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UNITED STATES ATOMIC ENERGY COMMISSION  
VOLUME IX

In the Matter Of:

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J. ROBERT OPPENHEIMER

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## UNITED STATES ATOMIC ENERGY COMMISSION

## PERSONNEL SECURITY BOARD

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 In the Matter of :  
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 J. ROBERT OPPENHEIMER :  
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Room 2022,  
 Atomic Energy Commission,  
 Building T-3,  
 Washington, D. C.  
 Thursday, April 22, 1954.

The above entitled matter came on for hearing,  
 pursuant to recess, before the Board, at 9:30 a.m.

## PERSONNEL SECURITY BOARD:

MR. GORDON GRAY, Chairman.  
 ER. WARD T. EVANS, Member.  
 MR. THOMAS A. MORGAN, Member.

## PRESENT:

ROGER ROBB, and  
 C. A. ROLANDER, JR., Counsel for the Board.

J. ROBERT OPPENHEIMER.  
 LLOYD K. GARRISON,  
 SAMUEL J. SILVERMAN, and  
 ALLAN B. ECKER, Counsel for J. Robert Oppenheimer.  
 HERBERT S. MARKS, Co-Counsel for J. Robert Oppenheimer.

I N D E X

<u>Witness</u>	<u>Direct</u>	<u>Cross</u>	<u>Redirect</u>	<u>Recross</u>
Norris Edwin Bradbury	1575	1590	1623	1623
Walter Gordon Whitman	1634	1650	1674	1676
Hartley Rowe	1679			
Lee Alvan DuBridge	1703	1734	1763	

\* \* \* \* \*

P R O C E E D I N G S

DR. GRAY: Gentlemen, we will start. I am sure this is unnecessary, but I would like to remind the witness that he is still testifying under oath in the proceeding.

MR. GARRISON: Mr. Chairman, after a superficial examination of the record, which was not really quite completed, we reached the conclusion last night, rather late last night, that the questions we had thought had not perhaps been sufficiently covered, and that might need amplification or some further explanation had been covered at one point or another in the record, and wishing to avoid any unnecessary duplication or repetition of what has gone past, we decided not to have any formal redirect examination, but to ask Dr. Oppenheimer to sit where he is sitting this morning and to respond to all questions which you might wish to put to him upon any of the subjects of the inquiry.

Of course, he will be available for your questioning at any other time, also.

MR. GRAY: The Board accepts your decision as to procedure, of course, in this matter. Do I understand that you have no questions to ask?

MR. GARRISON: That is right, but we would welcome questions from the Board at this time or any time.

MR. GRAY: I see. Mr. Robb, do you have any questions?

MR. ROBB: I have nothing further to ask Dr. Oppenheimer.

MR. GRAY: Dr. Evans?

DR. EVANS: No.

MR. GRAY: Mr. Morgan?

MR. MORGAN: No.

MR. GRAY: I don't believe the Board has any questions at this time, Mr. Garrison. I wonder if we are ready to proceed with other witnesses?

MR. GARRISON: I think after a very short recess, we shall be able, sir. I am sorry to waste any time of the Board, but I think you will understand.

MR. GRAY: Absolutely, yes.

MR. GARRISON: Professor Whitman will be shortly here, I believe, and I think Dr. Bradbury will also be shortly here. We will see what else we can do so as not to needlessly waste time.

MR. GRAY: Let us consider ourselves in recess until your next witnesses appear.

(Brief recess.)

MR. GRAY: I think we may as well proceed at the moment, even in Mr. Morgan's absence, because I am sure he will return by the time we get to any substantive testimony.

Do you wish to testify under oath, Dr. Bradbury?

DR. BRADBURY: Yes.

MR. GRAY: What is your full name?

DR. BRADBURY: Norris Edwin Bradbury.

MR. GRAY: Would you stand and raise your right hand.

NORRIS EDWIN BRADBURY, do you swear that the testimony you are to give the Board shall be the truth, the whole truth and nothing but the truth, so help you God?

DR. BRADBURY: I do.

Whereupon,

NORRIS EDWIN BRADBURY

was called as a witness, and having been first duly sworn, was examined and testified as follows:

MR. GRAY: Would you be seated.

I shall briefly call your attention to the existence of the perjury statutes. May we assume that you are familiar that there are such statutes with penalties?

THE WITNESS: Yes.

MR. GRAY: I should like to request that in the course of your testimony if it becomes necessary for you to disclose or advert to restricted data, you let me know in advance so we may take necessary and appropriate steps.

(Mr. Morgan entered the room.)

MR. GRAY: Finally, I should say to you that we consider these proceedings as a confidential matter between the Atomic Energy Commission and its officials and Dr. Oppenheimer, his representatives and witnesses. The Commission

will initiate no releases about these proceedings. In each instance on behalf of the Board I express the hope that witnesses will take the same view.

THE WITNESS: It is understood.

DIRECT EXAMINATION

BY MR. SILVERMAN:

Q Dr. Bradbury, what is your present position?

A I am Director of the Los Alamos Scientific Laboratory, Los Alamos, New Mexico.

Q Do you also hold any academic position?

A I am professor of physics at the University of California.

Q How long have you been Director of the Los Alamos Scientific Laboratory?

A Since October 1945.

Q Dr. Bradbury, you have read the Commission's letter of December 23, 1953, which suspended Dr. Oppenheimer's clearance.

A Yes.

Q Have you read his answer, too?

A Yes, at least as I have seen it in the press.

Q I want to draw your attention to that portion of the letter or direct your attention to the matter relating to development of a thermonuclear device, the hydrogen bomb as it has been called.



First, would you tell us, or would you describe for us something of the nature of the thermonuclear research that went on at Los Alamos. I don't mean for you to tell us what was done, but whether it was a matter that proceeded by jumps, whether there were long periods when there was no thermonuclear research, or whether it was continuous, and so on.

A The possibility of using cheap fuels to make effective military explosion --

Q Excuse me. Could we have dates on this where possible so it would be clearer to the Board?

A I will try to put dates in this.

Q Yes, sir, so the Board will follow you.

MR. GRAY: Since you are interrupted, I am sorry, the security officer is always properly quite nervous.

THE WITNESS: I will be equally careful about this. In fact, I suspect I am as conscious of these things as anyone.

MR. GRAY: I am sure you are.

MR. ROLANDER: I did not mean to suggest that.

THE WITNESS: The possibility of using cheap fuels of which the so-called hydrogen bomb is an example was of interest at Los Alamos from its inception. There was active research, investigation and exploration in this field during the war years.

This interest continued after the war in a very active way; not only was basic fundamental nuclear fission done, in the relevant nuclear field, but experimental groups

having to do with techniques that might be applicable were carried on and carried on actively. There were a number of conferences held during the years immediately following the war. There was actually a system, essentially thermonuclear in nature, devised shortly after the war in 1946-47 for which techniques were then not possible or appropriate to bring to fruition.

A number of people in our theoretical division kept an active interest in this field. The basic difficulty which confronted everybody at that time was the calculation difficulty, and indeed, no calculating machines existed that would permit some of the particular problems to be explored.

This interest in the field was continuous and lasted up to the present time. There were no gaps in it. I will say that following the Russian explosion in 1949, the laboratory on its own initiative, of course, actively explored all its areas of development, areas of research, to see if there were any that should be given still further attention or more active attention in an attempt to reestablish the lead which we thought we had enjoyed in the years following the close of the war.

Certainly the thermonuclear field in general at that time offered the only outstanding promise, of reestablishing the technical lead if indeed it were a possible field to bring to fruition.

At that time there were, let us say, grave

technical concerns, not only with the actual nature of the systems which had been thought of, that is to say, whether or not they would indeed work in an effective fashion, but whether they would be useful in terms of vehicles that might be expected to employ such devices.

As is the case with any technical development, further knowledge sometimes brought increased pessimism or sometimes it brought optimism. The thermonuclear field went through cycles of this sort.

The one thing that was clear at all times was that unless there was active thought in this field, active exploration of it, that potentially useful ways to make such a device would not be found.

Is that enough to answer your question?

Q I think it does, Dr. Bradbury.

I think it does. Would you say that there was active thought and active exploration of this field continuously at Los Alamos both before and after the fall of 1949?

A Yes.

Q Was the fall of 1949 some sort of a crossroads in that?

A The fall of 1949 was really a crossroads in the atomic energy business. As I said in my earlier remarks, at that time it became clear that a step had been accomplished by Russia,, Naturally we explored our own activities to make sure that our own technical progress was devoted as well as

we could see it to maintaining the lead which we had thought we had.

Q What would you say as to the cooperation or lack of cooperation that was evidenced by specifically Dr. Oppenheimer and generally by the General Advisory Committee with respect to the thermonuclear program?

A Both the General Advisory Committee and Dr. Oppenheimer, I always found from my personal knowledge extremely helpful and cooperative -- I am seeking an appropriate word -- actively cooperative with the Los Alamos Laboratory in this field. This was, of course, not a unique thing in the thermonuclear field. The GAC and Dr. Oppenheimer had always to my knowledge been an active friend and been active friends of the laboratory, and had been helpful and had worked closely with us in all our discussions relevant to Los Alamos, or many discussions relative to Los Alamos. They invited the staff of the laboratory to meet with them. I met with them myself on many occasions.

Their comments were always helpful. Their advice was always helpful. I never knew them or Dr. Oppenheimer to take a stand or a position or to give advice which was other than useful and helpful to the laboratory.

Q By the way, in general did you and the people at Los Alamos, perhaps, if you can speak for them, agree or disagree with the position taken by the GAC in October 1949?

A I think that if we disagreed, we disagreed perhaps in flavor rather than in a substantive way. We felt extremely strongly that the thermonuclear field had to be explored, had to continue to be explored, that indeed it had grave obstacles in its way at that time, but that no decisions as to the wisdom or morality of making or stockpiling H bombs could be possibly undertaken by this country unless there was a complete knowledge of all the facts.

It was equally important that this country know what the potentialities were in this field from, let us say, a defensive point of view. In other words, we must know, we had to know, what the Russians might be able to accomplish in this field.

Accordingly, the philosophy of the laboratory was that we did not wish to enter into the debate as to whether or not this course was wise or moral or politically sound. We regarded ours as the technical responsibility to know as much as it was possible to know and as rapidly as it was possible to know it, about what was broadly called the H bomb.

This is not a very satisfactory terminology, but if it is read as relevant to the thermonuclear field, I think this will correctly describe our position.

There was, as I have said, active interest in this field and had been. It seemed to us unfortunate that the way the issue came out in the public was that here was a

crossroads, and that the country or the laboratory went this way or that way. Frankly it would have been impossible to have stopped the active consideration and exploration of this field by any fiat. You cannot step people from thinking. It was an exciting field. It apparently violated no laws of nature and inventive and ingenious scientists are bound to think about and do the work which is relevant to this activity.

We, of course, agreed with the publicly announced decision that this work should indeed go ahead and go ahead vigorously. Whether or not this was at variance with the general flavor of the GAC's thinking at that time, I would not want to say.

Q Do you recall a meeting at Princeton in the spring or summer of 1951?

A Yes, I did.

Q You were present at that meeting?

A I was present.

Q Would you care to say something about the role played by Dr. Oppenheimer there, particularly in connection with what it may indicate to the Board as to his cooperation in the thermonuclear program.

