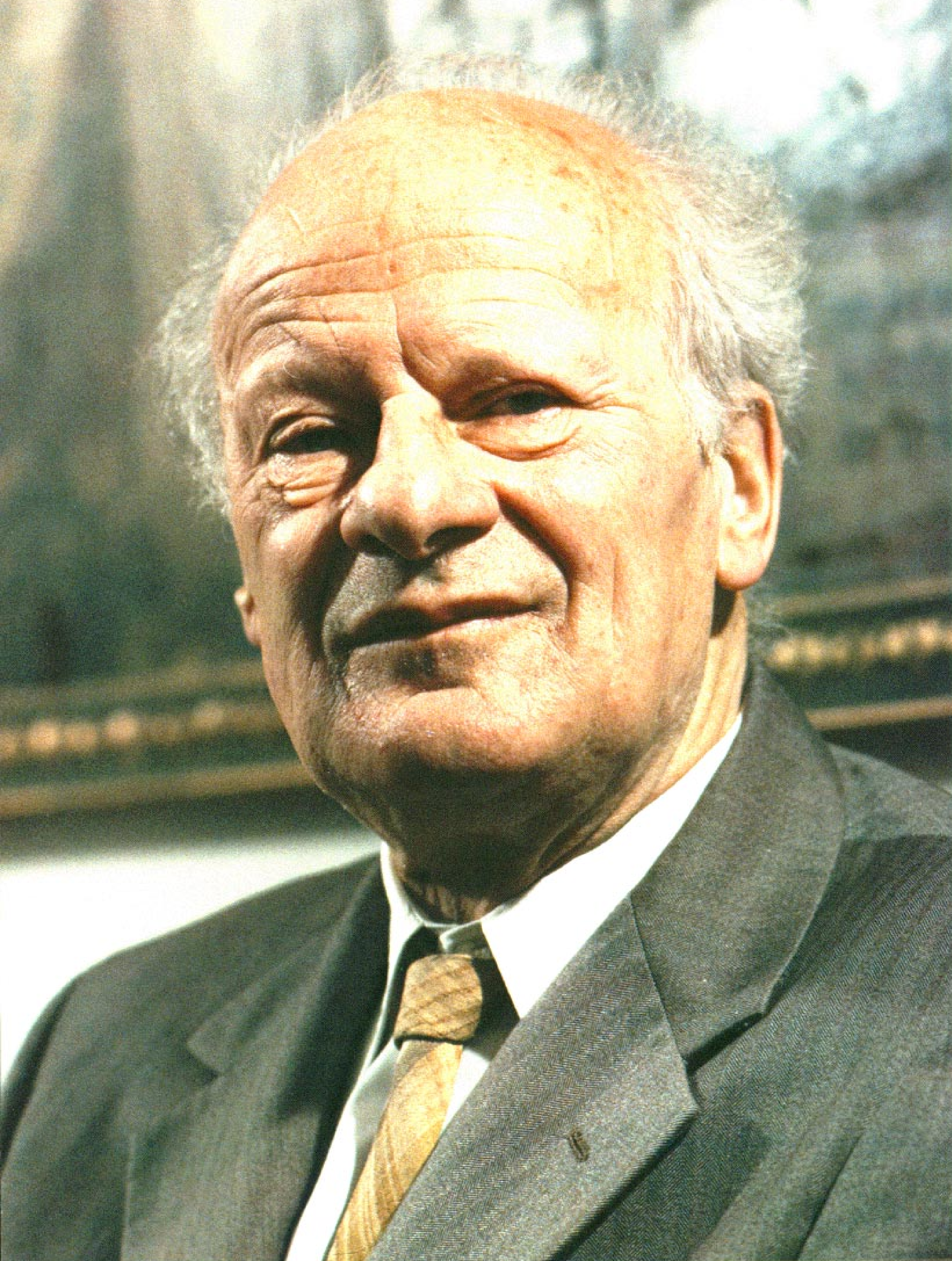

Comments on The History of the H-Bomb

by Hans A. Bethe

Theoretical Division Leader, Los Alamos, 1943-45
Consultant, Los Alamos, 1946-Present

Back in 1954 I wrote an article on the history of the H-bomb, stimulated by a book by Shepley and Blair which gave an entirely distorted view of that history. It took until recently to have that article declassified. I had intended to put this article into the Laboratory's archives and not to publish it, in order not to stir up old controversies. However, now there has appeared the very popular book by Peter Goodchild, *J. Robert Oppenheimer: Shatterer of Worlds.* While this book is excellent in most respects, it gives among others a very wrong impression of the development of the H-bomb. Therefore, I am now publishing this article, and I have added a few remarks specifically correcting some of the mistakes in Goodchild's book. What follows is a (slightly edited) version of the 1954 article, which was written in some anger about certain events of 1953-54.

. . .The first of these events was an article by C. J. V. Murphy* in *Fortune* of May 1953 which presented a highly biased and inaccurate picture of the H-bomb development and of the efforts of many American scientists to establish a more adequate air defense system for this country. Next came the most important event, the Oppenheimer case. The hearings on this case, and their unexpected publication by the Atomic Energy Commission, have made the general public aware of the deep conflicts which, at various times, arose in connection with the thermonuclear development. Fortunately, the record of the Oppenheimer hearings contains testimony which enables anyone who takes the trouble to read through its 992 pages to form his own opinion on the issues.

Now, however, [that is, in 1954] a book has appeared which requires an immediate answer. It is written by James R. Shepley and Clay Blair, Jr., and purports to tell the American public the history of the hydrogen bomb. Apart from official public statements, which were in any case not particularly informative on the matters discussed so freely by the authors, the information and opinions presented in the book have obviously been obtained from persons holding extreme views on a number of matters. Whoever these persons may have been, they were extreme in their dislike and/or distrust of Oppenheimer, extreme in their certainty of the malfeasance of Los Alamos, extreme in their conviction that anyone who expressed misgivings or raised questions concerning the wisdom of committing ourselves to the H-bomb program was ipso facto subversive. As a result, the book is full of misstatements of fact, and so phenomenally biased as to retain little contact with the events that actually occurred.

Many of the readers of the book will be familiar, from other reports, with some of the political moves on the H-bomb project that went on in Washington. The book is made only more misleading because it reports a number of these moves outwardly accurately, as far as I can judge. Many readers may thereby be misled into believing that the progress of the technical work is also reported correctly by Shepley and Blair. With very few exceptions this is not so; and the fact that the technical history was different puts a completely different light on the reasons and justification for various "political" moves, e.g., on the agitation for the establishment of a second weapons laboratory.

In this article I will talk in the main about the technical history of the project since this is the only subject which I know first-hand. Unfortunately, any factual account of technical development must be incomplete because large parts of the subject remain classified. Many of the points in this article would become even more convincing if classified matters could be discussed.

I shall not attempt to give an exhaustive list of the misstatements of fact in the Shepley-Blair book. On many matters reported in the book I have no first-hand knowledge. Even where I do have such knowledge, I shall leave out much detail, as well as much that is still classified, and, finally, many of the points that were discussed by Dr. Bradbury in his excellent press statement and press conference which were published in *The New Mexican* of Santa Fe, New Mexico, on Friday, September 24, and Sunday, September 26, 1954.

