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A Short Account of Los Alamos Theoretical Work on Thermonuclear Weapons, 1946-1950

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FOREWORD

This report is an unclassified—and consequently, somewhat abridged—version of a document prepared during the summer of 1954. Except as required to remove classified references, and to restore continuity, it follows the original.

The earlier document (issued on October 1, 1954) was the first draft of a chapter for a proposed history of the technical work at Los Alamos from the end of the war up to 1954. This particular chapter was to cover the Los Alamos work on thermonuclear weapons from 1946 to January 1950—the time of President Truman's decision concerning U.S. work on the hydrogen bomb. Several other sections for such a history were also drafted (by other authors); but the project as a whole began to appear to be too onerous to carry further—at least in the hands of a group of persons already fully occupied (and much more intensely interested) in the more immediate undertakings of the Laboratory.

This unclassified version has been prepared in order to provide a factual account of the Los Alamos work on thermonuclear weapons during this particular period. Because of necessary classification restrictions it would not have been possible many years ago to release an account which was both factual and coherent. Many such restrictions are, of course, still in effect; but the steady progress of declassification has now reached the point (or may even have reached it several years ago) at which it is possible to make the present report available. Inasmuch as a distinctly erroneous impression of these matters is rather widely held, it will seem worthwhile if this report should ultimately help establish a better understanding of what actually took place.

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A SHORT ACCOUNT OF LOS ALAMOS THEORETICAL WORK ON THERMONUCLEAR WEAPONS, 1946-1950

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ABSTRACT

A factual account of work (mainly theoretical) on thermonuclear weapon development at the Los Alamos Scientific Laboratory from 1946 to 1950, this is an unclassified version of a chapter (written in 1954) for a proposed technical history. It outlines the computational and theoretical work devoted to study of the Super and other thermonuclear problems.

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 reports prepared for the conference. 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.

The estimates available of the behavior of the various steps and links in the sort of device considered were rather qualitative, and open to question in detail. The main question of whether there was a specific design of that type which would work well was not answered. Had the physical facts been such that there was a large factor to spare in attempting to demonstrate that such a device would detonate, then the type of considerations which it had been possible to devote to this problem up to that time would have been sufficient to establish that fact. As it was, the studies of this question had merely sufficed to 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. The general belief of those working on the problem at that time, however, was that some such design could be made to detonate, although it was fully understood that much study would yet be required to establish this fact and determine the most favorable pattern.

The requirements for materials, engineering developments, and more detailed understanding of basic physical processes were impressive, and (as said at the time) "would necessarily involve a considerable fraction of the resources which are likely to be devoted to work on atomic developments in the next years." An active program to realize such a device was then thought to require amounts of tritium beyond the reach of the Hanford plant to produce in any relevant time, so that the building of a 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 handling deuterium was pointed out. Laboratory experiments, measurements of cross sections, and studies of properties of materials were necessary. The development of a large-yield fission device was obviously required.

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. The most difficult of these was, of course, the central problem (which came to be known as the "Super Problem") of whether, and how, and under what conditions a burning might proceed in thermonuclear fuel in the pattern envisaged at the Super Conference. 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 phenomena occurring in 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 of the properties 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.

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.

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 in the understanding 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 (for example: cross-section studies, observation of behavior of fast jets) 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 were believed would be needed in connection with any attempt to estimate the behavior of a thermonuclear system. It was unlikely of itself to reduce the difficulty of undertaking a theoretical estimate, or 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.

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 background type of work referred to above, as well as support of the activities of other sections of the Laboratory, not to mention 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.

END OF:	1946	1947	1948	1949	1950	1951	1952	1953
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:

1. **Consultants.** Ever since the war, the theoretical program at Los Alamos has benefited greatly from the assistance 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 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, 1950, 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.

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

2. Project Matterhorn. In July 1951, J. A. Wheeler established and directed a 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 callaboration with the work at Los Alamos. After the formation of the Livermore Laboratory, Project Matterhorn 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 contact with the AEC and continued administratively unaffected by the termination of the group engaged on weapons studies.)

3. Opacity Group at Argonne Laboratory. The wartime 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 in 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 (1954) 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.