A The meeting of the General Advisory Committee in June, I believe it was, of 1951, was called following an Eniwetok operation. It was called following, let me say, the discovery at Los Alamos of some extremely promising ideas in

this field, and at that time the exploitation of these ideas seemed to us at Los Alamos and to others of our consultants and associated with us in the field warrant some attention by the Commission to certain decisions, let me say, of production, which were extremely important, and could well be quite expensive.

We as the Laboratory made this proposal. We found the General Advisory Committee and Dr. Oppenheimer extremely enthusiastic both about this idea and about the general proposals which were needed to implement this idea, particularly in so far as they required Commission action. Indeed, I think it fair to say that the General Advisory Committee and Dr. Oppenheimer were willing to go farther than the laboratory in support of this, let us say, new approach to the problem, and that their recommendations to the Commission were at least as enthusiastic as ours, and actually went somewhat beyond, in terms of support, what we had originally drafted.

I would regard this myself as very positive evidence of the interest and enthusiasm which the GAC was showing and showed in this field.

Q You have read the portion of the Commission's letter of December 23, 1953, which referred to the circulation and distribution of the General Advisory Committee report?

A Yes.

Q What was the practice at the laboratory with respect to information as to the work recommendation and reports of the

General Advisory Committee?

A If I may go back to 1946 or 1947, I guess, when the General Advisory Committee was first set up, I believe it was widely recognized that the atomic weapons field was that field in which the Commission had its greatest immediate concern at that time. They were extremely anxious to support the Los Alamos Laboratory and to make sure its work was in the most fruitful directions, and had the maximum amount of assistance from the Commission.

To this end they asked the GAC to pay particular attention to Los Alamos and they requested of me that I loan to GAC as its recording secretary Dr. John Manley, who was then my associate director for research. Manley was an outstanding physicist and had long experience with many phases, in fact almost all phases, of the atomic energy program since its inception in the early 1940's. His selection was motivated both by his qualities as an individual and by the fact that he was intimately aware of the activities of the laboratory and this intimate awareness was regarded as extremely useful to the GAC in their deliberations.

In consequence of Manley's relation both to me and to the GAC, it was customary as I have indicated earlier both for me and members of my staff to meet with GAC when problems of Los Alamos were being discussed.

It was also customary for me at least to see in draft



form these portions of the GAC minutes which were relevant to Los Alamos. I probably would have been unable to find any specific piece of paper which said this is indeed the request of either the Commission or the GAC. However, I am quite personally certain that it had the knowledge and at least the tacit consent of all concerned.

As I say, it was frequently the occasion when we met with the GAC and to see the results of our remarks or deliberations in the draft form which were not surprising.

Q Did you also see them in final form?

A Probably so, because Mr. Manley's drafts were generally as good as his final form.

Q Now, with respect to the GAC report of the meeting of October 1949, do you recall whether you saw that specifically, and if so, whether there was anything unusual about it, whether it was the normal practice, or what happened?

A I presume I did. I cannot give any precise date that I remember seeing this precise document. But I would regard it as most likely that I did see it. Certainly we had met with the GAC in discussing some of these matters either at that time or in the general vicinity of that time, and I was well aware of the general concern of the GAC in these matters. It would have been quite natural for me to have seen these and discuss them with Manley and for members of my senior staff to have seen them.

Q So far as you observed was there anything that Dr. Oppenheimer did to cause, as far as you know or ever heard, any unusual distribution of this GAC report?

A Not to my knowledge.

Q Did he play any role in the distribution of the report?

A Not to my knowledge.

Q Perhaps distribution is a word of art. I understand it means giving people copies and so on. Did the circulation, the showing, the knowledge of the GAC report, shall we say, so far as you know, cause anybody to change his opinion at the Los Alamos Laboratory about working on the thermo-nuclear program?

A Not to my knowledge. The Laboratory scientists in general, and those who contributed conspicuously to this field are strong minded individuals and generally reach their own conclusions about matters of this nature. While I think that we regretted what seemed to be in some degree -- I won't say opposition, but some degree of divergence from what might have been the flavor, let us say, of the GAC approach to it, I know of no senior person directly concerned with the weapons program at Los Alamos who left the laboratory. Indeed Dr. Manley did leave the laboratory some time in, I think, the latter part of 1950. This could be found from the record, of course -- to accept the position of chairman of the department at the University of Washington, Chairman of the Department of

Physics. Manley had not been directly connected with the weapon program, and the weapon development program in the laboratory. I think his title was associate director for research.

Another senior individual did leave the laboratory in 1951, that was Dr. Edward Teller, but in view of Tellers' connection with this whole matter, I think you may guess it was not because of any feeling he may have had about the position of the GAC.

Q He certainly didn't leave because he didn't want to work on thermonuclear.

A This, I believe, is correctly said.

Q You have seen the portion of the Commission's letter in which the statement is made, "It was further reported that you, Dr. Oppenheimer, were instrumental in persuading other outstanding scientists not to work on the hydrogen project, and the opposition of the hydrogen bomb of which you are the most experienced, most powerful and most effective member has definitely slowed down its development."

What would you say about the statement that the program was slowed down because of Dr. Oppenheimer's opinion or activities?

A It is not my opinion that the program was slowed down, as I have said. Of course, if he himself had been in a position or wished to work on it directly and personally,

this would undoubtedly have been a great help. However, it is my opinion that the program went and has gone with amazing speed, particularly in view of the predictions made regarding the difficulty of this program throughout the years 1945 to 1949. I know of no case, if you wish me to pursue these remarks, where Dr. Oppenheimer persuaded anyone not to work in this field.

As I have remarked, scientists of this caliber generally make up their own minds about wishing to work or not to work in this field. A number of outstanding people whom we would like to have brought into this program felt that their best contribution to the country was to remain in university circles and contribute to the training of graduate students.

With this point of view, one can hardly differ. Of course, Los Alamos Laboratory had a selfish approach to it.

Q Would you say that Dr. Oppenheimer's attitude, opinions, activities with respect to the development of thermonuclear weapons in any way indicated that there were some malevolent or sinister motives about it?

A Absolutely not. As I have remarked, from 1946 on, I have never known him to act in a way other than was a help to the laboratory. In one specific instance -- and doubtless others if I could recall them -- outstanding young men, this was in 1949, incidentally, <sup>an</sup> outstanding young theoretical physicist by the name of Conrad Longmire had been

offered an appointment by Dr. Oppenheimer at the Institute. This of course is evidence itself of the outstanding caliber of this individual. It turned out he was always willing to consider coming to Los Alamos, and we were extremely anxious to have him. Dr. Oppenheimer very graciously extended or postponed his appointment to the Institute indefinitely to permit him to come to Los Alamos. Indeed, Dr. Longmire never did return to the Institute, and even in the last year we have explored with Dr. Oppenheimer the possibility of Longmire taking a sabbatical at the Institute, and Dr. Oppenheimer has been willing to consider this.

He has given us frequently prospects, outstanding young individuals, whom we might be able to approach particularly in the field of theoretical physics to join the laboratory.

With me personally he has never been other, from October 1945 on and during the war years, other than encouraging, helpful, congratulatory and generally both a personal friend and a friend of the laboratory.

Q How long have you known Dr. Oppenheimer?

A I knew him as an instructor when I was a graduate student at Berkeley in 1932-31, probably, somewhere through there. I knew him as Director of Los Alamos Scientific Laboratory from June of 1944 until October of 1945. I knew him thereafter as Chairman of the General Advisory Committee and

saw him regularly, I would say, several times a year, in that capacity. He visited Los Alamos, I would again say, at least once a year or perhaps twice, in connection with his responsibilities as Chairman of the General Advisory Committee.

Q How well do you think you know him as a man, his character, and so on, the kind of person he is?

A I would think I would know him as well as one knows any individual with whom one has had friendly and professional contact over quite a long number of years, and perhaps better than the average having seen him in his capacity as Director of the Laboratory, in which I then had an assisting subordinate position.

Q Do you have an opinion as to Dr. Oppenheimer's loyalty to the country, and as to whether he would be a security risk?

A I do have such an opinion and it is a very strong one.

Q Would you state it, please?

A I would regard him from my observation as completely loyal to this country. In fact, I would make a statement of this sort, I think, that while loyalty is a very difficult thing to demonstrate in an objective fashion, if a man could demonstrate loyalty in an objective way, that Dr. Oppenheimer in his direction of Los Alamos Laboratory during the war years did demonstrate such loyalty. I myself feel that his devotion to that task, the nature of the decisions which he was called

upon to make, the manner in which he made them, were as objective a demonstration of personal loyalty to this country as I myself can imagine.

Q As to this business of a security risk, which I take it is perhaps a little different from loyalty, do you have an opinion on that?

A I do not regard him as a security risk.

MR. SILVERMAN: I have no further questions.

MR. GRAY: Mr. Robb.

#### CROSS EXAMINATION

BY MR. ROBB:

Q Doctor, Dr. Oppenheimer in his answer at page 25, "I resigned as Director of Los Alamos on October 16, 1945, after having secured the consent of Commander Bradbury and of General Groves that Bradbury should act as my successor."

Would you tell us about what happened in that connection? I assume that is true, is it not?

A This statement is true. I had been assigned to the Los Alamos Laboratory as a commander in the United States Naval Reserve in June of 1944. I had been on active duty since 1941 on leave of absence as professor of physics at Sanford University. Frankly to my great surprise and equally frankly still to my surprise, sometime in September -- I don't remember the precise date -- Dr. Oppenheimer called me in and asked if I would be willing to undertake the direction of the Los Alamos

Laboratory, that he himself intended to leave and return to academic work and asked me, as I have said, to undertake this task.

The only specific reason for this, as far as I can see, was that in the course of my duties there from 1944 to 1945, I had had contact with a number of activities in the Laboratory. My background was in physics, at least, and partly in nuclear physics. I did not agree to do this at that particular moment when he asked me. I asked time to think about it. I wanted to speak further with General Groves. I wished to consult with some of the senior members of the Laboratory, Fermi, Bethe, and others, and ask them their opinion of my competence of this task, and what they foresaw of the problem.

I was personally extremely concerned -- this is purely a personal opinion -- that the laboratory continue its task. Its task in the war years had been outstandingly accomplished, but there were a number of avenues that remained to be explored. There was certainly my personal conviction that in the exploration of these avenues still further avenues would be found that it would be necessary to go into. I regarded it as inevitable that with the disclosure to the world that such bombs would be made, that other countries would undertake this activity, and that the United States would have to be the leader in this field in so far as it could make itself sure of this.



So I had a deep personal conviction that the laboratory should continue. I ultimately agreed to undertake the task for a period of six months or until some more logical successor could be found. Apparently no more logical successor could be induced to take the task, and I also became then convinced that it was impossible for a short time man, a man on a short time basis, with the announced intention of leaving, to build a permanent and enthusiastic laboratory. Whereupon I agreed to remain on an essentially indefinite basis.

Q Doctor, you will forgive me. I am not a physicist so I don't know too much about such matters, but we have heard a number of times here reference to work on a thermo-nuclear device or work on a fission device. I wonder if you can tell us without getting into classified detail just what does a physicist, when he works on such a device? Does he just lock himself up in a dark room and think, or what does he do?

A No. I am afraid to answer your question directly would require a detailed discussion of how a laboratory works.

