE b

Figure 6 summarizes the calculations made to determine the weight of material shipped; the amount of material required to be issued under baseline conditions; and the amount of material required to be issued if the goal conditions were met.

Figure 7 shows the resultant award scale that was determined by GAO-86-3 and the calculated "Baseline" and "Goal" ratios, and the achievement.

Figure 8 shows the quantities of material issued and shipped in each of three months of the GAO performance period. It also shows the three-month issue-to-ship that determined the amount of the award. [redacted] DOE b(

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DOE b(3)

J. E. Kinzer, Director
Program Management

C. W. Weidner, Director
Production Operations

W. F. Weston, Director
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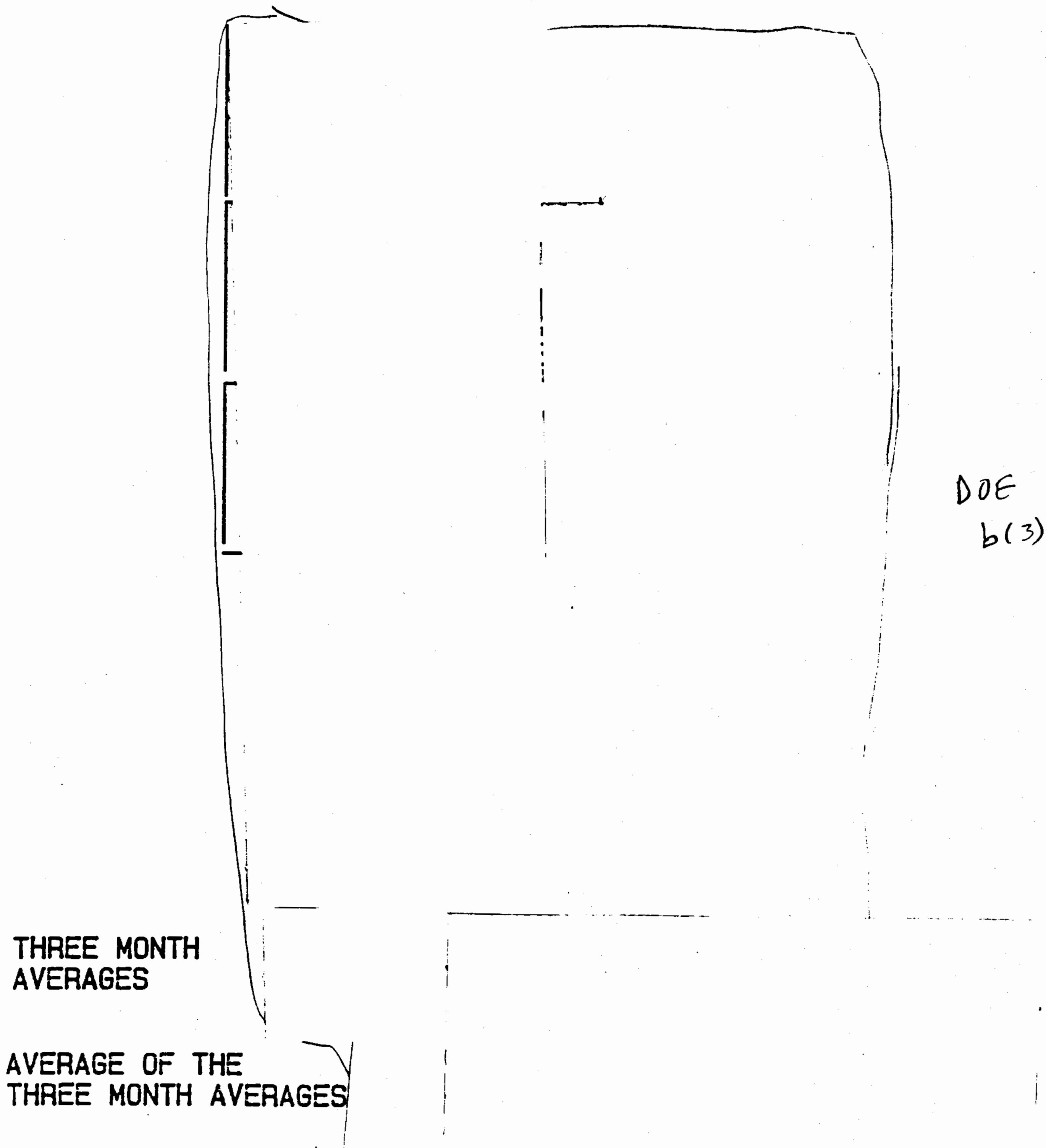
TABULATION AND MATCHING OF WEEKLY OXIDE SHIPMENTS TO "MONTHLY" CASTING ACTIVITY