4. 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. This work which in itself would be unclassified assumed importance as necessary data for the detailed consideration of various problems. 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 to the present. Incidentally, among the problems to which Breit has given considerable attention under this arrangement, has been that of checking, refining, and extending the considerations first applied by Teller and Konopinski 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 is 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. A 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 under way at that time and prepare final reports, with the exception of Landshoff, who remains at Los Alamos and continues studies of aspects of the Super Problem. (Landshoff remained at this problem until the summer of 1947.) From mid-July to end of September, Hoyt works on same problem.

September 1946: Teller proposes a new thermonuclear system which later came to be called the TX-14, and Richtmyer takes up problem of estimating performance.

October 1946: Evans takes up studies related to the Super Problem.

November 1946: First TX-14 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 propose possible experiment to check predicted features of thermonuclear burning, and make estimates of feasibility of carrying this out in conjunction with some fission bomb test of moderate yield.

January-February 1947: Richtmyer starts to develop an improved theory of efficiency of the TX-14. 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 the summer of 1947, primary objectives being to: continue studies of burning of fusion fuel, develop Monte Carlo method, initiate work on obtaining detailed calculation of explosion of fission bomb, study the proposed experiment to check ideas on thermonuclear burning, and prepare status report on thermonuclear systems.

May-June 1947: A report on Improved Theory of the TX-14 issued by Richtmyer. Hoyt rejoins study of processes in Super Problem.

July 1947: Nordheim joins Richtmyer in study of TX-14.

August 1947: Efficiency calculations for a number of possible TX-14 configurations completed with Richtmyer's improved theory. Landshoff takes up work on fission bomb explosion calculation.

September 1947: Further TX-14 examples calculated. A report prepared by Teller* describes the status of the studies of the Super and the TX-14. It discusses a variety of possible thermonuclear fuels, and it urges consideration of tests of the sort suggested in January 1947—but in an improved form. Subsequent work on such experiments was directed at the new pattern for which the designation "boosting experiment" or "Booster" came into general use.

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 "Hippo," the work by Landshoff was known as "Baby Hippo.") Preliminary consideration also given to preparing a

*The reference here is to a more comprehensive account of Los Alamos work in this period which was discussed, and given preliminary attention, but never pulled together into a fully coherent "history."

*The following entry appears on the title page of this report: "Work done by: F. Evans, F. Hoyt, R. Landshoff, M. Mayer, L. Nordheim, R. Richtmyer, E. Teller, E. Zadina."

detailed calculation of the Super Problem for handling on the electronic computer expected to be completed at Princeton within a couple of years.

January 1948: The program of analytical study and attempts at numerical solutions (using only desk computing machines) of the Super Problem brought to a close, and results written up. (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 thermonuclear burning, as well as other types of problems.)

March 1948: Richtmyer and von Neumann introduce so-called "viscosity treatment" of shocks. (This technique, which was devised to meet needs arising in connection with Hippo, reduced the problem of calculating 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 begins of behavior of a Booster system (considered either as a test of thermonuclear principles or a possible weapon). Work begins on equation of state of certain materials (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 thermonuclear burning).

September-October 1948: Two reports issued by Reitz and Rosenbluth giving the results of detailed calculations concerning the behavior of possible specified Booster systems. These are the first detailed studies relevant to the proposal to include such a device in the tests then scheduled for 1951. Logical lay-out of calculation of Super Problem begun by Evans, Metropolis, Teller, von Neumann, and Ulam. (From this point on, the planning and preparation of this calculation 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: Detailed discussion starts of plan to prepare a machine calculation (in simplified form) of a particular phase of the Super Problem. (While this proposal would not provide definitive answers to the main questions, it would at least help establish the amount of tritium likely to be required to provide possible ignition conditions for the "classical" Super. By avoiding many of the enormous complications involved in the Super Problem proper, progress could be expected much more rapidly.) A report outlining the steps to be considered in the calculation proposed 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 the spring of 1949, and the computer began effective operation in the spring of 1952.)

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

July 1949: Work starts on improved calculations of the equation of state for hydrogen, required in connection with all thermonuclear studies.

August 1949; Several calculations begin concerning details of behavior of thermonuclear fuel in Booster.

September 1949: Serious worries raised about possible deleterious effects of extraneous processes on behavior of boosting experiment. Bethe and others initiate study of these processes with intention of using results to guide Booster design.