Q I don't want that. I am wondering what you do when you work on these things.

A No one man, I think it is fair to say, works on a fission bomb. Let me give you just a broad example here. One group of people, theoreticians, mathematicians, computers, will be exploring the behavior of a number of, let us say,

possible systems.

MR. GARRISON: Just for clarity, you asked about thermonuclear. He used the word fission just now.

MR. ROBB: I said thermonuclear or fission.

MR. GRAY: Did you intend to say fission?

MR. ROBB: Yes.

THE WITNESS: My words will be essentially applicable to both. Let us use fission and fusion indistinguishably here, because I think my remarks would be applicable to both. Working on designs for possible systems and computing, as far as the techniques of the time permit their behavior.

Another group of people, experimentalists, technicians, mechanics, shop people, will be making relevant experiments on quantities which have to go into these calculations.

Still another group of people will be working on the techniques of making the actual parts which will be required and obtaining them in the proper physical form or the proper purity, or whatever is required. All these activities follow along and periodically come to pyramids of accomplishment.

Another group of people will be doing actual, let me say, nuclear weapon engineering. That is, making out of a theoretician's schematic drawing a practical operable system. So when you speak of a person working on an atom bomb, whether it be fission or fusion, you can hardly speaking of a person doing this. It is a group of persons whose activities

have to be correlated, some at the broad base of research looking toward problems in the future; others which are involved in activities leading to a specific weapon accomplishment.

BY MR. ROBB:

Q That helps me very much, Doctor. In other words, the development of a fission device or a fusion device requires a lot more than just thinking about it.

A This is absolutely true.

Q Doctor, between 1946 and 1950, how many people at Los Alamos were working on the thermonuclear as distinguished from just thinking about it?

MR. SILVERMAN: I am not sure that the witness indicated that thinking was not a part of working.

MR. ROBB: I think we can define our terms here.

MR. GRAY: This is a very intelligent witness, and I am sure he is not easily confused.

BY MR. ROBB:

Q I am not trying to confuse you, Doctor.

A I am sure you are not.

Q I am trying to find out, because it has always been foggy to me.

A I understand the import of your question, but it will be necessary to answer it in a somewhat ambiguous fashion for this reason.

Let me take an example which will certainly be obvious, and certainly unclassified. The hydrogen bomb is widely known to potentially utilize one or more isotopes of hydrogen. The nuclear cross sections of these isotopes have to be known in the various energy spectrums with great accuracy for the computations. Accordingly, during the war and even after we had active groups, actively engaged in exploring the nuclear properties of the light elements, the elements which might possibly be effective or utilizeable in the fusion of thermonuclear field. Those people were doing physics. They were also engaged in research which was relevant to the thermonuclear weapon.

Another example which will be difficult for me again to give because of security reasons, but I will try to guard my words -- certain aspects of the so-called fission field are directly relevant, intimately related to the fusion field. If you wish to have an unclassified example of this, again it is widely known in the comic strips, that apparently some sort of primary bomb, trigger mechanism as it is called, is apparently required. How then does one distinguish developing very unique and specialized skills in primary bombs as an example?

Is this directly related to the fission field where it is immediately applicable or directly related to the thermonuclear field where it becomes applicable as soon as the

techniques become sufficiently skilled.

I cannot answer your question as to what group was engaged in thermonuclear work and what people were engaged in fission work. The fields intermingle to such an extent that while we have been asked this question for a period of years by a variety of bodies, no definite answer is possible without going into detail; this man was doing this and it had that applicability and it had that applicability.

Q Had you finished?

A Yes.

Q Was there any particular group at Los Alamos during that period from 1946 until 1950, or team that was working on the thermonuclear particularly?

A There were a number of people in our theoretical division supported by computers and computing machinery that were particularly concerned with the exploration of various phenomena that would be relevant to the behavior of thermonuclear systems.

Q Am I right in your explanation that the fission bomb is one step towards the thermonuclear; is that right?

A I am quoting commonly accepted --

Q Yes, sir. Were Dr. Richtmyer and Br. Nordheim and Dr. Teller on that team that was working definitely on the thermonuclear at Los Alamos?

A Dr. Richtmyer devoted a good portion of his time to

this matter, but also a good portion of his time to the fission field. At one time Dr. Richtmyer served as alternate division leader, so he had other interests. One of his major interests was a certain type of system which may be described properly as thermonuclear, although this should not be construed to be a specific definition of it.

Dr. Nordheim was also specifically interested in a definite type of thermonuclear system, and made very definite contributions to the potential design of such systems.

Q And Dr. Teller?

A Dr. Teller the same thing. Dr. Teller had been interested in this field very much, and probably a major portion of his time during the war was devoted to the exploration of this type of system. It was not uniquely so, and was not during his contact with Los Alamos after the war. But it was always one of his enthusiasms.

Q Was anybody else, if I may use the expression, during the period of 1946 to 1950 at Los Alamos specializing on the thermonuclear?

A How should I describe the position of people who were measuring the cross sections of deuterium?

Q I don't know, Doctor.

A I don't know either. You ask me were they specializing in thermonuclear.

Q Yes, sir.

A There were those people, if I wish to do so, that could be described as particularly interested in the thermonuclear field. I would not so describe them. They were doing fundamental research in physics, which was relevant to the thermonuclear field. Another group of experimentalists I prefer not to describe in detail who were doing work which might have been undertaken by the laboratory as general research, but was undertaken undoubtedly by the laboratory because of its probable relevance at that time to the technology of thermonuclear devices. Were they doing work in the thermonuclear field specifically, or were they not, and I cannot answer your question directly.

I am trying to make it clear that the thermonuclear field had active support both in the theoretical side, and in the relevant experimental and technological side during the war and thereafter.

Q Could you give us any idea of how long Dr. Richtmyer devoted to the thermonuclear as distinguished from his other work?

A You mean the percentage of his personal time?

Q Yes.

A I suppose roughly 50 per cent so distributed.

Q How long was he down there, sir?

A He has been there since the war up until last year. He is still on our payroll. He is currently assigned by us

to the Computing Center at New York University. He is shortly going to assume the directorship of that group, is is my information. He has become extremely interested in the techniques of computation.

Q Is Dr. Nordheim still there?

A Dr. Nordheim was and is a consultant to us. He spent roughly a year with us on leave of absence from Duke University.

Q I believe he was down there one summer.

A He has spent summers with us. He has spent one year and a good part of another on leave of absence with us.

Q Was it during the summer that he was actively interested in the thermonuclear?

A Certainly during the summers and during the year he spent with us. He was engaged in the computations, let us say, and trying to formulate a design for a specific type of thermonuclear system.

Q Did you have some computers who were working on the thermonuclear problem?

A Computers are an essential part of any thermonuclear computation. They have a very great task to play because the computations in this field are not things you make with a slide rule or a small pad of paper. As I believe I remarked earlier, one of the stumbling blocks in the years 1943 or '48 or 1949 was the absence of computing machinery, the so-called electronic brains of sufficient capacity and magnitude to handle



the type of computations which were involved. Only recently, with the development of machines such as the Maniac, the computer at Princeton, IBM computers, have we had machines which even begin to attack the problem which was confronting us during the 1944-48 era.

DR. EVANS: They are differential equations that have no integral?

THE WITNESS: They are only attackable by essentially calculation methods, by approximation methods.

BY MR. ROBE:

Q Doctor, you mentioned in 1949 Dr. Longmire had an appointment at Princeton, but came down to Los Alamos and stayed. Did that take place before or after the Russian explosion?

A His arrival at Los Alamos was in August or September of 1949. This is clearly almost coincidental with the Russian explosion. So his decision to come there I think must have preceded the actual knowledge of the Russian explosion.

Q Doctor, what was your position after the Russian explosion on the question of whether or not we should develop the thermonuclear bomb? Were you for it or against it?

A I was under the impression I had made some remarks on that subject. When you say develop the thermonuclear bomb, may I qualify my remarks to this extent. I felt, as I believe I said earlier, extremely strongly that the laboratory must

undertake all possible attacks upon the thermonuclear system to see what there was of utility in this field. Now, it seems easy now to say thermonuclear bomb has been developed by public announcement; it seems obvious that there must always have been such a device in the obvious cards. This was not the case. The state of knowledge of thermonuclear systems during the war, and thereafter, and really up until the spring of 1951, was such as to make the practical utility or even the workability in any useful sense of what was then imagined as a thermonuclear weapon extremely questionable. This does not mean that -- in fact, it meant very much to us that one must find out what is there in this field. Only by work in it will one find out. It is possible that we would have explored the field and out it was not, that we could not find a useful military system in it. But without this exploration, it is clear you wouldn't know.

We felt very strongly that we had to know the fact. In 1949-50 the state of knowledge at that time would certainly permit one to be very pessimistic about the practical utility of what was called a hydrogen bomb.

Q Did you think that the Russians would certainly try to find out?

A I was personally certain that no group of people knowing the energy which was available in these-called fusion type of reaction would fail to explore this field.

Q Therefore you thought we ought to also?

A I certainly feel this way, yes, felt and feel.

MR. ROBB: Thank you, Doctor. That is all I care to ask the Doctor.

MR. GRAY: Dr. Bradbury, you referred to regaining a lead which we had had. I believe this was your expression with respect to this kind of thing we are talking about today.

THE WITNESS: Yes.

MR. GRAY: I suppose if in that context one refers to the thermonuclear weapons, it is a question of size. Is that a fair statement?

THE WITNESS: You mean size of bang?

MR. GRAY: That is right, yes.

THE WITNESS: I am afraid it is more complicated than that.

MR. GRAY: What I am trying to get at, Doctor, did your approach to this problem involve any kind of moral consideration or was this purely technical on the ground of practicability and useability?

THE WITNESS: You are inquiring as to my personal opinions in this matter?

MR. GRAY: That is correct. During this period that we are talking about from 1946 when you became Director of the Laboratory up until the present time. I may be making an effort at distinction which can't be as clearly made as I

am trying to do it. But let us take a very simple matter. I suppose any ordinary conventional method, with respect to that, the question of making it more efficient is not a moral question at all. If you assume the weapon you have already swallowed the moral implications, I suppose.

What I am trying to get at is what you meant by regaining the lead.

THE WITNESS: I meant by this only the fact that in, I think, the general guesses that people made that the Russians in the development of both the actual fact of atomic weapons and the related production enterprises had been expected to be something of the order of five or more years behind us. The appearance of a Russian atomic explosion in September 1949 was generally regarded, I believe, as a year or two or three earlier than one might have reasonably expected the Russians to reach this accomplishment. They were clearly therefore working at a high rate of speed, even granting what I think became evident later, the treachery of Fuchs.

At the time of course, we were not aware, as I recall, that Fuchs had indeed passed information on. Perhaps this made it seem a little more plausible that they had made such rapid progress. But at any rate it was clear at that time that -- I am now only quoting my own thinking and opinion in this matter -- it seemed to me that we were in the position of two runners in the race, where it was quite clear that your opponent was running and running quite fast. It was

probably you were ahead of him in actual distance. It was not obvious that he was not running faster than you were. Our own objectives at that time had to be as far as we could make them to be sure we were running as fast as he was.

MR. GRAY: And successful work on thermonuclear weapons might have been considered one of our legs.