ACCOUNTING MONTH	WEEK ENDING SUNDAY	OXIDE SHIPPED (KGS)	TOTAL OF OXIDE SHIPPED (KGS)	TOTAL WT. CAST IN ACT'G MO. (KGS)
MAY	27 APRIL			
	4 MAY			
	11 MAY			
	18 MAY			
	25 MAY			
JUNE	1 JUNE			
	8 JUNE			
	15 JUNE			
	22 JUNE			
JULY	29 JUNE			
	6 JULY			
	13 JULY			
	20 JULY			
AUGUST	27 JULY			
	3 AUG			
	10 AUG			
	17 AUG			
	24 AUG			
SEPTEMBER	31 AUG			
	7 SEPT			
	14 SEPT			
	21 SEPT			

DOE b(3)

FIGURE 1

CALCULATION OF OXIDE REDUCTION ACHIEVEMENT USING THE DATA FROM FIGURE 1



DOE b(3)

FIGURE 2

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FIGURE 3

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FIGURE 4

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FIGURE 5

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SUMMARY OF "BASELINE" AND "GOAL" [WEIGHT RATIO CALCULATIONS]

PROGRAM	TO BE SHIPPED	"BASELINE" CONDITIONS	"GOAL" OBJECTIVES

FIGURE 6

DOE b(3)

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AWARD SCALE AS DETERMINED BY "BASELINE" AND "GOAL" RATIO CALCUALTIONS

RATIO OF $\frac{\text{WT OF MATERIAL ISSUED}}{\text{WT OF MATERIAL SHIPPED}}$

DO
b(

FIGURE 7

[redacted]

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DOE b(3)

[(DATA IS FROM PRODUCTION CONTROL'S REPORT OF 10-2-86)]

MONTH	WEIGHT ISSUED KGS	WEIGHT SHIPPED KGS	RATIO FOR THE MONTH	3 MONTH RATIO
JULY				
AUG				
SEPT				

DOE b(3)

FIGURE 8

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[redacted]

CALCULATION OF AWARD FOR [GAO-86-3 PIPELINE REDUCTION INITIATIVE]

RATIO OF

ACHIEVEMENT

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FIGURE 9

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FINAL STATUS REPORT ON THE "SUPPLY AND DEMAND" GOAL ACHIEVEMENT OBJECTIVES, GAO-86-1, 2, AND 3

The following is a report of work accomplished through September, 1986.

DOE b(3)

DOE b(3)

STATUS OF THE "SUPPLY INITIATIVE"

1. Accomplishments

DOE b(3)

DOE b(3)

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This outstanding achievement on processing low-level residues was possible because of the success achieved with the "Oxide Reduction Initiative" portion of this GAO. The supply of oxide from the Building 707 Foundry was dramatically reduced in the second, and third quarters of FY-86. This allowed Building 771 to convert one of its oxide recovery lines to the processing of low-level residues.

2. ISSUES

None

3. ACTION

Although the period of this GAO has ended, Building 771 will continue to concentrate on the processing of low-level residues. Recovery of metal from foundry oxide will continue, but at a greatly diminished rate. The reduction in the amount of oxide generated in the 707 Foundry is permanent and will continue to decline.

4. EXPECTED RESULTS

With continued strong emphasis on the processing of low-level residues, the backlog of waste drums will be rapidly reduced, and a significant amount of plutonium will be returned to active use. The reduction in drum backlog will also release valuable storage space for more productive uses; and reduce the plant's exposure to risk in the areas of safety, security, accountability, and environmental issues associated with the drum backlog.

STATUS OF "OXIDE REDUCTION INITITATIVE"

1. ACCOMPLISHMENTS

Figure 1 shows the history of oxide generation from October, 1985 through September, 1986. The figure shows the final adjusted values for April, May, June, July, August and September. The adjustments were made to achieve exact matching of weekly oxide shipments to total casting activity in an inventory period. Oxide shipment was then prorated (i.e., averaged for the inventory period) to each of the two months within an inventory period. (Hence, the oxide percentage is the same for the two months in any inventory period.)