At various points in this article, reference will be made to the book by Shepley and Blair, which will be quoted as SB with the page number. Reference will also be made to testimony in the Oppenheimer case, which will be quoted as OT with the page number in the official publication.

The historical material is arranged under three major headings: Wartime development, Postwar development of fission bombs, and Thermonuclear weapons. In these sections I try to follow the historical sequence and mention SB as I go along. In a fourth section

This article and interviews of Mr. Murphy with persons concerned are quoted as one of their chief sources of information by Shepley and Blair. (Letter to the Editor of the* New York Herald-Tribune, *October 15, 1954.)

I discuss the things which were required before success in a thermonuclear program could be achieved.

1. Los Alamos During Wartime

After the Los Alamos Laboratory was started in the Spring of 1943, it became clear that the development of a fission bomb was far more difficult than had been anticipated. If our work was to make any contribution to victory in World War II, it was essential that the whole Laboratory agree on one or a very few major lines of development and that all else be considered of low priority. Teller took an active part in the decision on what were to be the major lines. Before any specific work of an engineering or design nature could be taken up, it was necessary that theoretical investigations be brought to the stage where they could provide some detailed guidance. A distribution of work among the members of the theoretical division was agreed upon in a meeting of all scientists of the division, and Teller again had a major voice.

In the early Summer of 1944, the Laboratory adopted as its main line the development of the implosion, a method since described publicly, e.g., in the testimony in the Greenglass trial and in instructions to U.S. Customs and Postal Officials for the purpose of helping them to detect clandestine import of atomic bomb parts.

As soon as the implosion method was proposed by Neddermeyer, Teller advocated that the Laboratory should devote major effort to its development. In 1944 he was given the responsibility for all theoretical work on this problem. Teller made two important contributions. He was the first to suggest that the implosion would compress the fissile material to higher than normal density inside the bomb. Furthermore he calculated, with others, the equation of state of highly compressed materials, which might be expected to result from a successful implosion. However, he declined to take charge of the group which would perform the detailed calculations of the implosion. Since the theoretical division was very shorthanded, it was necessary to bring in new scientists to do the work that Teller declined to do. Partly for this reason, some members of the British Atomic Energy team, already working in the U.S. on other aspects of the Manhattan District project, were brought to Los Alamos and asked to help with this problem. The leader of the British theoretical group was Rudolf Peierls, and another very hardworking member was Klaus Fuchs.

With the pressure of work and lack of staff, the theoretical division could ill afford to dispense with the services of any of its members, let alone one of such brilliance and high standing as Teller. Only after two failures to accomplish the expected and necessary work, and only on Teller's own request, was he, together with his group, relieved of further responsibility for work on the wartime development of the atomic bomb. This was done by me, as the Leader of the Theoretical Division, not by Oppenheimer, the Director of the Laboratory.

About this same development Shepley and Blair have the following to say (page 40): "Edward Teller also worked at Los Alamos during the war. But because Oppenheimer did not like him personally-a fact that was perhaps traceable to their differing political views—Teller was denied a specific job in connection with the development of the atomic bomb." It is obvious that this is almost the exact opposite of the truth.

It is difficult to judge another man's personal feelings toward a third, even if you see both of them almost daily. But as far as I could see, the personal relations between Teller and Oppenheimer were very good at the beginning of Los Alamos. Later on, Teller's attitude toward his own work and toward the program of the Laboratory created a strain in his relations with Oppenheimer, and, to a lesser degree, in his relations with myself. At the start I had regarded Teller as one of my best friends and as the most valuable member of my division. Our relation cooled when Teller did not contribute much to the work of this division. More important perhaps for a disturbance of relations was his wish to spend long hours discussing alternative schemes which he had invented for assembling an atomic bomb or to argue about some remote possibilities why our chief design might fail. He wanted to see the project being run like a theoretical physics seminar and spent a great deal of time talking and very little time doing solid work on the main line of the Laboratory. To the rest of us who felt we had a vital job to do, this type of diversion was irksome. To come back to the relations between Teller and Oppenheimer, politics certainly played no role in them. Communism in particular was no issue at that time at Los Alamos.

The success of Los Alamos rested largely on its teamwork and the leadership of its director. Shepley and Blair do not wish to give credit to Oppenheimer because (footnote on page 28) "the technical contributions at wartime Los Alamos" were not made by him. It is not the primary function of the director of a laboratory to make technical contributions. What was called for from the Director of Los Alamos at that time was to get a lot of "prima donnas" to work together, to understand all the technical work that was going on, to make it fit together, and to make decisions between various possible lines of development. I have never met anyone who performed these functions as brilliantly as Oppenheimer, as Goodchild rightly emphasizes.

The individuals mentioned in the footnote on page 28 of SB as having made "the technical contributions at wartime Los Alamos" are an odd collection. Some, like von Neumann, really did contribute most important ideas. Other very important names like Kistiakowsky, Bradbury, Bacher, Rossi, Cyril Smith, R. R, Wilson, Feynman, et al., are omitted. Instead, the footnote mentions two persons who did not work significantly on the A-bomb at Los Alamos, but almost exclusively on the H-bomb.

The implosion, which has been mentioned as the main program of the Laboratory, consists of placing a large quantity of high explosive around the surface of a small sphere of uranium-235 or plutonium. This method was invented *during* the war, while SB, page 115, make it appear as if this method had been invented only in 1950. Also, the idea of using a fraction of a critical mass (fractional crit) for an atomic explosion orginated during the war; it was not "sparked by Teller's intuition" in 1950. Rather, it was common knowledge and strongly advocated by the Los Alamos Laboratory, and by the Atomic Energy Commission, in 1948-49. The idea developed from the same implosion calculations which Teller had refused to perform. I believe in fact that I was the first to point out this possibility but it is true that Teller quickly supported it, all in 1944. However, it was not until the art of fission bombs had been thoroughly developed by the postwar Los Alamos Laboratory that the fractional crit became a practical possibility. In other words this scheme had long been on the Los Alamos books and was waiting only for the perfection of techniques. To give Teller and the year 1950 credit for this idea as SB do on page 115 is entirely false.

There are two interesting sidelights on the accuracy of SB's reporting. In the first place, the important development of the fractional crit weapon had no bearing on the thermonuclear work at all, contrary to SB's statement. Secondly, SB claim that the General Advisory Committee [a nine-man committee, established in 1947 and chaired by Oppenheimer until 1952, that advised the AEC on scientific and technical matters] was against fractional crit weapons. If they were, Oppenheimer must have had a badly split personality because the Vista report, with which Oppenheimer was prominently identified but which SB and their trusted colleague, C. J. V. Murphy, have criticized so much, recommended fractional crit weapons as a mainstay of our arsenal.

2. Postwar Development of Fission Bombs

It has been made amply clear in the Oppenheimer testimony and elsewhere that at the end of the war the number of scientists at Los Alamos declined severely and that this was especially true of the number of senior staff members. The theoretical division, which has the main responsibility for the conceptual design of weapons, was reduced from over thirty scientists to eight in 1946 (according to Bradbury's press statement); it has since increased again to over fifty [in 1954]. This decline was part of the general movement to "let the boys come home." We all felt that, like the soldiers, we had done our duty and that we deserved to return to the type of work that we had chosen as our life's career, the pursuit of pure science and teaching.