THE WITNESS: This I would definitely so consider. As you are aware, the thermonuclear field has two obvious military characteristics. One, apparently that in a single strike the destructive effort to deliver would be presumably very great; two, that if the materials that went into this system were indeed cheap and available, that the cost of such systems and therefore their number would not be subject to the same sort of restrictions that so-called fissions are subject of to. Both these characteristics are/obvious military interest.

There are other characteristics of thermonuclear systems or any weapons systems for that matter which have to do with essentially deliverability. In other words, a weapon is no good if it is of such a character that it can't be delivered. Hence any weapons system must be looked at in terms of its net operational worth, in terms of its cost, its effects and its relation to the vehicle system appropriate to it.

All of these questions with respect to fusion systems had to be explored. They were not known at any time in 1949, certainly, and it was possible, I will not guarantee, that

effort in this field would lead to something which would have military utility. However, I would like to emphasize that this was at that time a technological question. It was not guaranteed by Los Alamos or anyone else that indeed there would be a feasible or effective useful thermonuclear system.

MR. GRAY: But on this matter of lead, thermonuclear weapons certainly were a part of that picture.

THE WITNESS: Very definitely so. There were also leads that had to be established in the fission field or were being established in the fission field. This was another part of the military strength of the country.

MR. GRAY: As a matter of hindsight, suppose there had been a Presidential directive in 1945 or at some later date, perhaps, but earlier than January 1950; is it possible that we might have had the invention or discoveries earlier?

THE WITNESS: My personal opinion in answer to that question is in the negative. I would like to say as much as I can within the bounds of security as to why.

Could I consult just a moment on the question, Mr. Rolander?

(Consultation)

MR. GARRISON: Mr. Chairman, I will leave it to the Board if the Board would like to, after hearing what Dr. Bradbury has to say, explore it in classified terms. We would withdraw.

MR. GRAY: Thank you. I hope that won't be necessary.

THE WITNESS: I believe I can make my remarks in a fashion which will be acceptable. The only line of attack which had occurred to us on this problem throughout the years 1942 onwards seemed to be a line of attack during 1945-1949 which would be fraught with enormous technical difficulties, that is, practical technological difficulties.

There was also a grave question as to whether or not the systems then thought of would have any behavior that would be at all, let us say, effective in terms of their probable complexity, probable size and probable cost. Had we endeavored to explore those fields in that state of knowledge, we would have had in my opinion two extremely undesirable courses, one of which would have been, I believe, almost fatal. We would have spent time lashing about in a field in which we were not equipped to do adequate computational work. We would have spent time exploring with inadequate methods a system which was far from certain to be successful, and we would not have made the relevant progress which would have been required in the fission field.

I am getting here on thin ice, but if you will let me stick by my earlier remarks that skill and ingenuity in the fission field is an essential prerequisite to the success in the thermonuclear field, the progress of the

laboratory during the years following the war in the understanding and development, and indeed, some systems of very close relevance to the thermonuclear system as we know them today, were an essential part of the ultimate actual ability to make an effective thermonuclear weapon.

Hindsight is a difficult thing. Perhaps the statement I am making is self-serving. But my own personal opinion is that the course of action pursued by the laboratory is right. I regret to make this statement in this fashion, perhaps because it was partly I presume my decision. But in retrospect I cannot see how we could have reached our present objectives in a more rapid fashion by any other mechanism except the mechanism by which we went.

MR. GRAY: You think there has not been delay in any event. You reject the notion that there has been delay in the development of this weapon?

THE WITNESS: I reject this notion. I also think that it is perhaps correct to say that at any time, particularly in 1945, '46 and '47, there were certain fundamental objectives at the laboratory that simply had to be met. If we had, let us say, retained our 1945 technology in weapons through the next three or four years, with or without thermonuclear systems, this country would have been enormously deficient in strength compared to what it was actually at that time because of the efforts of the laboratory in the fission



field. These efforts. . . also made possible subsequent developments in the thermonuclear field.

MR. GRAY: In your conversations with Dr. Oppenheimer in 1945 with respect to the possibility of your becoming Director, did you discuss what policy of the laboratory might be with respect to this matter we have been talking about, do you recall?

THE WITNESS: With respect to the development of thermonuclear systems?

MR. GRAY: Yes.

THE WITNESS: No, we did not discuss this. Let me say I have no recollection of discussing this. I would like to make one additional comment in that connection. Shortly after I assumed the directorship of the laboratory, I had a meeting of all the staff members then present and one to which I was essentially talking -- let me say the senior staff members, the coordinating council of the laboratory, at that time I discussed my own philosophy of the laboratory and included in that philosophy was the continuation of the exploration which we had been doing in the thermonuclear field.

MR. GRAY: Do you recall any change of attitude on Dr. Oppenheimer's part towards the development of thermonuclear systems at any time during your association with him?

THE WITNESS: I mentioned earlier the developments, the ideas in this field which occurred during the spring of

1951, prior to the meeting at Princeton in June of 1951. I think I would be correct in saying that these ideas seemed technically sound to Dr. Oppenheimer and that he upon hearing of them, regarded the prospect of success in the field as extraordinarily more likely. I think his opinions expressed at the meeting in Princeton reflected this opinion, if you wish, that here was a technique or an idea which cast a new light on the practicality of such systems.

MR. GRAY: . But you don't recall anything at the time, for example, of the use of the atomic weapon in the late months of the war that reflected any changed attitude towards thermonuclear weapons?

THE WITNESS: I don't believe I ever discussed the use of any atomic weapon in war with Dr. Oppenheimer. Certainly not at that time. It would not have been my position in the laboratory to do so. We probably had discussed the GAC meetings later on of how such weapons might be employed, what vehicles might be used for them, the problems of vehicles, questions of that sort.

MR. GRAY: Dr. Bradbury, you mentioned Dr. Teller's departure from the laboratory. I am not familiar with the circumstances of that. Could you very briefly indicate what the circumstances were?

THE WITNESS: If I could do so in what I might regard as administrative confidence. This is not restricted data,

but on the other hand, it has to do with personal relationships between Teller and myself.

MR. GRAY: I don't know how important it is to have that.

THE WITNESS: Perhaps I can answer this without any serious difficulty, but again I would like to say that this is essentially -- could I make it off the record, if you wish?

DR. EVANS: I think Dr. Bradbury doesn't have to answer.

MR. GRAY: Let us go off the record.

THE WITNESS: I don't care whether it is in the record or not. All I would like to say is that Teller and I disagreed as to the most effective method of the administration of the thermonuclear program at Los Alamos for its most rapid accomplishment, and ultimately we disagreed on essentially a matter of trivalency, that is to say, the projection in point of time in advance, a date for a definitive test operation. I think for some time prior to that, Dr. Teller and I had had some differences of personal opinion not regarding the importance of the program or the general way in which it should be going, but we had differences of opinion regarding the best way to administer it. These were differences of a rather fundamental nature in the administration of a laboratory, and since the administration of the laboratory was essentially my responsibility, I had to do it in a way that seemed best to me.

Ultimately Teller left. Our relations are personally friendly. He was a consultant to the laboratory thereafter. He still spends occasional time with us, although his primary interests are now with another group.

MR. GRAY: At the time of the close of the war, there were varying views as to what should be done with the laboratory, I believe. There were some who wished to close it up, some who wished to continue full speed, some who favored its removal to some other place. Is that a correct statement of the varying views among the staff?

THE WITNESS: I am afraid that I probably would not have a complete cross section of all the views. My own opinion was obviously strong, and my own, that the laboratory should not be closed up. It is unlikely that very many people came to argue with me that it should be closed up.

MR. GRAY: Did you ever hear Dr. Oppenheimer express the view that it should be closed?

THE WITNESS: I never did. In fact, I would probably be the last person to have heard him make such a statement inasmuch as he was instrumental in me taking it over. It would be unlikely that he would say at the same time to close it up. I was aware, and this was the proper question at the time, was Los Alamos, New Mexico, the best place to operate this laboratory. This question was actively explored by the Manhattan District in the year following the war, and the

ultimate decision was that it was probably the best place to operate it.

MR. GRAY: Dr. Bradbury, I don't want you or anyone else to misunderstand the next question I am going to ask. It points to no conclusion certainly in my mind about anything at all. It has to do with perhaps the most serious underlying implication involved in these proceedings. That has to do with loyalty to country.

I think your statement in response to a question from counsel was that you had no question about Dr. Oppenheimer's loyalty, and you based it at least in part on his very remarkable accomplishments during the war years as Director of the Laboratory. I think there were those perhaps who questioned Dr. Oppenheimer's loyalty and who might argue that an individual who was sympathetic to the USSR could very consistently have gone far beyond the call of normal duty in his war work, which was beneficial to the interests of the United States, and still have felt that sympathetic interests for the Soviet Union were also being served. That is at least an argument can be made, and I am sure you are familiar with it.

THE WITNESS: Yes.

MR. GRAY: In your testimony about Dr. Oppenheimer's loyalty, are you prepared to give your judgment to the war years? In other words, do you think that his actions since the war are of the same character and nature as to lead you to a

conclusion about his loyalty?

THE WITNESS: I do, and I have the same opinion. I think it can be supported by the same sort -- perhaps not quite the same sort of objective evidence. I am well aware that it is possible to attribute ulterior motives to almost any human action. It is possible to argue these questions in perpetuity along those lines. Referring to my statement about his behavior as Director of Los Alamos Laboratory, in my own opinion, this to me constitutes as strong objective evidence as one can hope for, of loyalty. I have to base this not only upon the technical accomplishments of the laboratory, but upon the way in which these accomplishments were done, upon the manner in which he sought and made use of advice from his senior staff, essentially upon a sort of subjective impression which you can only get by seeing a man look worried, that indeed the success of this laboratory and its role in the war that was then going on were objectives which were uppermost and surpassed all others in his mind. I was not looking in his mind, and I cannot say this of course from definite knowledge. You can never say anything about a man's loyalty by looking at him except what you feel. I would feel from everything that I could see of his operation at Los Alamos during the war years that here is a man who is completely and unequivocally loyal to the best interests of this country.

I would make the same remark about the associations

I had with him after the war years. I suppose it is true, although he can say this better than I, that he had deep personal concerns about the actual role of atomic weapons in the national security. I think anyone is entitled and should have this same sort of concern. What personal decisions one makes in the long run is of course a personal matter. But certainly his chairmanship of the GAC after the war years never questioned the fact or never questioned the assertion that the Los Alamos Laboratory should continue, should be strengthened, should proceed along lines of endeavor which were of military effectiveness. Every decision that I can recall that the GAC made with respect to the laboratory, with the possible exception of what may have been their opinion regarding thermonuclear development, seemed to me to be the right decision. In other words, there was never to my knowledge any degree of difference of opinion between myself, my senior staff, and the positions taken by the GAC.

This was particularly the case that the laboratory felt extremely strongly that actual test of nuclear weapons were a fundamental part of the progress in this field. We still feel that way extremely strongly. The GAC supported us in this. Had they not done so, our progress would have been enormously slower or almost zero. This could have been a point where one might have taken a contrary position perhaps. The GAC did not do so.

I believe the question which I tend to believe was exaggerated at the time in the public press and got into erroneous importance at the time through the efforts of a number of people -- it assumed an erroneous stature in public debate -- was on a case where we might have found ourselves in a difference of opinion with the GAC. Whether this difference was real or not, I am not prepared to say. But I have stated what the opinion of the laboratory was as strongly as I can.