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[redacted] DOE b(3)

Figure 2 shows the trend of the three-month moving average of oxide generation (based on the final adjusted monthly values shown in Figure 1). [redacted] DOE b(3)

The reduction in oxide generation rates has dramatically reduced the total amount of oxide generated. [redacted] DOE b(3)

[redacted] DOE b(3) The award for the "Oxide Reduction" portion of the GAO is determined by the average of the values (stated above) for the months of July, August and September. [redacted] DOE b(3)

2. ISSUES

Two things caused the oxide generation rate to increase in September:

1. September was an inventory month, and in preparation for the inventory, the glove box line was cleaned, swept and purged of all residues. [redacted] DOE b(3)

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2. The 707 Foundry did not have a normal supply of clean plutonium buttons to blend with dirtier material such as molten salt buttons and briquetted material. An analysis of the source and type of material cast in the foundry in September shows:

Clean Buttons	- [redacted]
Dirty Buttons	- [redacted]
Briquetted Material	- [redacted]
Solid Scrap	- [redacted]

DOE b(3)

[redacted] The solid scrap was clean material and was cast directly into WR ingots in the low vacuum tilt-pour furnaces. [redacted]

DOE b(3)

DOE b(3)

3. ACTIONS

The PUIP team has prepared a FY-87 plan that includes eight specific projects to improve or replace equipment and improve procedures and control. [redacted]

DOE b(3)

4. EXPECTED RESULTS

[redacted]

DOE b(3)

STATUS OF "PIPELINE REDUCTION INITIATIVE"

1. ACCOMPLISHMENTS

Figure 3 shows the monthly values of the "issue-to-ship" ratio from April through September, 1986. [redacted] This extraordinary achievement was due to the intense coordinated effort of the people in Building 707 to increase yields; reduce leadtimes from 6 weeks to 3-1/2 weeks and accelerate the disposition of non-conforming product. [redacted]

DOE b(3)

DOE b(3)

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[redacted]

DOE b(3)

Figure 5 shows the trend of Building 707 Inventory from June, 1985 to September, 1986 (i.e., last fifteen months). [redacted] This was another major benefit from the PUIP activity.

DOE b(3)

The award for the "Pipeline Reduction" portion of the GAO is determined by the three-month average of the "issue-to-ship" ratio at the end of September. [redacted]

DOE b(3)

2. ISSUES

The supply of plutonium buttons to the 707 Foundry is expected to be limited (and marginal at best) for the next several months; therefore, the IP schedules for the next few months could be in jeopardy. [redacted] (See Figure 5.)

DOE b(3)

[redacted]

DOE b(3)

3. ACTION

The PUIP team has prepared a FY-87 "Pipeline Reduction" plan that includes 27 seperate projects. These projects, combined with Production Control's desire to maintain the short (three week) leadtime established in September and the building's management drive to improve quality and eliminate paperwork errors, will cause the building inventory to decline.

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4. EXPECTED RESULTS

[redacted] DOE b(3)

The continued decline in inventory would measure and reflect the continued improvement in the building's production capacity and capability.

J. E. Kinzer, Director	C. W. Weidner, Director	W. F. Weston, Director
Program Management	Production Operations	Plutonium Operations

Distribution:

1A - A. E. Whiteman - DOE, RFAO
2A - A. E. Whiteman - DOE, RFAO

cc:
3A - H. F. Greer - NASO, Rocky Flats
4A - R. C. Hood - NASO, Rocky Flats
5A - J. E. Kinzer - NASO, Rocky Flats
6A - W. A. Kirby - NASO, Rocky Flats
7A - D. J. Sanchini - NASO, Rocky Flats
8A - C. W. Weidner - NASO, Rocky Flats
9A - W. F. Weston - NASO, Rocky Flats

UNCLASSIFIED

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DOE b(3)

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-7-

SECRET

SECRET UNCLASSIFIED

DOE b(3)

SECRET

FIGURE 2

-8-

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SECRET UNCLASSIFIED

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DOE b(3)

FIGURE 3

SECRET

UNCLASSIFIED

SECRET

FIGURE 4

DOE b(3)

SECRET UNCLASSIFIED

DOE b(3)

SECRET

FIGURE 5

SECRET

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