The older ones among us felt a heavy responsibility to our teaching. Wartime had shown that this country had a very short supply of competent scientists, and Los Alamos was one of the best examples. The young scientists whose careers had been interrupted by the war wanted to get training under the G.I. Bill of Rights. The largest graduate schools in physics before the war had about fifty graduate students; now this number jumped to a hundred and, in some universities, to over two hundred. The great effort which was made in training these young people has borne fruit in the meantime. Only because of it could laboratories like Los Alamos gather their large staff of highly competent scientists in the years since 1948. Only in this way could the Los Alamos theoretical division grow to its present [1954] 50-odd members, not to speak of the important work that other young scientists are doing in industry, in other governmental laboratories, and in the universities themselves.

For most of the scientists, young or old, who participated in the wartime work at Los Alamos, this was their first experience with work of a secret nature or work having immediate practical military significance. It is in no way surprising that most of them preferred the free interchange of ideas with their colleagues in this country and abroad which goes with pure, non-secret research. Moreover, it was not obvious in 1946 that there was any need for a large effort on atomic weapons in peacetime. All these factors help explain the exodus of scientists from Los Alamos and other wartime projects in 1946. The most effective cure for this attitude was the behavior of Russia in the first years after the war. For many scientists one of the most convincing points in the Russian behavior was their negative attitude toward our offer to make atomic power and atomic weapons an international rather than a national development, a plan to which Shepley and Blair (page 170) refer as the scientists wanting "to give the secrets of the A-bomb to the world". Most scientists soon recognized that the Russians were not willing to open the Iron Curtain to an International Atomic Authority and Oppenheimer was one of the first to recognize this, as has been demonstrated amply in the Oppenheimer testimony. The negotiations in the U.N. Atomic Energy Commission, as much as anything else, made many of the wartime members of the Los Alamos Laboratory willing to return to weapons work at least on a part-time basis.

The fact remains that in 1946 the Los Alamos Laboratory was very weak. To demand, as Teller did as a condition for his staying, that Los Alamos tackle the super-bomb on a large scale, or plan for twelve tests a year on fission bombs, was plainly unrealistic to say the least. Dr. Bradbury, in his statement of September 24, 1954, pointed out that only as late as 1951 could a schedule of twelve test shots be reached. In only one subsequent year, 1953, was the firing of such a large number again found necessary. It is hardly possible to give enough credit to the small group of scientists who decided to stay at Los Alamos in 1946 without making demands beyond the Laboratory's capacity.

The development laboratory at Los Alamos was not the only part of the atomic energy program which was hard hit immediately after the war. The very production of bombs of the existing models also declined severely. It has been reported, e.g., in SB page 53, that only a very small stockpile of atomic bombs existed when the AEC took over from the Manhattan District on January 1, 1947. Shepley and Blair, by being unclear about dates, find here one of their opportunities for conveying a false impression while not actually making a false statement. A casual reading of their remarks on page 53 gives the impression that Oppenheimer expressed himself as satisfied with the status of the weapons program as of January 1947. If you read carefully, however, you find that his satisfaction was expressed as of the Summer of 1949, a time when great strides had been made in the A-bomb program.

As soon as the AEC took over, it and the General Advisory Committee, under the chairmanship of Oppenheimer, considered the weapons program their most important task. This is amply shown by the testimony in the Oppenheimer case. SB, pages 114 and 115, state that the GAC, and many other scientists, when they opposed the H-bomb advocated the improvement of atomic bombs, "though" (they had) "not" (done so) "before." Of course, this advocacy of better A-bombs was not made in public, but in the privacy of its reports the GAC recommended improved A-bombs from the beginning of its existence, which was shortly after the AEC took over from the military.

Already in the interim period of 1946, but especially when they received the full support of the AEC and GAC in 1947, Los Alamos set out to work on the improvement of A-bomb design. This work bore fruit as early as 1948 in the "Sandstone" tests. SB on page 100 quote a statement by Senator Johnson that the Sandstone bombs were already improved by a factor of 6 over the wartime A-bomb. I can neither confirm nor deny the accuracy of this figure or any other figures given in SB because such figures are classified. But, assuming the statement by SB to be correct, I submit that this was a tremendous achievement of the Los Alamos Laboratory in so short a span of time.

Immediately after the results of the Sandstone tests were known, the Los Alamos Laboratory began planning further improvements in fission bombs. It was also planned that these improved designs would be tested in another test series in the Pacific, and the approximate date of that series, known later as Greenhouse, was agreed upon. It must be realized that a long time is required between the first conceptual design* and the final test of an improved weapon.

First, theoretical calculations have to be done; then a great deal of experimentation, including non-nuclear explosions, is necessary to test the soundness of the theoretical concept; simultaneously fabrication techniques may have to be developed; then a final design must be made and fabricated; and finally elaborate preparations must be made for observing the performance of the weapon at the test and for the test itself. No such development can be accomplished in a few months as has often been implied in newspaper speculations on A- and H-bomb development. It is true that now with extensive experience and expanded resources such developments can be made much more rapidly than they used to; but planning in 1948 and 1949 for a major test series in Spring 1951 seemed then a fairly strenuous time scale.

Advanced designs of A-bombs, conceived at Los Alamos in 1948 and 1949 and tested in 1951, included weapons of small diameter. This idea was proposed by Los Alamos and most vigorously supported by the AEC and the GAC. There was little interest in it among the military at first, but now [1954] they are clamoring for more of these weapons. This throws some light on the remark of SB, page 10, that "The military was. , , uneasy about the development of weapons. "

It also throws light on the charge that Los Alamos was "over-cautious" (SB page 144) and therefore slow. The goal in technical development is usually reached faster if the development is methodical and sustained and if mistakes are avoided, than if novel schemes are pursued before the groundwork has been laid.

"Conceptual design" involves a general decision on the properties of a weapon to be developed, including its power and its approximate geometric arrangement.

3. The Development of the H-Bomb

The H-bomb was suggested by Teller in 1942. Active work on it was pursued in the summer of 1942 by Oppenheimer, Teller, myself, and others (see Oppenheimer's testimony). The idea did not develop from Teller's "quiet work" at Los Alamos during the war as claimed by SB, pages 40 and 45.

When Los Alamos was started in Spring 1943, several groups of scientists were included who did work on this problem specifically. However, it was realized that this was a long-range project and that the main efforts of Los Alamos must be concentrated on making A-bombs (see Section 1). Teller, working on the H-bomb at Los Alamos, discovered a major difficulty (testimony by Oppenheimer). This discovery made it clear that it would be a very hard problem to make a "classical super" work, as this type of H-bomb was called. I shall refer to the classical super as Method A.