I do not personally believe that if there was this difference of opinion, and I presume there was some difference of opinion here, that it was based on malevolent motives.

I believed and still believe that the apparent position of the GAC was based upon a defensible argument although one with which I might not personally agree. I might not have personally agreed with one of the conclusions of the question of policy that some members of the GAC arrived at. Nevertheless, I do not regard them as opinions which are either malevolent or subversive. I positively regard them as opinions which can be held and which were held as matters relating to the safety of the United States.

The safety of the United States I am convinced was uppermost in the minds of all members, including the Chairman, of the GAC. We may have differed as to the best methods of obtaining the safety. I think such differences are an essential



part of any democratic system. I never had then nor do I now have the slightest feeling that these differences were motivated by any other than a direct deep and sincere concern for the welfare of the country.

That was only substantiated by the actions of the GAC after the President's decision, which again were in strong support of this whole field which we characterize as thermo-nuclear. Basically the GAC supported the laboratory as a weapons laboratory in all fields. If there was a difference of opinion in 1949-50, it had to do with perhaps the technical question of emphasis on one or another line of attack in the weapons field in general.

Does that answer your question?

MR. GRAY: I think probably it does. I think your answer is in the affirmative. I think my question was that you feel that the character and nature and intensity of Dr. Oppenheimer's loyalty has been as great in postwar years as you saw it in the war years.

THE WITNESS: That is my feeling.

MR. GRAY: Are there any questions?

DR. EVANS: Yes. Dr. Bradbury, where did you have your undergraduate and graduate education?

THE WITNESS: I received the bachelor of arts degree from Pomona College in Claremont, California, in 1929. I received the Ph. D. from the University of California in 1932.

There for two years I was research fellow at MIT. Thereafter I was on the academic staff at Stanford University, first as assistant professor, associate, and then full professor.

DR. EVANS: Are you a Communist?

THE WITNESS: No, sir.

DR. EVANS: Have you ever been?

THE WITNESS: No, sir.

DR. EVANS: Have you ever been a fellow traveler?

THE WITNESS: No, sir.

DR. EVANS: There were a lot of organizations that the Attorney General listed as under Communistic control, Doctor; do you know that list?

THE WITNESS: I have seen that list.

DR. EVANS: Are you a member of any of those organizations?

THE WITNESS: I am not. I think it would be an awful time to find out if I were.

DR. EVANS: Were you surprised when the Russians fired a bomb?

THE WITNESS: In 1949?

DR. EVANS: Yes.

THE WITNESS: Yes, sir.

DR. EVANS: You were surprised?

THE WITNESS: I was surprised.

DR. EVANS: Do you think the knowledge that Fuchs

might have given them helped them in that?

THE WITNESS: I now think so. I was surprised at the time that it came so early. It is now my personal impression although I have no evidence to support this, of course, that probably they were assisted along these lines by the information Fuchs appears to have given them.

DR. EVANS: You do think that scientific men should be required to keep their discoveries secret when they might affect the country and not publish them?

THE WITNESS: That is a very difficult question to answer, sir. It is very difficult for a scientist doing basic research to be sure that in the course of time this particular technical report, paper, invention or discovery may affect the security.

May I give you an example of this? It would have been a perfectly normal thing for a scientist to do, although somewhat difficult, to measure certain neutron cross sections of deuterium in 1932, '34, '36 and '38, and so on. It would have been a nice task and perfectly good nuclear science at that time. At the present time, such cross section measurements are, of course, carefully guarded secrets because they are relevant to a thermonuclear problem. How in 1934 or 1936 would one have known that these cross sections are going to be something that would affect national security? I can't give you an answer to your question. I think if an individual knows

or believes that his discovery is immediately relevant to national security, he has definite responsibility to the country in that connection.

DR. EVANS: Do you think that scientific men as a rule are rather peculiar individuals?

THE WITNESS: When did I stop beating my wife?

MR. GRAY: Especially chemistry professors?

DR. EVANS: No, physics professors.

THE WITNESS: Scientists are human beings. I think as a class, because their basic task is concerned with the exploration of the facts of nature, understanding, this is a quality of mind philosophy -- a scientist wants to know. He wants to know correctly and truthfully and precisely. By this token it seems to me he is more likely than not to be interested in a number of fields, but to be interest in them from the point of view of exploration. What is in them? What do they have to offer. What is their truth. I think this degree of flexibility of approach, of interest, of curiosity about facts, about systems, about life, is an essential ingredient to a man who is going to be a successful research scientist. If he does not have this underlying curiosity, willingness to look into things, wish and desire to look into things, I do not think he will be either a good or not certainly a great scientist.

Therefore, I think you are likely to find among

people who have imaginative minds in the scientific field, individuals who are also willing, eager to look at a number of other fields with the same type of interest, willingness to examine, to be convinced and without a priori convictions as to rightness or wrongness, that this constant or this or that curve or this or that function is fatal.

I think the same sort of willingness to explore other areas of human activity is probably characteristic. If this makes them peculiar, I think it is probably a desirable peculiarity.

DR. EVANS: You didn't do that, did you?

THE WITNESS: Well --

DR. EVANS: You didn't investigate these subversive organizations, did you?

THE WITNESS: No. Perhaps my interest lay along other lines. I don't think one has to investigate all these political systems.

DR. EVANS: Do you go fishing and things like that?

THE WITNESS: Yes, I have done a number of things. Some people, and perhaps myself among them, I was an experimental physicist during those days, and I was very much preoccupied by the results of my own investigations.

DR. EVANS: But that didn't make you peculiar, did it?

THE WITNESS: This I would have to leave to others to say.

DR. EVANS: Younger people sometimes make mistakes, don't they?

THE WITNESS: I think this is part of people's growing up.

DR. EVANS: We all do.

THE WITNESS: That is, take actions which turn out to be wrong later on. Whether they were mistakes at the time may be a debatable question.

DR. EVANS: Do you think Dr. Oppenheimer made any mistakes?

THE WITNESS: My personal feeling here with regard to the situation specifically to the question of organizations is that these are actions which in the light of history, in the light of subsequent developments, turn out to have been undesirable. I would not like to say that I regard them as either right or wrong. I say that simply they turn out to have been bad for him to have done at this time. At the time they were done, I regard them as potentially at least without significance. They reflected a certain area of interest, an interest which as you recall was held by a number of people at that time. The Spanish war was of concern to a number of people.

DR. EVANS: That is potentially they should have been of no interest to this Board?

THE WITNESS: No, I cannot say that. I don't wish to

make a speech. It is unfortunate that the number of objective examples which one has of, let us say, people who are disloyal is extremely small. You can count them on the fingers of one hand. In every case these people seemed to have been drawn from a certain type of background in which at least some degree of interest in liberal, left wing or Communist activities was a part. Therefore, I have to agree that where this background of interest in these affairs occurs, that a query at least is indicated.

It is a fact of life, but I think it perhaps regrettable that because a few people out of thousands have been discovered in this particular area, that thousands or tens of thousands are automatically thereby put potentially in the same category. I think the question has to be raised because of the things which Fuchs, Alan Nunn May, Greenglass have done. Perhaps it is one of the most serious things they have done, to cast a shadow of suspicion on those who were interested in these activities for completely humanitarian or intellectual motives.

I think therefore this question has to be raised. I myself do not regard the matter of membership in such societies or interest in them as particularly significant in the light of the times -- let me say necessarily significant in the light of the times. I think it is a question which must be raised, must be explored. It may turn out to have meaning.

It might be in this case it does not have meaning.

DR. EVANS: You spoke of loyalty. Would you put loyalty to your country above loyalty to your friends?

THE WITNESS: I would.

DR. EVANS: That is all I have.

REDIRECT EXAMINATION

BY MR. SILVERMAN:

Q Dr. Bradbury, from your knowledge of Dr. Oppenheimer, today, do you think he would put loyalty to his country above loyalty to a friend?

A I believe he would.

MR. SILVERMAN: That is all.

RECROSS EXAMINATION

BY MR. ROBB:

Q Doctor, I have one question suggested by your discussion with the Chairman about what might be the result had there been a Presidential directive in 1945 or 1946 to undertake all out work on the H bomb.

It has been testified here, Doctor, that something happened in the spring of 1951, and that accelerated the successful development of the thermonuclear so that work came to a successful conclusion maybe 18 months thereafter.

My question is, supposing that something had happened in 1945 or 1946, what would have been the result? How soon do you think you would have had the thermonuclear weapon



perfected?

A We had this idea --

Q Is that an intelligent question?

A This is a question that I would answer this way. Had this idea occurred in 1945, '46, '47 or '48 or almost any time before it did occur, we would not have known how to use it in an effective military fashion. We were already pursuing in the years following the war those techniques, specifically in the fission field, which made the implementation of this idea a practical thing. We had already conducted experiments. I can't describe them for security reasons. They were in the fission field, and bore directly upon this field. Frankly, if I may go back to one of your other potential questions, had there been a Presidential directive to proceed along thermonuclear lines in 1945, I would almost doubt in retrospect that we would have done or could have done anything much different than we did. In other words, the active exploration of the fission field was a necessary and essential prerequisite known all along to the fusion field. Had there been such a hypothetical decision, it is impossible to answer. Had there been, we would have done exactly as we did. We might have been persuaded otherwise, and I think if we had we would have found ourselves farther behind in 1954 than we are.

Q I am not sure your answer -- and that is my fault and not yours.

A It is my fault.

Q Your answer about not knowing how to use this discovery in 1946 or 1947, could you explain that a bit further?

A I would have great difficulty in doing so without going into restricted data. Let me think for a moment to see if I can find some way around this.

There would be two possibilities. We would not have been able to make the relevant calculations for mechanical reasons. We would not have been able to make them for let us say technological reasons, because only in the course of those years did we begin to get some understanding of how to compute atomic or fission bombs. Thirdly, we would not have been able to make use of it practically because we would not have had the comparable skills, let us say, to make fission bombs whose characteristics would be appropriate to this sort of a system. Of course, by that I am implying that there are certain relationships between these things, and that will have to be a part of this argument.

Q Doctor, in the years between 1946 and 1950, did you have the staff and the equipment then to do what you did subsequent to this discovery in 1951?

A Between when did you say, 1945 and 1950?

Q Yes, sir. In other words, assuming this discovery in 1945, '46 or '47, did you then have the staff to do what you did with the discovery in 1951 and 1952?

A As you are doubtless aware, in 1945 the laboratory of course was partly civilian and partly military. We had a couple of thousand SED, special engineering detachment of the military personnel. We had a number of officers. In 1945 and early 1946, a great part of our civilian personnel left to return to school, to their industrial and academic jobs. The size of the laboratory reached its minimum roughly in September of 1946, at which time its size was roughly half, perhaps a little less than half of its size at the present time. From that time on it has grown steadily up to about the present time.

There were admittedly difficulties in taking the laboratory through the transition period prior to the Atomic Energy Act, while personnel straightened themselves out in their own desires. In 1946, throughout the entire year, or at least until the adoption of the Atomic Energy Act, perhaps we were lucky to keep ourselves alive. We had the Crossroads operation to carry out, and life was far from easy. I don't say it has ever been easy, but in those days certainly our task was not simple. We were devoting, as I have said earlier, our major directed effort, the efforts which come to the peaks of these pyramids of development, two things which would make the production capacity of the United States as effective in a military way as it possibly could be right then and there. We were also devoting our efforts to making atomic weapons as they then existed more effective as part of a weapons

system for the country; in other words, an effort to maximize the immediate potential of the country.