October 1949: Work begun in July on equation of state for hydrogen is completed* and data made available for relevant thermonuclear calculations.

Calculation starts for a device having a new and different pattern. (This was in preparation for studies of the sort which later led to the design of the George Shot at Greenhouse.)

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

November 1949: Full 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.

December 1949: Preparations of the calculation of the Super Problem advance far enough 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 starts on preparation of a machine calculation of the simplified problem proposed in November-December 1948. Further simplified hand calculation of same problem begun by Ulam and Everett to provide information sooner, even though this information would be less precise.

Results of first basic calculation of new pattern proposed in October 1949 become available. Discussion started of choice of parameters for further detailed consideration.

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.)

Consideration of controlling various parameters of Booster indicate ways to relieve difficulties met in November.

January 1950: Study status of range of designs in which boosting experiment might be applicable.

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

Machine calculation for which preparations started in December 1949 further trimmed to fit on the ENIAC, with plan to prepare for first calculation during the spring of 1950. First example of hand calculation continues, with results expected before the end of February.

H. Mayer completes "The Super Pocketbook," 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.

1. Calculation of Details of Fission Explosion. The need of such calculations 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, and also important to understanding the interaction between a fission explosion and the possible ignition of thermonuclear fuel. The advent of the TX-14 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

*This work was embodied in a report written by J. Reitz; with work done by: Bethe, Longmire, M. Mayer, Reitz, M. Rosenbluth, Sternheimer, Teller.

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 originally supposed, and 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 Operation Greenhouse.)

2. Calculations on the Super. The main question, that of the burning of thermonuclear fuel, was studied from the time of the Conference up to the end of 1947. 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 improtant 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.

The much simpler question (analogous to that for which detailed preparations were begun in December 1949) was the one which had received the most specific study at the time of the Conference. Though subsidiary in principle to the main question, it had considerable importance in that it could be learned from study of this problem how much tritium might be required in the bomb. From this information a judgment could be made of whether a Super might be tolerably or prohibitively expensive. At the time of the Conference it was believed that, though the amount of tritium required was likely to be large, it was not prohibitively large, and that modifications of design might enable this amount to be reduced appreciably. In September 1947, after mentioning some adverse effects not previously taken into account, Teller wrote, "Thus, I believe that a total amount of (so much) tritium suffices to set off the Super." (The amount mentioned was roughly twice as large as the amount envisaged at the time of the Super Conference.) This estimate was with respect to a straightforward, but probably wasteful, disposition of tritium; and it was pointed out that by using a more favorable disposition ". . . it is very likely that considerably less tritium will suffice for ignition." Starting late in 1948, plans for a calculation to give a more detailed and realistic picture of a process of this type began to take shape.

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 of the problem referred to above 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 were necessarily on a somewhat subjective basis. In many of the discussions of the amount of tritium required, which proceeded through the fall of 1949 both at Los Alamos and at other places, very small amounts were considered likely to suffice. In this context, the first calculation of Ulam and Everett was started with a rather modest amount. By the end of January 1950, this first calculation was about half complete. Before the end of February 1950, the results showed that the amount of tritium chosen was not nearly enough; so the first calculation was discontinued, its results written up on March 9, 1950, and a second calculation, with a larger amount of tritium, was started immediately.

3. TX-14 Studies. The TX-14 system was proposed by Teller in September 1946. Work started immediately to obtain estimates of ignition conditions and available efficiencies. In a report, issued November 15, 1946, the conclusion is given, "At the present time it seems the proposal is entirely feasible, provided (some possible adverse effects are) not too serious . . . ." Measurements of relevant neutron reaction cross sections were undertaken, and improvements in the theoretical treatment were developed by June 1947 which would enable specific 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 a report: "Because of the great technical difficulties that would be encountered in constructing such a bomb, we have not further pursued this possibility . . . ." The most favorable calculation available at that time indicated the possibility of obtaining 10 megatons from a certain configuration weighing from about 40 to 100 tons. Although a megaton had been used in the calculation to provide the initiating explosion, there were arguments to the effect that a smaller explosion might suffice, possibly as small as 200 kilotons. (The largest explosion then realized had been about 20 kilotons.) In the same report it was suggested that ^{6}LiD might be used as a fuel. This would simplify some problems but require the production of separated lithium and leave the problem of the required initiating explosion to be solved.