It was decided to write down, at the end of the war, an extensive record of the technical knowledge of the entire Los Alamos project. In line with this effort, it seemed also desirable to record the status of the "Super" so that work on it could be resumed the better when more manpower and other requisites were available. A summary report on this subject was written by Teller's collaborators in 1946 which turned out to be very useful for later work. I believe (but I am not sure because I was not present at Los Alamos at that time) that the conference on the Super in April 1946 also was intended partly to provide a record for the future (particularly since almost all the persons who had been working on this program had made definite plans to return to academic or non-weapon work), and possibly in addition to get some physicists from outside Los Alamos who were attending the conference interested in the problems with the hope that they might continue to work on them, theoretically and rather quietly. SB on page 55 present this conference as "a last-minute effort ... to spur the government into proceeding further with the H-bomb."

The work on thermonuclear weapons at Los Alamos never stopped. At this stage of the development, the main requirements were for theoretical work and for a few experimental physics measurements. Both of these types of work went ahead. On the basis of the monthly reports of the theoretical division of Los Alamos, it has been estimated that between 1946 and 1949 the work of that division was about equally divided between fission weapon design and problems related to thermonuclear weapons. (In this respect I was mistaken when testifying in the Oppenheimer case. I said then, from memory, that a relatively small fraction of the scientists of the division, though consisting of especially able men, were working on thermonuclear problems. Actually, the fraction was large.)

Two new methods of designing a thermonuclear weapon were invented (Methods B and C). Both inventions were due to Teller. Method B was invented in 1946, Method C in 1947. Method B was actively worked on by Richtmyer, Nordheim, and others. However, at the time, there seemed to be no way of putting Method B into practice, as Dr. Bradbury has mentioned in his statement to *The New Mexican.* Teller himself wrote a most pessimistic report on the feasibility of this method in September 1947.

Method C is different from all the others in that thermonuclear reactions are used only in a minor way, for weapons of relatively small yield. This method seemed quite promising from the start, and as early as the Summer of 1948 it was added to the devices to be tested in the Greenhouse tests.

Theoretical work on the "classical super," Method A, proceeded continually, since this method was considered the most important of all thermonuclear devices. New plans for calculations were made frequently, mostly by consultation between Teller and the senior staff of the theoretical division. However, as Teller stated in 1946, "The required scientific effort is clearly much larger than that needed for the first fission weapon." In particular, the theoretical computations required were of such complication that they could not be handled in any reasonable time by any of the computing machines then available. Some greatly simplified calculations were done but it was realized that they left out many important factors and were therefore quite unreliable. Work was therefore concentrated on preparing full-scale calculations "for the time when adequate fast computing machines become available"-a sentence which recurs in many of the theoretical reports of this period. The plans for such a calculation on Method A were laid in September 1948, and the mathematical work was virtually completed by December 1949—all *before the* directive of President Truman-but it was not until mid-1952 that adequate computing machines finally became available, and by that time the most capable of them were fully engaged on the new and more promising proposal (Method D) discussed below.

When Dr. Teller and Admiral Strauss proposed in the Fall of 1949 to start a full-scale development of H-bombs, the method in their minds, as well as in the minds of the opponents of the program, was Method A. To accomplish Method A, two major problems had to be solved which I shall call Part 1 and Part 2. Part 1 seemed to be reasonably well in hand according to calculations made by Teller's group from 1944 to 1946 although nobody had been able to perform a really convincing calculation, as discussed in the paragraph above. Teller now believed that he had a solution for Part 2. In principle, the accomplishment of Part 2 had never been seriously in doubt, although the question of whether or not any particular device would behave in the way required could not be settled without experiment.

The Greenhouse thermonuclear experiment mentioned in SB was designed to test Part 2. After President Truman made the decision to go ahead with a full-scale thermonuclear program, Los Alamos made plans to add to the Greenhouse test series an experiment intended to test a particular proposal relating to Part 2. Teller played a large part in the specification of this device, and as it turned out it behaved very well. However, as on previous occasions, Teller did not do so well in directing the detailed theoretical work of his group. Only as late as January 1951, a month or so before the test device had to be shipped to the Pacific, was the full theoretical prediction of the (probably successful) behavior of the device available. But even while complete theoretical proof was lacking, most of us connected with the work at Los Alamos were confident that the Greenhouse experiment would work. As far as I could make out, at a meeting at Los Alamos in October 1950 which I attended as a guest, this was also the opinion of the GAC including Dr. Oppenheimer. Shepley and Blair instead report on page 116 that Dr. Oppenheimer expected the test device to fail. (The correct story on Oppenheimer's attitude will be discussed below.)

A very large fraction of the members of the Los Alamos Laboratory, not just a "small handful of his" (Teller's) "associates" as SB say on page 115, were extremely busy from Spring 1950 to Spring 1951 with the preparation of Teller's thermonuclear experiment. They did this in addition to preparing the Nevada tests of early 1951. The hundreds of scientists and technicians who worked for months to get the Greenhouse test ready will not enjoy Shepley and Blair's reference (page 116) to the Laboratory's "unwillingness to get involved in Teller's work."

The major feature of the year 1950 was, however, the discovery that *Part 1 of Method A was by no means under control.* While Teller and most of the Los Alamos Laboratory were busy preparing the Greenhouse test, a number of persons in the theoretical division had continued to consider the various problems posed by Part 1. In particular, Dr. Ulam on his own initiative had decided to check the feasibility of aspects of Part 1 without the aid of high-speed computing equipment. He, and Dr. Everett who assisted him, soon found that the calculations of Teller's group of 1946 were wrong. Ulam's calculations showed that an extraordinarily large amount of tritium would be necessary, as correctly stated by SB on page 102. In the Summer of 1950 further calculations by Ulam and Fermi showed further difficulties with Part 1.

That Ulam's calculations had to be done at all was proof that the H-bomb project was not ready for a "crash" program when Teller first advocated such a program in the Fall of 1949. Nobody will blame Teller because the calculations of 1946 were wrong, especially because adequate computing machines were not then available. But he was blamed at Los Alamos for leading the Laboratory, and indeed the whole country, into an adventurous program on the basis of calculations which he himself must have known to have been very incomplete. The technical skepticism of the GAC on the other hand had turned out to be far more justified than the GAC itself had dreamed in October 1949.

We can now appreciate better the attitude of the GAC, and indeed of most of the members of Los Alamos, to the Greenhouse thermonuclear test. They did not expect it to fail, but they considered it as irrelevant because there appeared to be no solution to Part 1 of the problem. The correct description of this attitude is given by Oppenheimer in his own testimony, OT page 952.

The lack of a solid theoretical foundation was the only reason why the Los Alamos work might have seemed to some to have gotten off to a slow start in 1950 (SB page 114). Purely theoretical work may seem slow in a project intended to develop "hardware," but there was simply no basis for building hardware until the theory had been clarified. As far as the mental attitude of Los Alamos in early 1950, it was almost the exact opposite of that described by Shepley and Blair. I visited Los Alamos around April 1, 1950 and tried to defend the point of view of the GAC in their decision of October 1949. I encountered almost universal hostility. The entire Laboratory seemed enthusiastic about the project and was working at high speed. That they continued to work with full energy on Teller's Greenhouse test, after Ulam's calculations had made the success of the whole program very doubtful, shows how far they were willing to go in following Teller's lead.