As I have said earlier this was not to the exclusion of thermonuclear work but it was the focus of achievement which was in the fission field. We would have had a hard time and unprofitable time and I think in the light of subsequent events, and it would have been an error and mistake to try to hash about in a field for which none of the basic technologies then existed, and at a time when there were very clear things to be done in the fission field.

Q Beginning with the Presidential directive in January 1950, did you thereafter receive additional personnel and additional funds and additional assistance in your work?

A The laboratory has never lacked for funds. The actual request for funds has always been supported by the Commission and the Congress. The growth of the laboratory has been as rapid as we could make it subject to housing and the ability to draw personnel into our isolated area, and into the classified field. There was no immediate change in either dollars or personnel before or after the President's recommendation. It was a matter of growth. We did at that time carry out an active campaign to enlist the services of a number of the senior scientists of the country who had been with the project during the war, to see if they could come back on a year's leave of absence, and we were successful in a

number of these cases, and in a number we were not because they felt their task was more urgent in the instruction of graduate students.

Q Whether it was immediate or not, as a result of the presidential directive, was there an expansion in your facilities and personnel and funds?

A As a result of the Presidential directive, I can't say there was. I would say there has been an expansion and an increase of our funds continuously in the years from 1945 on onward. I would have to look at a graph of the actual dollars per year spent. I don't have it with me. I would doubt if such a graph of dollars spent would show any significant fluctuation in the period we were talking about, except as a result of a test activity occurring in this year or not in this year. By this I do not mean that we lack support. We have always received from the Commission and the Commission from the Congress as much support as we could see our way clear to use in a justifiable fashion.

MR. ROBB: Thank you, sir.

MR. GRAY: I am sorry, Dr. Bradbury, that I am not through with my questions.

When did you go to Los Alamos?

THE WITNESS: I arrived July 4 or just about July 4 of 1944. I first visited there some time in June 1944 when I was about to be transferred there. Prior to that I was at

the United States Proving Ground at Dahlgren, Virginia.

MR. GRAY: I have forgotten at what time some of these people whom we discussed in earlier proceedings, such as Lomanitz, left. I guess he left before you arrived?

MR. ROBB: He was not at Los Alamos.

MR. BECKERLEY: He was at Berkeley. Did you know that man?

THE WITNESS: No.

MR. GRAY: Did you know David Hawkins?

THE WITNESS: Yes.

DR. EVANS: Weinberg?

THE WITNESS: Weinberg, no.

MR. GRAY: What were some of the other names?

MR. SILVERMAN: I don't believe Weinberg was at Los Alamos.

DR. EVANS: No, he wasn't.

MR. GRAY: You knew Hawkins?

THE WITNESS: I knew David Hawkins, yes.

MR. GRAY: Did you know anything about his sympathies?

THE WITNESS: At that time, no. I was unaware of his background until it was about to appear in the public notice.

DR. EVANS: That is, it is perfectly possible to be about a man quite a long time and not know anything about his background?

THE WITNESS: It is perfectly possible. I knew David Hawkins in a friendly fashion. I presume I have had cocktails with him. I presume I have been to dinner with him. I never discussed politics with him and found him a very loyal supporter of our activities there.

MR. GRAY: Were you surprised when you read or heard that he had been a member of the Communist Party?

THE WITNESS: I would say I was surprised, yes. I don't wish to have this interpreted that I was shocked. I have no idea of this. I had no reason to have any idea.

MR. GRAY: Did you know Philip Morrison?

THE WITNESS: Yes.

MR. GRAY: Do you know anything about his sympathies?

THE WITNESS: I would say my personal contact with him was the same as with David Hawkins. I had more technical contact with him because he was very active in the design of one of our research tools, the so-called fast reactor. We valued his professional advice extremely highly. I never recall discussing with him political problems. I was, I think, indirectly aware that he was not entirely sympathetic to the development of the atomic bomb. But I don't think he was unique in this feeling among people who were about to leave Los Alamos.

MR. GRAY: This would indicate that you could know an individual and see him frequently, as Dr. Evans said, in

complete ignorance of membership in the Communist Party?

THE WITNESS: I am sure this is certainly true. I knew Fuchs well.

DR. EVANS: You did know him?

THE WITNESS: I wouldn't say well. I am sure Fuchs has been a guest at my house, and has had cocktails at my house or perhaps even eaten dinner at my house.

MR. GARRISON: In Los Alamos?

THE WITNESS: Yes. I must say in that case I was deeply shocked by what appeared to have been Fuch's activities at the time. This was a great shock to all of us at Los Alamos.

DR. EVANS: It was a great shock to everybody.

MR. GRAY: There seems to be no question that he had a commitment to a foreign power, does there?

THE WITNESS: I perhaps might have a slightly different interpretation of it. I think it must be said in fairness to Fuchs that he worked extremely hard and effectively for Los Alamos and this country. He appears to have a divided or double loyalty. I think his accomplishments at Los Alamos it must be said were very effective.

MR. GRAY: This was the point I was trying to make in the question I asked you earlier, and when I asked you not to misunderstand the import of the question, that here is an example, Fuchs, himself, who at the same time could want Los Alamos to be a marvelously successful laboratory, and still



have loyalty to another country.

THE WITNESS: I never saw in Fuchs anything other than to indicate a hardworking, effective, skilled physicist. I think it is agreed that his accomplishment at Los Alamos did assist the laboratory in the attainment of his objectives.

DR. EVANS: He was a Dr. Jeckyl and Mr. Hyde.

THE WITNESS: I have to admit a complete failure to understand Mr. Fuchs.

MR. GRAY: Thank you very much, Dr. Bradbury.

(Witness excused.)

MR. GRAY: We will take a little recess.

(Brief recess.)

(The following portion of the transcript, pages numbered 1633 to 1677, inclusive, is classified and appears in a separate volume.)

## AFTERNOON SESSION

2:30 P.M.

MR. GRAY: Mr. Rowe, do you wish to testify under oath? You are not required to do so. I should tell you that all the witnesses to this point have.

MR. ROWE: I would prefer to.

MR. GRAY: Would you be good enough to stand and raise your right hand? What is your full name?

MR. ROWE: Hartley Rowe.

MR. GRAY: Hartley Rowe, do you swear that the testimony you are to give the Board shall be the truth, the whole truth and nothing but the truth, so help you God?

MR. ROWE: I do.

Whereupon,

## HARTLEY ROWE

was called as a witness, and having been first duly sworn, was examined and testified as follows:

MR. GRAY: Would you be seated, please, sir.

It is my duty to remind you of the existence of the perjury statutes. I trust we need not discuss those here. You are familiar with them?

THE WITNESS: I have read them several times, yes, sir.

MR. GRAY: In the event, sir, that in the course of your testimony it becomes necessary to disclose restricted data, I should like to ask that you notify me in advance, so that we might take appropriate steps.

Finally, I point out to you that we consider these proceedings a confidential matter between the Atomic Energy Commission, its officials on the one hand, and Dr. Oppenheimer, his representatives and witnesses on the other hand. The Commission will take no initiative in releasing material to the press about these proceedings, and on behalf of the Board, I express the hope to each witness that he will take the same view.

Mr. Marks.

DIRECT EXAMINATION

BY MR. MARKS:

Q Mr. Rowe, will you please identify yourself for the record?

A In just what manner?

Q Your present position.

A I am vice president and director of the United Fruit Company.

Q What is your profession?

A I am an engineer.

Q Will you describe very briefly your professional career in just a few sentences?

A I started after graduation from college as an engineer with the Isthmian Canal Commission, which was later termed the Panama Canal Commission, and served there 15 years.

I came back to the United States at the end of that time and entered in consulting service with a firm by the name

of Lockwood, Green and Company, first in Detroit and then in Boston. I was with them about seven years, and then went to the United Fruit Company as their chief engineer, and I have been with them ever since.

Q When did you become a vice president of United Fruit?

A 1928.

Q Will you also describe briefly your original connection with war work, that is, World War II, and what it consisted of?

A In 1940 I was connected with the National Defense Research Committee, headed up by Dr. Vannevar Bush, Dr. Karl Compton and Dr. Conant. That was later made into the Office of Research and Development.

I was chief of Division 12, which handled mobile equipment and naval architecture from 1940 until the conclusion of the war, and the conclusion of our reports in 1946. I was also a consultant to the Secretary of War. I was a consultant on the Rubber Division of the War Production Board and several other short time jobs that I don't recall at the moment.

Q What developments did you have a share in while you were with the NDRC and its successor?

A The one that gained the most notoriety was the Duck, from that to the Weasel, which was a very light snow vehicle traveling over snow and over marshy ground.

Q In your capacity as a consultant for the Secretary

of War, did you have any overseas assignments?

A Yes. In May 1944 I was assigned to General Eisenhower's staff as a technical advisor primarily for the purpose of bringing to the attention of the field commanders and the troops there the military things that had been developed by OSRD up to that time. I served with him for about seven months.

Q Served with him?

A With SHAEF for about seven months.

Q What were the conditions under which you took that assignment?

A There were two. One ordinary condition is that I requested I be introduced to General Eisenhower and his staff by a general officer, and second, that I thought I could be most effective operating out of channels and directly by a pipeline to Washington.

Q Why were you interested in that latter?

A Principally because I don't know how to operate through military channels. Secondly, that I felt I could be more effective and save a great deal of time -- time was of the essence -- and be much more effective to the field commanders.

Q What were some of the things with respect to which you had any influence in that assignment?

A Radar and radar controlled guns, the proximity fuse, and its introduction to combat the buzz bomb, the infrared

instruments that were used by the paratroopers to collect together after a drop.

Q You have any difficulties persuading them to adopt these measures?

A None whatever.

Q After your assignment with SHAEF, what was your next connection with war work?

A As soon as I returned to the United States from that work, I was notified that they wanted me to go to the Pacific and do the same kind of work for General MacArthur. It had all been arranged with his consent under the same conditions. Before I could get away, Dr. Conant and Major General Leslie Groves came to me and said they had a job they wanted done and I told them I was afraid I couldn't do it, because I had already signed up, and they said this takes priority over everything you have been assigned to, so you better do what we want you to do.

The only question I asked was whether or not the assignment would be in the continental United States or whether it would still be abroad.

Q What was that assignment?

A I was assigned as a consultant to General Groves and Dr. Oppenheimer in the procurement of materials in the development of the A bomb, trying to be of what assistance I could to bring it to a conclusion on a predetermined date.

Q Where did you do that work?

A In Los Alamos.

Q How much time did you spend on it?

A I spent a greater portion of my time commuting between Los Alamos and my office in Boston. I usually spent the weekends in Boston and spent from Monday to Friday in Los Alamos, or in some other city in connection with the work.

Q During that period how well did you come to know Dr. Oppenheimer?

A I was reporting more to him than I was to anyone else. I became very well acquainted with him.

Q I take it during all of this period you continued your connection with the United Fruit Company?

A Yes, sir. The only time I had a leave of absence was when I was in Europe.

Q After the war what connections did you have in any role with the government?