In proposing a program of research and development, Teller suggested a number of cross-section measurements; some tests to check predictions of thermonuclear burning on a small scale (of the sort which subsequently became known as "boosting experiments"); and 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 TX-14 behavior; and continued "I think that the decision whether considerable effort is to be put on the development of the TX-14 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 a TX-14 system of the type considered, or of achieving a practically useful object by any means then envisaged, further study of TX-14 was soon laid aside. One of the persons, for instance, who through most of the preceding year had participated in studies related to the TX-14 turned his attention to work required in connection with Operation Sandstone. As mentioned earlier, Richtmyer, who had conducted the detailed study of the TX-14, 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 behavior referred to by Teller.

At the end of January 1950, therefore, the understanding and prospects of the TX-14 were in essentially the state indicated above. Systems of this kind were believed to be feasible in principle, and capable of providing arbitrarily large yields. However, the system required to obtain a significant amplification of the initiating yield was so large and heavy as to appear to be of little practical value.

4. The Booster. The Booster, or the boosting principle, refers to the notion of using a fission bomb to initate a small thermonuclear reaction with the possibility that—in addition to being instructive with respect to our understanding of the processes involved—the neutrons from this reaction might increase the efficiency of the use of the fissile material.

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 by Teller in September 1947.

In the summer of 1948 a detailed study was begun to determine the necessary characteristics of a device in which this interaction between fission and thermonuclear processes might be realized. 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 then 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 and the more promising lines of approach had been identified.

Study of the Booster was continued through 1949 and, starting early in the summer, was greatly intensified. Several 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; Evans, on the ignition and progress of thermonuclear burning; Reitz and others, on equation of state problems; the members of the hydrodynamics calculation group, under Hammer; the various persons who had experience with the neutronics calculations required for standard fission bombs; Bethe, Longmire, and others, to consider possible extraneous processes and to make estimates of their effects; and many others outside the Theoretical Division, to measure cross sections and other quantities required for the calculations, and to solve the mechanical problems involved. 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 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 experiment were frozen late in October 1950.)

5. 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 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 energy 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, which picture can itself only be obtained by a 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.

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 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 wartime 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 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 system chosen was unsuitable, or that the required ignition conditions had not been met. The fantastic requirements on calculation imposed by an attempt to explore the question of the classical Super as envisaged in 1946 did not, of course, apply to the same extent with respect to the thermonuclear devices in the form considered since early 1951; but even those requirements still far exceeded the ones which had to be met for the successful design of fission weapons.

The most complicated calculations available at the time of the Super Conference in 1946 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 quite important effects and various complex physical phenomena were almost necessarily ignored. The results of the calculations 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 wartime 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.), appeared a couple of years later and the UNIVAC in 1951. Then followed the Los Alamos MANIAC and the Princeton machine in 1952, and the IBM 701 in 1953. Los Alamos problems were put on all these machines soon after they became effective. At the present time (1954), 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 was 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 in which 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 comparable in size to merely one of several basic calculations required in connection with the design of a modern thermonuclear 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 handle a single example) 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
1952	MANIAC	2 days	2-3 days
1954	IBM 701	1-2 days	2-3 days
1974[a]		30 min	15 min

[a]As a matter of interest this entry has been added to the Table while preparing the unclassified version of this account (1974) to indicate the further progress on this point since 1954. The major part of the elapsed time indicated here is required to prepare and check the input numbers, gain access to the machine, and wait while the printer lists the results.

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 calculations 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 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 or more 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.

6. **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 under way 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 studies or development of processes or techniques. Some work related to the thermonuclear field (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. Because of this, 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 everything, 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 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 have proceeded with less ingenuity, and at a slower pace, without the benefit of his keen physical insight and contagious enthusiasm.

*Many instances of relevant work carried out in P Division during this period could be listed. The cross sections of many light element reactions were measured to be sure that possible reactions of weapon value were not overlooked. See, for example, the measurement of the cross section of the T+T reaction, performed by Agnew, Leland, Argo, Crews, Hemmendinger, Scott, and Taschek, and reported in Phys. Rev. **84**, 862 (1951).