Teller himself was desperate between October 1950 and January 1951. He proposed a number of complicated schemes to save Method A, none of which seemed to show much promise. It was evident that he did not know of any solution. In spite of this, he urged that the Laboratory be put essentially at his disposal for another year or more after the Greenhouse test, at which time there should then be another test on some device or other. After the failure of the major part of his program in 1950, it would have been folly of the Los Alamos Laboratory to trust Teller's judgment, at least until he could present a definite idea which showed practical promise. This attitude was strongly held by most of those on the permanent staff of the Laboratory who were responsible for its operation. As might be expected, the many discussions of aspects of this situation bred considerable emotion.

Between January and May 1951, the "new concept" was developed, (This I shall call Method D.) SB, page 119, say of this period "Teller found it impossible to get the necessary help at Los Alamos to carry on with his 'new concept' at the pace he thought the idea and program deserved." It would not have been surprising if this had been the case and if, after the major effort the Laboratory had made to prepare the Greenhouse test on Part 2, which to everybody's understanding had lost the major part of its point before the test was fired, there might have been some hesitation about immediately becoming committed to a large-scale effort along a new line of inquiry. In addition, it should be remembered that between January and May both tests in Nevada and the Greenhouse series of tests took place, and this required many senior members of the Laboratory to be at the test sites for prolonged periods of time and the attention of many others was engaged on study of results of these tests.

But what are the actual facts about this alleged delay in work on the new concept? In January Teller obviously did not know how to save the thermonuclear program. On March 9, 1951, according to Bradbury's press statement, Teller and Ulam published a paper which contained one-half of the new concept. As Bradbury has pointed out, Ulam as well as Teller should be given credit for this, Ulam, by the way, made his discovery while studying some aspects of fission weapons. This shows once more how the important ideas may not come from a straightforward attack on the main problem.

Within a month, the very important second half of the new concept occurred to Teller, and was given preliminary checks by de Hoffman. This immediately became the main focus of attention of the thermonuclear design program.

It is worth noting that the entire new concept was developed before the thermonuclear Greenhouse test which took place on May 8, 1951. The literature is full of statements that the success of Greenhouse was the direct cause of the new concept, This is historically false. Teller may have been influenced by thinking about the Greenhouse design when developing the new concept, but the success of Greenhouse (which was anticipated) had no influence on either the creation of the new concept, or on its quick adoption by the Laboratory or later by the GAC. The new concept stood on its own.

As early as the end of May 1951, I received from the Associate Director of Los Alamos a detailed proposal for the future program of the Laboratory in which Teller's new concept figured most prominently. By early June, when I visited Los Alamos for two weeks, everybody in the theoretical division was talking about the new concept.

Not only was the acceptance of the new concept not slow; but the realization of the development was a sensationally rapid accomplishment. in the same class as the achievement of Los Alamos during the war.

The impression is given in SB, pages 119-21, that Los Alamos would not have put major effort on the new concept so quickly if it had not been directed to do so by Gordon Dean, then Chairman of the AEC. Actually, Teller's new concept was so convincing to any of the informed scientists that it was accepted very quickly anyway. Certainly the events of the year 1950 would hardly seem to have given Teller any justification to ask the AEC, in the Spring of 1951, to establish a second weapons laboratory to compete with Los Alamos, as he did according to SB, page 120. (I read for the first time in the book by Shepley and Blair that Teller had asked for the second laboratory as early as Spring 1951. I did not hear of this proposal until the end of that year, although Teller was arguing both at Los Alamos and in Washington through the Spring of 1951 that the requirements of the thermonuclear program could only be met if the Los Alamos Laboratory underwent a major reorganization.)

The immediate acceptance of Method D by the AEC and GAC has been described in the Oppenheimer testimony. This meeting is quite incorrectly described in SB on page 135. It was not a "mass meeting". Invitations were issued only to persons directly concerned with the program, not to "any. ., scientist who wished to attend." This would obviously have been against all security regulations. Many scientists besides Teller took part in explaining the method. The meeting by no means started out in gloom, because most participants (including some members of the GAC) had some advance knowledge of the new concept. It did not require much persuading to make the GAC accept the new concept. "If this had been the technical proposal in 1949," (they) "would never have opposed the development" (Oppenheimer testimony). Now at last there was a sound technical program, and now immediately the GAC and everybody else connected with the program agreed with it. The Oppenheimer testimony shows that the GAC went beyond the Los

Alamos recommendations in allocating money for the support of the new concept.

It is difficult to describe to a non-scientist the novelty of the new concept. It was entirely unexpected from the previous development. It was also not anticipated by Teller, as witness his despair immediately preceding the new concept. I believe that this very despair stimulated him to an invention that even he might not have made under calmer conditions. The new concept was to me, who had been rather closely associated with the program, about as surprising as the discovery of fission had been to physicists in 1939. Before 1939 scientists had a vague idea that it might be possible to release nuclear energy but nobody could think even remotely of a way to do it. If physicists had tried to discover a way to release nuclear energy before 1939, they would have worked on anything else rather than the field which finally led to the discovery of fission, namely radiochemistry. At that time, concentrated work on any "likely" way of releasing nuclear energy would have led nowhere. Similarly, concentrated work on Method A would never have led to Method D. The Greenhouse test had a vague connection with Method D but one that nobody, including Teller, could have foreseen or did forsee when that test was planned. By a misappraisal of the facts many persons not closely connected with the development have concluded that the scientists who had shown good judgment concerning the technical feasibility of Method A were now suddenly proved wrong, whereas Teller, who had been wrong in interpreting his own calculations was suddenly right. The fact was that the new concept had created an entirely new technical situation, Such miracles incidentally do happen occasionally in scientific history but it would be folly to count on their occurrence. One of the dangerous consequences of the H-bomb history may well be that government administrators, and perhaps some scientists, too, will imagine that similar miracles should be expected in other developments.

Before the end of the Summer of 1951, the Los Alamos Laboratory was putting full force behind attempts to realize the new concept, However, the continued friction of 1950 and early 1951 had strained a number of personal relations between Teller and others at Los Alamos. In addition, Teller insisted on an earlier test date than the Laboratory deemed possible. There was further disagreement between Teller and Bradbury on personalities, in particular on the person who was to direct the actual development of hardware. Bradbury had great experience in administrative matters like these. Teller had no experience and had in the past shown no talent for administration. He had given countless examples of not completing the work he had started; he was inclined to inject constantly new modifications into an already going program which becomes intolerable in an engineering development beyond a certain stage; and he had shown poor technical judgment. Everybody recognizes that Teller more than anyone else contributed ideas at every stage of the H-bomb program, and this fact should never be obscured. However, as an article in *Life* of September 6, 1954, clearly portrays: Nine out of ten of Teller's ideas are useless. He needs men with more judgment, even if they be less gifted, to select the tenth idea which often is a stroke of genius.

It has been loosely said that the people at Los Alamos couldn't "get along" with Teller and it might be worthwhile to clarify this point. Both during the difficulties of the wartime period and again in 1951, Teller was on excellent terms with the vast majority of the scientists at Los Alamos with whom he came in contact in the course of the technical work. On both occasions, however, friction arose between him and some of those responsible for the organization and operation of the Laboratory. In each case, Teller, who was essentially alone in his opinion, was convinced that things were hopelessly bad and that nothing would go right unless things were arranged quite differently. In each case, the Laboratory accomplished its mission with distinction. In September 1951, when the program for a specific test of the new concept was being planned, Teller was strongly urged to take the responsibility for directing the theoretical work on the design of Mike. But he felt sure the test date should be a few months earlier; he didn't like some of the people with whom he would have to work; he was convinced they weren't up to the job; the Laboratory was not organized properly and didn't have the right people. Teller decided to leave and left. The Mike shot went off exactly on schedule and was a full success.