A I was made a member of the first General Advisory Committee in 1946, I believe, and served for the four year term to which I was appointed, from 1946 to 1950. I think the initial date was August or September and it ended in August or September.

Q That is the initial date of the term was August 1946?

A Yes.

Q But you actually began your service early in 1947?

A No. As soon as I was appointed, I think we met within the next month. I am quoting entirely from memory, because I kept no papers of any kind covering any of this confidential or secret work that I did.

Q In connection with your work on the General Advisory Committee in those first four years of its existence, did you again work closely with Dr. Oppenheimer?

A After the conclusion?

Q No, in that four year period.

A We met once a month for two or three days and two or three nights.

Q Do you recall the meeting of the General Advisory Committee at the end of October 1949?

A Yes, sir.

Q That would have been not long after the announcement of the Russian explosion of the atomic weapon?

A I don't know whether they had their first atomic explosion or not, but your records must show.

Q To refresh your recollection, the announcement of the Russian explosion was at the end of September 1949. In all events, do you recall the session of the GAC at which the subject of a crash program for the hydrogen bomb was the subject of debate?

A Quite vividly, yes.

Q Do you recall how the question came to you, how



the question came to the General Advisory Committee?

A My recollection is that it was brought up by the then Chairman of the committee, and asked for --

Q The then Chairman of the Committee?

A Of the Commission, asking for the advice of the General Advisory Committee on whether or not we should enter into a crash program looking toward the development of the H bomb.

Q Do you have any recollection whether that would have been an oral or a written request from the Chairman of the Commission?

A I couldn't say. I never saw the written request that I know of.

Q Would you give an account, as far as you can on the basis of your memory, and without getting into classified materials, of that meeting of the GAC, of its discussions and of your own views on the subject of the crash program for an H bomb?

A My recollection is that it was a pretty soul searching time, and I had rather definite views of my own that the general public had considered the A bomb as the end of all wars, or that we had something that would discourage wars, that would be a deterrent to wars. I was rather loath to enter into a crash program on the H bomb until we had more nearly perfected the military potentialities of the A bomb,

thinking that it would divert too large a portion of the scientific world and too large a portion of the money that would be involved to something that might be good and it might be bad.

Q As far as you yourself were concerned, did you have any qualms about the development of an H bomb or the use of it if it could be developed?

A My position was always against the development of the H bomb.

Q Could you explain that a little?

A There are several reasons. I may be an idealist but I can't see why any people can go from one engine of destruction to another, each of them a thousand times greater in potential destruction, and still retain any normal perspective in regard to their relationships with others countries and also in relationship with peace. I had always felt that if a commensurate effort had been made to come to some understanding with the nations of the world, we might have avoided the development of the H bomb.

Q Did you oppose the actions that the Atomic Energy Commission was taking and with respect to which the General Advisory Committee was advising during the period between 1947 and 1950 to realize the full potential of the A bomb?

A Will you state the question again?

Q Did you oppose the efforts that were made to realize

the full potential of the A bomb during the period 1947 onward?

A Not knowingly, no. We were in that, and my earnest opinion was that we should make the best of it.

Q If you can, would you explain why on the one hand you supported the development of A bombs to their full potential, but at the same time held views that were in opposition to the H bomb?

A I thought the A bomb might be used somewhat as a military weapon in the same order as a cannon or a new device of that sort, and that we perhaps could use it as a deterrent to war, and if war came, if we had all the potentialities of it developed, we would be in a stronger position than if we only had the bomb itself without any of the other characteristic military weapons that were developed later.

Q Why did you distinguish between that and the H bomb?

A Purely as a matter of the order of destruction. The H bomb, according to the papers, this is not classified, is a thousand times more destructive than the A bomb, and you haven't yet reached the potentiality of it.

Q I am not clear whether you are saying that you felt that the B bomb was big enough for our needs.

A I think the A bomb was exploited to its full capacity, yes. I don't like to step up destructiveness in the order of 1,000 times.

Q There has been talk that the H bomb had unlimited

capacity for stepping up destructiveness.

A I believe that to be true. Whether it was portable at that time or not is another question.

Q Could you describe, if you have any recollection, what influence other members of the GAC had on your thinking about the H bomb?

A Very little, if any.

Q Did any of them have any particular influence?

A I think I arrived at my conclusions even before the discussion came before the committee.

Q After the President announced his decision in January 1950 to proceed with an all out program to develop an H bomb, you served on the General Advisory Committee for some months?

A Yes.

Q During that period can you state what your attitude was and what the GAC's attitude was about cooperating in this program which the President had announced?

A I can only state definitely what my attitude was, and that was that we had received a directive and we had to go ahead. From my observations of the other members of the committee, I don't think there was any lag anywhere in either thought or deed. There were great scientific discussions which must necessarily take place before you can organize a procedure and ask for funds for the development of something that was as obscure at the moment as that was.

Q Did you ever notice anything that Dr. Oppenheimer did that was contrary to the course you have just described?

A No, sir.

Q I would like to turn now, Mr. Rowe, to a quite different subject. Have you had any experience with Communism?

A You may be getting me into trouble, because I don't think so. I have had for many years, and recently renewed, was my Q clearance. One of the questions I was asked at that time was whether I ever knew or associated with Communists. My answer was that I knew Communists in Central America, but I had not associated with them. I didn't either know or associate knowingly with any Communists in the United States. Knowing that, I can answer your question.

Q Let us confine the question to Central America. What experience have you had in Central America? How often have you been down there?

A I went to Central America first in 1904 and served 15 years in those countries, and then came back and later went with the United Fruit Company in 1926, and I have made an annual trip to the tropics, with the exception of two war years -- one of them was 1944 and the other was 1946.

Q When you make this annual trip, how much time do you spend in the various Central American countries?

A I have to cover seven or eight countries, and it is usually two or three weeks in each country.

Q Don't answer this question if there is any reason from your own standpoint why you should not. Let me ask you: Is it a matter of business interest to you to know what is going on in these Central American countries politically?

A Oh, absolutely.

Q Would you say that you are familiar with the situation in Guatemala.

A I am familiar with all of the principal things that have taken place there. I don't know of the every day detail in the country. I do know their pattern and that is, it follows a very distinct pattern. In my experience in other countries it always follows the same pattern. They start out by wanting to do something for the common people, and they usually pass what they call an agrarian law, which allows the government to take up any lands that are not being used for other purposes for distribution among the population.

Q Do you think you know in a general way what is going on in Guatemala and how the Communists are manipulating affairs there?

A Yes. You will find that there are very, very few, if any, in elective office. They are always appointed, and they are in the policy positions only. Not often are the presidents of the countries Communist or Communistically inclined until they are in so deep that they can't get out of it.

DR. EVANS: Would you say that again?

THE WITNESS: None of the countries with which I am familiar are any of the out and out Communists that report to the Internationale in Moscow ever elected to office. They don't run for office except very, very seldom. In Guatemala especially there is not a one of them that is in an elective office. They get themselves appointed to policy control officers lower down than the Congress. You will find them appointed as judges very frequently. So that they can control the judicial and also the labor.

BY MR. MARKS:

Q Mr. Rowe, I think it fair to say that the problem before this Board is one of formulating advice to the Atomic Energy Commission on the question of whether it would endanger the common defense and security if Dr. Oppenheimer were permitted to continue to have access to restricted data. In formulating that advice, the Board has to take account of the provisions of the Atomic Energy Act, which stated that the determination should be made on the basis of a man's character, loyalty and associations. Do you have an opinion on this subject?

A Yes, I do.

Q Would you state what your opinion of Dr. Oppenheimer is in the background of the question I have asked?

A I can only speak from my acquaintance with Dr. Oppenheimer during these years that I have outlined to you.

So far as I am personally concerned, and so far as my own observations go, Dr. Oppenheimer is no greater risk than any other American citizen except for one thing, and that is he has a greater knowledge of atomic fission than anyone else that I know of in the country. If you are put in a position of knowing secret and top secret information, the more you know, the greater risk you become, if you are ever in circumstances where you, as our boys have been in Korea -- I don't know how I would react, and I don't know how Dr. Oppenheimer would react to brutal treatment. But in the course of his associations in the United States, I would have no reservation whatever.

Q Are you saying that you have no question as to the loyalty, character or associations?

A None whatever, based on my association with him.

Q Have you taken into account in expressing this personal opinion the fact that at least up to some time in the early forties there is what is described technically as derogatory information, which means that there is an extensive record of associations with left wing and with Communist personalities and affairs?

A I haven't reviewed that testimony thoroughly. I have only read what is in the papers. I have never discussed it with Dr. Oppenheimer at all. Until I knew some more of the surrounding facts and reasons and the climate of public opinion at those times, I would not modify my statement.



Q Would it surprise you if he had such associations and engaged in such activities as I have indicated in that period that the man you know, Dr. Oppenheimer, is a changed man?

MR. ROBB: How as that again?

(Question read.)

THE WITNESS: There are really two questions there.

MR. MARKS: I think it is not a good question. Would you strike it out.

MR. ROBB: Mr. Chairman, I am not objecting and don't intend to, but a thought does occur to me that sometimes the questions are a little bit leading.

MR. MARKS: I think I have asked enough questions, Mr. Robb.

MR. GRAY: You are not making any objection?

MR. ROBB: I am not making any objection. I am just calling attention to that fact for whatever it may be worth.

I have no questions, Mr. Chairman.

MR. GRAY: Mr. Rowe, I was very much interested in your description of your feelings in late 1949 about the development of the H bomb. I think you made it very clear how you felt about it.

I would like to ask you whether you ever, in thinking about our problem and what we should do in this country, whether it was a source of concern to you that the Soviet Union might be working and perhaps successfully, towards the

development of this kind of weapon. Perhaps my question is does that make any difference to you at all?

THE WITNESS: It makes some difference, yes, but I would place more reliance on the proper use of the A bomb without the H bomb unless it developed as it did later that we had to go into it as a deterrent. I don't think it will ever be used against our enemies. I am quite concerned as to whether we would ever use the A bomb or the A bomb artillery or other military weapons.

MR. GRAY: Some witnesses who have come before this Board have testified that the news of the Soviet success in early fall, whenever it was, September, announced in September --

THE WITNESS: You mean last year?

MR. GRAY: No, I mean in 1949, the A bomb of the Soviet.

THE WITNESS: Yes.

MR. GRAY: Some witnesses have testified that at that point they felt that we should do something to regain our lead, in the way it has been expressed, I believe; that we had a margin of advantage we thought over a possible enemy, and the one with whom we would most likely be engaged in conflict if we became so engaged, that with the announcement of the Soviet explosion it appeared that the lead we had might dwindle and perhaps not continue to be a lead, and therefore something

should be done to regain it. Do I understand your testimony correctly in thinking that you felt that proper exploitation of the weapon we already had and the knowledge we already had would have enabled us to maintain the lead, or was that important?

THE WITNESS: I wasn't thinking so much of the lead, but I thought it would be more effective, and we would have a better balanced military arm, the Army, the Navy and the Air Force. Whatever you take away from any one of those three is going to unbalance them. A trade of the effort being put on the H bomb would detract from the things that needed to be done to get new weapons so that in the next world war we would not be fighting the war with the weapons of the previous war, as we have in the last two. It seemed to me we had a much better chance militarywise in perfecting our A bomb weapons. You understand what I mean by the different kind of weapons?