It took much more than the idea of the new concept to design Mike. Major difficulties occurred in the theoretical design in early 1952, which happened to be a period when I was again at Los Alamos. They were all solved by the splendid group of scientists at Los Alamos.

At this time more than one-half of all the development work of the Los Alamos Laboratory went into thermonuclear weapons and into the preparation of the Mike test in particular. All but a small percentage of the theoretical division were thinking about this subject. In addition, there was a group of theorists working in Princeton under the direction of Professor John A. Wheeler in collaboration with the theoretical group at Los Alamos. Shepley and Blair, however, have to say of this period (on page 141) "Progress on the thermonuclear program still lagged."

Teller "helped" at this time by intensive agitation against Los Alamos and for a second laboratory. This agitation was very disturbing to the few leading scientists at Los Alamos who knew about it. Much precious time was spent in trying to counteract Teller's agitation by bringing the true picture to Washington. I myself wrote a history of the thermonuclear development to Chairman Dean of the AEC which was mentioned in the Oppenheimer testimony. This loss of time could be ill afforded at a time when the technical preparations for Mike were in a crisis.

Nevertheless, the theoretical design of Mike was completed by June 1952 in good time to make the device ready for test on November 1. Not only this, but, in the same period, much work was done leading to the conceptual design of the devices which were later tested in the Castle series in the Spring of 1954. The approximate date for the Castle tests was also set at that time, and it was planned then that it should lead to a deliverable H-bomb if the experimental Mike shot was successful. It is necessary always to plan approximately two years ahead. Between Summer 1952 and Spring 1954, theoretical calculations on the proposed thermonuclear weapons proceeded; they were followed and in some cases paralleled by mechanical design of the actual device and finally followed by manufacture of the "hardware."

In July 1952, the new laboratory at Livermore was officially established by the AEC. Its existence did not, and in fact could not, accelerate the Los Alamos work because in all essentials the work for Castle had been planned before Livermore was established. In August 1952 an additional device was conceived at Los Alamos which might possibly have been slightly influenced by ideas then beginning to be considered at Livermore. In addition, Los Alamos decided to make a few experimental small-scale shots in Nevada in the Spring of 1953, and this program may have been slightly stimulated by the existence of Livermore. Livermore did assist in the observation of the performance of some of the devices tested at Castle.

Concerning the performance of Livermore's own designs, I will only quote the statement of Dr. Bradbury to the press which says, "Every successful thermonuclear weapon tested so far" [1954] "has been developed by the Los Alamos Laboratory."* This statement has not been contradicted.

(Note added in 1982: In the intervening 28 years, Livermore has contributed greatly to nuclear weapons development. Some weapons programs are assigned to Livermore, some to Los Alamos, and the talents of the two laboratories complement each other.)

4. Requisites for the Thermonuclear Program

The requirements for a successful thermonuclear program were four. First, there had to be an idea; second, there had to be many competent people who could work together in a team and could carry out this idea; third, there had to be well-developed, highly efficient fission bombs; fourth, there were needed high-speed computing machines.

The development of the idea has been dealt with in the last section. As far as people were concerned, Dr. Bradbury showed in his press conference that during 1950 the number of scientists in the theoretical division increased from 22 to 35. This is in striking contrast to the statement of Shepley and Blair (footnote on page 104), "The roster of theoreticians at the weapons laboratory actually declined during 1950, the year of President Truman's decision to build a hydrogen bomb." In the meantime [1954], this number has increased to over 50. That all this was possible was due to the extensive training program of graduate students in physics at our universities in the years following the war.

The third requirement, an excellent fission bomb, is perhaps the most important of all, It is well known that a fission bomb is needed to create the high temperatures necessary to ignite an H-bomb. Since in such a process there is an obvious need to adapt the fission bomb to the particular requirements of the situation, much more detailed understanding of the fission explosion process is required and much more flexibility in the design of the fission weapon itself than was needed to develop the first fission weapon. Not until 1950 or 1951 did we begin to have the sort of capability required for this important prerequisite to a real attack on the thermonuclear problem.

The obligation of Los Alamos and the AEC after the war was in the first place to develop advanced models of the fission bomb. I have tried to show in Section 2 that this was done with competence and speed. But even if our side aim had been to develop the H-bomb, we would probably not have proceeded along a very different path than we did. As far as experimental and hardware development was concerned, the fission bomb simply had to come first. It is therefore clear that the fission bomb requirement did not permit successful development of an H-bomb substantially earlier than we actually got it, even if Teller's new concept had been available much earlier. There simply are no three lost years from 1946 to 1949.

There was a great deal of theoretical exploration during those three years, as discussed in Section 3. One might have wished that still more theoretical work had been done, but this would have required more manpower, which perhaps was the scarcest item in the early postwar years. But even supposing the manpower had been available, the work would undoubtedly have been concentrated on Method A which proved futile. As far as one can imagine such a hypothetical history, we might then have known by the Fall of 1949 that Method A would most likely not work. Even had we reached that stage at that time there is no discernible argument to indicate that Method D would consequently have been uncovered earlier than it was. Of course, it might have been, since in principle there was nothing to prevent one from conceiving of this approach. But even if it had been invented somewhat earlier, the time from invention to realization would necessarily have been considerably longer than it was, the way things actually happened. The size of the Los Alamos Laboratory, the experience of its staff, and the sophistication of their control over fission bomb design were all enormously greater in 1951 than they had been a couple of years before. In addition, there is the matter of the revolutionary change in computing facilities and techniques between 1947 and the present time [1954], which was just beginning to take real effect about the beginning of 1951.

Immediately after the war at many places in the United States work was started to design and build high-speed computing machines. This work was pursued with great vigor and enthusiasm. The first machine of the modern type which was used in connection with the weapons program was the ENIAC, and from early in 1948 persons at Los Alamos had made considerable demands on this machine. It was, however, of very limited capacity by modern

This shows that the GAC were right when they said in 1951 that the facilities of Los Alamos were quite adequate for both H-bomb and A-bomb development (SB page 121). SB reproached them for this because in 1949 they had said that H-bomb development would interfere with A-bomb program. However, the staff of the Los Alamos Theoretical Division had doubled between 1949 and 1951, much A-bomb progress had been achieved, and the new concept, as well as the advent of fast calculating machines, had made H-bomb development far easier than could be anticipated in 1949.

standards. The IBM Company's SSEC in New York began to operate sometime in 1948 and although it had a very large capacity, it was very slow by modern standards. Against this situation one must judge the statement by SB, page 61, "Lawrence received assurance from Teller that Los Alamos and Princeton would begin the machine calculations immediately." No fast computing machine existed either at Los Alamos or Princeton at the time, and the two machines existing elsewhere were not adequate for the calculations which were to be performed.

The first major improvement ***in*** this situation occurred during 1951 when the SEAC began to operate at the Bureau of Standards in Washington. Not long after this machine was running, a large fraction of its time was taken over for calculations required in the thermonuclear program, Later in 1951 large blocks of time were taken over on various models of the UNIVAC. Early in 1952 the MANIAC at Los Alamos came into operation and was immediately put to work on the thermonuclear program. This machine had been built with thermonuclear calculations specifically in mind. In the program leading up to Mike and later to Castle, the resources of the new machines were taxed to the limit, This was true in spite of the fact that these machines could accomplish in days calculations which would have required weeks to handle on the ENIAC and months to handle with the means available at Los Alamos in 1947.

5. Was the H-Bomb Necessary?

Until now I have tried to give a factual history of the development of fission and H-bombs. The vast majority of the scientists connected with this development will agree with me on this history. What I have to say now is entirely my own responsibility, and my views may not be shared by many of my colleagues.

It seems to be taken as an axiom nowadays [1954] that the H-bomb simply ***had*** to be developed. Shepley and Blair, as well as the much more balanced accounts in *Life* (September 6, 1954) and in ***Newsweek*** (August 2, 1954) and even the dispassionate opinion rendered by the Gray Board [the Personnel Security Board convened in 1954 to deliberate on the charges against Oppenheimer], seem to take it for granted that a decision in favor of a full-scale H-bomb program was the only one possible in 1949. They seem to feel that a delay of even a few months would have endangered this country. Finally, SB say on page 228 that Oppenheimer's "tragically and frightfully wrong" recommendations of 1949 were "not criminal. . . only fatal." They imply, here and throughout their article, that we would be virtually defenseless, and therefore subject to any amount of Russian diplomatic pressure, if we had not developed the H-bomb and the Russians alone had done so. I do not agree with any of these axioms.

Let us first assume the worst case, namely that the Russians are where they are now, while we have no thermonuclear weapons at all, but only our fission weapons. In assessing this possibility, I shall use again the figures given by SB, whose accuracy I can again neither confirm nor deny.

According to them (page 230) the Russian bomb was one megaton, whereas we could "any time in the year 1954 . . . put 1,000 atomic bombs of 500 kilotons' force on Soviet targets." Five hundred kilotons is half a megaton, and this 500 kiloton bomb is, of course, the one which President Eisenhower mentioned ***in*** his speech to the United Nations in December 1953. Since the Russian H-bomb is a new development, it is not likely that they have many of them at present.

Even if the situation were as unfavorable as I have just pictured, it seems to me that we would still be in quite a good position. The "wrong decision" would have been by no means fatal.

It might be objected here that I am arguing by hindsight, that in 1949 we could not know whether the Russian bomb might not come much earlier or much bigger. But so are the partisans of Teller arguing by hindsight when they say that our H-bomb development was after all successful, contrary to what might reasonably have been expected in 1950.

Moreover, I think that in fact the shortest possible time scale of the H-bomb development, in Russia as well as here, was predictable, much more so than whether ultimate success would be achieved. Since good fission bombs have to come first, the Russians, just as we, could hardly have had their H-bomb much earlier than they did.

It is often held against reassuring predictions that GeneraI Groves and Dr. Bush predicted in 1945 that the Russians would need 15 or 20 years to build an atomic bomb. But this prediction was at the time strongly opposed by the majority of scientists. For instance, in the book *One World or None,* published in 1945, Professor F. Seitz and myself reasoned that it would take a determined nation about 5 years to build an A-bomb. None of us then knew about Fuchs' betrayal, which certainly helped the Russian effort.

In spite of all this, the possibility that the Russians might obtain an H-bomb was of course the most compelling argument for proceeding with our thermonuclear program. It was, in my opinion, the *only* valid argument. It is interesting in this connection to speculate whether the Russians were indeed already engaged in a thermonuclear program by 1949. Mr. Strauss has stated in a speech that the Soviet H-bomb test, coming as early as August 1953, indicated that they had started work on the thermonuclear bomb much in advance of the United States (SB page 156). I believe that the opposite conclusion is equally justified.

We have seen that even in the worst case, i.e., if the Russians had developed their H-bomb and we had not, our present situation would not be untenable. The best case on the other hand would have been if neither country had developed such a weapon, and if thereby the mortal peril in which the whole world now finds itself had been avoided. When I started participating in the thermonuclear work in Summer 1950, I was hoping to prove that thermonuclear weapons could not be made. If this could have been proved convincingly, this would of course have applied to both the Russians and ourselves and would have given greater security to both sides than we now can ever achieve. It was possible to entertain such a hope until the Spring of 1951 when it suddenly became clear that it was no longer tenable.

The GAC'S minority plan of 1949 in which they proposed that we should try to reach an agreement between Russia and the United States so that neither side would proceed with the H-bomb development still does not seem to me utopian. This I will discuss later on.

After the worst and the best case, let us consider our actual situation at present [1954]. The balance of power is now much more in our favor than it would have been under the assumptions of the worst case. Clearly this is to be welcomed. However, it must always be kept in mind that the advantage we now enjoy through the greater power* of our H-bombs may not last. I will not venture a prediction of the time it will take for the Russians to catch up with us again.

While we have a temporary advantage in the armaments race, we now have the H-bomb with us for all time. In the words of SB, page 228, "it is inescapable that two atomic colossi are doomed for the time being 'to eye each other malevolently across a trembling world.' " We can now only rely on the sanity of the governments concerned to prevent an H-bomb holocaust.

In the course of time, the present conflict between Communism and Democracy, between East and West, is likely to pass just as the religious wars of the 16th and 17th century have passed. We can only hope that it will pass without an H-bomb war. But whichever way it goes, the H-bomb will remain with us and remain a perpetual danger to mankind. Some day, some desperate dictator like Hitler may have the bomb and use it regardless of consequences.

The U.S. atomic scientists foresaw in 1949 "The horror of this monstrous balance of potential annihilation", as SB themselves say at the end of their book (page 231). To anyone with such knowledge and with any imagination, the decision to start full-scale development of an H-bomb was a tremendous step to take, and one that must not be taken lightly. This was a decision for which the scientists, inside and outside the GAC, could not take the responsibility on themselves. It was also too big a responsibility for the AEC. One of the arguments of the GAC and of the majority of the AEC was that the decision had to be made at higher governmental levels. Furthermore, they felt it their duty to tell the President and his close advisors of the implications of this step, which they saw so clearly, while members of the government, not so familiar with the potential power of an H-bomb, could not visualize these consequences to the same extent.

I never could understand how anyone could feel any enthusiasm for going ahead. I could well understand that President Truman and his close advisors were forced to a positive decision by the potential threat of a Russian H-bomb development. But I am sure they came to this decision with a heavy heart, and that most of the scientists who went to work on this project also had heavy hearts. I certainly had the greatest misgivings when Teller first approached me in October 1949 to return to Los Alamos full-time to work on this project.

Yet there seemed to be some scientists who apparently had no scruples on this account. If we can believe SB, pages 88 and 89, or even the testimony of Alvarez in the Oppenheimer case, Lawrence, A1varez, and others associated with them had only one concern, namely how to overcome the technical obstacles. This unquestioning enthusiasm for the thermonuclear program looks to me very much like the enthusiasm that many Germans felt in 1917 when the German Government declared unrestricted submarine warfare. This gave the Germans a temporary advantage in the war but later on was the main cause which brought the U.S. into the war against Germany and thus caused the German defeat.

To most of us the important question seemed not how to build an H-bomb, but whether one should be built. The conference which was to be called at Los Alamos for November 7, 1949 (SB page 68), was to discuss this problem at length as much as the technical problem. Nearly every scientist felt the way Oppenheimer did in his letter to Conant (SB page 70): "It would be folly to oppose the exploration of this weapon. We have already known it had to be done; and it does have to be done. . . But that we become committed to it as the way to save the country and the peace appears to me full of dangers. " It is remarkable, by the way. that this letter could be quoted by anybody as evidence against Oppenheimer; it seems to me an excellent letter which is clear proof that Oppenheimer was only against a crash program, not against *exploration* of thermonuclear problems.

The GAC report concluded: "We all hope that by one means or another, the development of these weapons can be avoided. We are all reluctant to see the United States take the initiative in precipitating this development. We are all agreed that it would be wrong at the present moment to commit ourselves to all-out efforts towards its development." The report of the GAC might well be considered as a prayer for some solution to the dilemma, not as an answer. Scientists are not especially qualified to find a solution in the domain of statecraft. All they could do was to point out that here was a very major decision and it was worth every effort to avoid an irrevocable, and perhaps fatal, step. (An intermediate step which would have left time for careful consideration of the problem by the government and yet not have wasted time in the technical development, might have been to direct intensified theoretical work on the H-bomb at Los Alamos, but not to take any immediate steps toward any major "hardware" development.)

Although the GAC were seeking a solution rather than offering one, the proposal of its minority still seems worthwhile, even as seen from today's [1954] viewpoint. The proposal was to enter negotiations with Russia with the aim that both countries undertake an obligation not to develop the H-bomb. If such an agreement could have been reached and had been kept, it would have gone far to avoid the peril in which the world now stands. At that time neither we nor the Russians presumably knew whether an H-bomb could be made. In this blissful state of ignorance we might have remained for a long time to come. Since the technical program was a very difficult one, it could never be accomplished without a major effort. It is possible, perhaps likely, that the Russians would have refused to enter an agreement on this matter. If they had done so, this refusal would have been a great propaganda asset for us in the international field

According to SB page 161, the largest of our test shots reached a force of 15 megatons, compared to the Russians'1 megaton. As in the earlier cases, I cannot comment on the accuracy of the figure.s.

and would in addition have gone far to persuade the scientists of this country to cooperate in the H-bomb program with enthusiasm.

Many people will argue that the Russians might have accepted such an agreement, but then broken it. I do not believe so. Thermonuclear weapons are so complicated that nobody will be confident that he has the correct solution before he has tested such a device. But it is well known that any test of a bomb of such high yield is immediately detected. Therefore, without any inspection, each side would know immediately if the other side had broken the agreement.

It is difficult to tell whether or not the Russians would have developed the H-bomb independently of us. I am not sure what would have happened if we had followed the recommendations of the GAC majority and had merely announced that for such and such reasons, we would refrain from developing the H-bomb. Once we announced that we would go ahead, the Russians clearly had no choice but to do the same. In the field of atomic weapons, we have called the tune since the end of the war, both in quality and in quantity. Russia has to follow the tune or be a second-class power.

In summary I still believe that the development of the H-bomb is a calamity. I still believe that it was necessary to make a pause before the decision and to consider this irrevocable step most carefully. I still believe that the possibility of an agreement with Russia not to develop the bomb should have been explored. But once the decision was made to go ahead with the program, and once there was a sound technical program, I cooperated with it to the best of my ability. I did and still do this because it seems to me that once one is engaged in a race, one clearly must endeavor to win it. But one can try to forestall the race itself.

This article, written in 1954, has now been declassified. In publishing it now, I wish to add a few remarks specifically correcting some of the mistakes in Peter Goodchild's book *J. Robert Oppenheimer: Shatterer of Worlds.*

The most important point concerns the meeting of the GAC in Princeton on June 16, 1951. The Goodchild book (page 210) states that "Teller was not included among those due to speak". This is incorrect. The whole meeting was held in order to discuss Teller's new concept for the design of an H-bomb. For this reason only, a number of scientists concerned with this concept were invited, namely Bradbury, Froman, and Mark representing Los Alamos and five more independent scientists, Teller, myself, Nordheim, von Neumann, and Wheeler. The most important part of the meeting was to be the presentation of Teller's new idea. Teller himself gave the main presentation, followed by me and the three others. I totally endorsed Teller's new idea. It was after this presentation that Oppenheimer warmly supported this new approach. So did Gordon Dean, the Chairman of the AEC.

Then, the meeting discussed the implementation of Teller's idea by the Los Alamos Laboratory. In this connection, the people directly involved with the Laboratory (Bradbury, Froman, and Mark), already well acquainted with Teller's ideas, presented their plans. As I remember it, Teller got impatient with these plans, and it was only then that he "could contain" (himself) "no longer" and "insisted on being heard" (page 210). He thought that the Los Alamos people were planning too slow a development, and he insisted on accelerating it. As it turned out Los Alamos completed the development up to the Mike test in a mere 18 months.

The Goodchild book also gives the impression that Gordon Dean was unfavorable to Teller generally. This was by no means the case. Mr. Dean took me aside privately and asked how the breach between Teller and Oppenheimer could be healed. He wanted very much to have Teller's cooperation in weapons development.

Goodchild also quotes (page 214) a testimony of Teller to the FBI that 1"had been sent by Oppenheimer to Los Alamos to see whether the' H-Bomb was really feasible after all." (This refers to my visits to Los Alamos before Teller's invention, i.e., in 1950 and January 1951.) Nobody ever sent me to Los Alamos. I was a regular consultant to the Laboratory, and I was strongly urged by members of the Laboratory, particularly Bradbury and Mark, to come again after Truman's decision to develop the H-bomb. It is true that I would have much preferred the H-bomb to turn out impossible, and that I was happy at the calculation by Ulam in the early Summer of 1950 which made it appear that the H-bomb of the original design might not be feasible. But I had made up my mind myself with not the slightest influence by Oppenheimer.

The Goodchild book also repeats the statement that the Russians exploded an H-bomb in August 1953 (page 219). This was not a true H-bomb, as I know very well because I was the chairman of the committee analyzing the Russian results. This Russian test is well discussed in the book *The Advisors* by Herbert York. The first true H-bomb exploded by the Russians was in late 1955, three years after our Mike test.

The claim that the August 1953 test was a true and deliverable H-bomb was strongly maintained by Lewis Strauss to justify his contention that the United States' development of the H-bomb had been necessary and urgent. As far as I can tell, the Russians made the 1953 test essentially just to show that they could also develop such a device. But once more, it was not the real thing.

Still another claim (p. 209) is that the Russians in late 1950 tested some kind of thermonuclear device. This claim is a pure fabrication. Herbert York investigated the history of the Russian tests very carefully and concluded that there was no such test. ■