MR. GRAY: Yes, sir.

THE WITNESS: That it would be to devote that effort to producing something that was a thousand times worse in explosive power at least, and can only be used in my opinion in retaliation. I don't think it has any place in a military campaign at all. Then if you used it in retaliation, you are using it against civilization, and not against the military.

I have that distinction very clearly in my mind. I don't like to see women and children killed wholesale because the male element of the human race are so stupid that they can't get out of war and keep out of war.

MR. GRAY: I would like to turn to something else for a moment. You have read General Nichols' letter and Dr. Oppenheimer's reply?

THE WITNESS: Yes, sir.

MR. GRAY: Do you reel that your present conviction about Dr. Oppenheimer's character, loyalty and associations would be the same if you knew that the information contained in the Nichols letter by early associations was true. Would your reply still be the same?

Let me repeat, Mr. Rowe, I am not saying that it is or is not true. Can you assume this derogatory information and still arrive at the answer you gave to Mr. Marks' question?

THE WITNESS: I think my answer to that would be I would make it just that much stronger because people make mistakes and people in the climate of public opinion in those days which was quite different than it is now -- we know a great deal more than we did then -- I think a man of Dr. Oppenheimer's character is not going to make the same mistake twice. I would say he was all the more trustworthy for the mistakes he made.

MR. GRAY: Let us not use Dr. Oppenheimer's name

in the next question or in reply to it. Do you feel that a man might have been in the late Thirties or early Forties a member of the Communist Party and in 1954 not be a security risk with respect to the most highly classified information?

THE WITNESS: That is rather hard to answer categorically, but a great many men would be a better risk. I would not say that they would all be a better risk.

MR. GRAY: What you are saying is that it is possible for a man to have been a Communist and to have so completely renounced that that he would not be a security risk in later years?

THE WITNESS: Yes, sir, that is what I am trying to say. Remember we all had an opinion during the depression days that our government was lacking in some respects. It was discussed in almost every meeting of men that got together. We did not seem to know how to cope and cope quickly with a condition that was facing us. There were all sorts of opinions, that we should follow the British constitution, that we should do this, that we should do that, we should do the other. One characteristic solution that I heard was that you should arm every other man with a pistol and let him go out and shoot one man, and that would cure the unemployment in very short order. Those points don't come from the heart or from the mind. They are just discussion.

MR. GRAY: I believe you indicated that you felt

that a man who had had no Communist associations might logically be expected generally speaking to be a better security risk than one who might have had such connections. I don't want to make a statement that does not represent your view at all.

THE WITNESS: I can't answer that for everybody.

MR. GRAY: I think you were careful to say that it would be important to know who the individual was.

THE WITNESS: And how he reacted to a mistake.

MR. GRAY: I think I can ask my next question which will cover what I am driving at. You would urge that the government would take whatever chance there was in a situation with an individual who might have had these associations and who apparently had renounced them. You would say if there is any chance the government ought to take it?

Excuse me, Mr. Rowe, I am really trying to get what your view is. This obviously is the kind of question that this Board must ask itself.

THE WITNESS: I understand your predicament.

MR. GRAY: I am doing a very poor job of putting my questions. I am not experienced in this kind of procedure.

THE WITNESS: In a great many instances the man would be a better risk knowing more about the Communist Party. I think if I had known more about it in 1930 and 1940, I would have acted quite differently in my business in connection with

my company and in treatment of government officials in these countries which have now become Communistic. I would have a better understanding of what the thing was all about.

MR. GRAY: Again, without asking you to consider that this refers to Dr. Oppenheimer, would your reaction as a citizen of the United States be necessarily unfavorable if you knew that the United States Government had given access to classified material to a former Communist if you were satisfied with the individual?

THE WITNESS: No, sir, that wouldn't worry me a bit.

MR. GRAY: I think you have answered the question which I have had quite a time putting to you. Dr. Evans.

DR. EVANS: I have just one question. You understand the position that this committee is in, don't you?

THE WITNESS: Yes, sir, I believe I do.

DR. EVANS: I hope you do. You are a man that has had experience, and you know what you are talking about. I have just one question to ask you. It is not quite the same as the Chairman was asking you.

If you had a lot of secret information in your mind, and you had some friends that were Communists, would you be in a more dangerous position than if you didn't have those Communist friends?

THE WITNESS: You probably would, yes, sir.

DR. EVANS: That is all I have to ask.

MR. GRAY: Mr. Marks, do you have any other questions?

BY MR. MARKS:

Q You used the expression, Mr. Rowe, in answer to some questions that were asked by the Chairman "better security risk."

I am not sure I understood what you meant by the term "better security risk." Let me put it this way. What is the difference between a man who is not a security risk in your opinion and a man who is a better security risk?

A His character.

Q Which of those two men would you trust most?

A The man I thought had the best character.

Q What I am trying to get at is -- it is just that I don't quite understand the sense in which you are using the term -- would you trust most the man that you regard as a better security risk or the man whom you simply regarded as not a security risk?

A What I was trying to bring out is that there are different degrees of security risks. The more secret information a man has, the more likely he is to get in difficulties if then it came to a point where he was subject to torture. That is what I was trying to distinguish between a small amount of secret information and a large amount of secret information.

MR. GRAY: A man with the greater amount would



involve a greater security risk, that is what you said?

THE WITNESS: That would be his personal risk.

BY MR. MARKS:

Q Do you think based on your experience with Dr. Oppenheimer he would have any difficulty, as you know him today, in exercising discretion not to reveal secret information or information he ought not to reveal to unauthorized individuals?

A I certainly do. I trust him implicitly.

MR. ROBB: I have no questions.

MR. GRAY: Thank you very much, Mr. Rowe.

THE WITNESS: Yes, sir.

(Witness excused.)

MR. GARRISON: May we have a short recess?

MR. GRAY: Yes.

(Brief recess.)

MR. GRAY: Do you wish to testify under oath?

DR. DuBRIDGE: As you wish; whichever you prefer.

MR. GRAY: You are not required to, but every witness who has come has done so.

DR. DuBRIDGE: Yes, I will be glad to.

MR. GRAY: What is your full name?

DR. DuBRIDGE: Lee Alvin DuBridge.

MR. GRAY: Lee Alvin DuBridge, do you swear that the testimony you are to give the Board shall be the truth, the whole truth and nothing but the truth, so help you God?

DR. DuBRIDGE: I do.

Whereupon,

LEE ALVIN DuBRIDGE

was called as a witness, and having been first duly sworn, was examined and testified as follows:

MR. GRAY: Would you sit down, please, sir.

I must mention to you the existence of the perjury statutes. I assume you are familiar with them and it is not necessary to review them.

THE WITNESS: Yes.

MR. GRAY: I should like to ask that if at any time during your testimony it becomes necessary to refer to or disclose restricted data that you will notify me in advance so that we might take certain appropriate and necessary steps.

THE WITNESS: Yes, sir. You wish the answer even if it does include restricted data.

MR. GRAY: Yes, that is correct. If you can answer a question without referring to something of that sort, let us know and then we will find out whether to put the question or not to put it. I should point out to you that we consider this proceeding a confidential matter between the Atomic Energy Commission and its officials on the one hand, and Dr. Oppenheimer and his representatives and witnesses on the other. The Commission will undertake no initiative in release of information about these proceedings. On behalf of the Board,

I express the hope to each witness that he will follow the same course.

Mr. Garrison.

DIRECT EXAMINATION

BY MR. GARRISON:

Q Dr. DuBridge, will you state your present position?

A I am the president of the California Institute of Technology in Pasadena, California.

Q "Would you tell the Board what government positions you have held and now hold?"

A The list that I have held is somewhat long.

Q Just the main ones.

A I don't have the complete list before me, but among them, the ones I would consider pertinent are the following: I was appointed by the President in 1946 as a member of the General Advisory Committee of the Atomic Energy Commission for a six year term which expired in 1952. This term was coincidental with the term of Dr. Oppenheimer and Dr. Conant.

I am now Chairman of the Science Advisory Committee of the Office of Defense Mobilization, a committee which was established under the chairmanship of Dr. Oliver Buckley, some two or three years ago, and I succeeded Dr. Buckley as chairman a little over a year ago. Dr. Oppenheimer has been a member of this committee also.

I was for a term a member of the Naval Research

Advisory Committee of the Department of the Navy and a member of the Advisory Panel of the U. S. Army.

For a term I was also a member of the Science Advisory Board to the U. S. Air Force.

Those I think are the principal advisory positions I have held since the war in the government.

Q What has been the general nature of your acquaintance with Dr. Oppenheimer? About when did you first meet him?

A I met him first some time in the Thirties as a physicist at Physical Society meetings and seminars. My first clear recollection is hearing him talk at a seminar at the University of Minnesota. I saw him occasionally during the Thirties at Physical Society meetings, but was not intimately acquainted with him.

In 1939 I spent the summer doing research at the Radiation Laboratory at the University of California, just as a summer period of relaxation and refreshment, and work and became a little bit better acquainted with him personally at that time. At least on one occasion I was invited to his home.

During the war I was at MIT in the Radiation Laboratory there which had nothing to do with the Radiation Laboratory of the University of California. We were working on radar. I did not see Dr. Oppenheimer during that period very much, since he was at Berkeley and later Los Alamos.

The beginning of what I would call our close

friendship, however, occurred in May 1945, when he requested that I come to Los Alamos with one of the members of our Radiation Laboratory to consult with the Los Alamos staff on some of the electronic and production problems which were being faced by the Los Alamos group, and particularly to discuss which members of the electronics group at MIT might be transferred to Los Alamos to assist in their work. I spent a week at Los Alamos at that time.

Following the war when we both became members of the General Advisory Committee, we also became what I consider to be good friends, and our friendship has continued since that time.

During the last years since 1946, I have frequently been a guest in his home and have seen him in Washington, of course, at many meetings where we have spent long hours together in the meeting room and outside. He has visited Pasadena. He was incidentally a member of the faculty of the California Institute of Technology when I arrived there as president in the summer of 1946. However, shortly thereafter he left to assume his present position at the Institute for Advance Study. So for a short time we were associated in Pasadena. Does that cover the situation?

Q Yes. Of course, he has been with you on the Science Advisory Committee, I think you said?

A That is correct, yes.

Q I want to ask you a little about the work of the General Advisory Committee from its inception up to the October 1949 meeting. I want to ask you a few questions about that meeting and then a few questions about what happened in the GAC after President Truman gave the go-ahead on the all out program for the H Bomb.

We have had a good deal of testimony already on these subjects. I don't expect an exhaustive discussion from you, but I would like you to tell the Board a few of the things that stand out in your memory during the period from the beginning of the GAC up to October 1949 in the way of recommendations made by the GAC to the Commission and what part Dr. Oppenheimer played in that effort.

A As you are aware, this is a very large subject, and I can only repeat a few things that come to mind that would seem to me to be pertinent. If I may say so, Mr. Chairman, it is my understanding that the object of this hearing is to secure information that casts light on Dr. Oppenheimer as a loyal citizen of the United States, and as a good security risk.

Some of the things that might have happened in GAC arguments back and forth, I think are irrelevant to that question.

MR. GRAE Did you say irrelevant?

THE WITNESS: Irrelevant to the question of security risk and loyalty. But I will start back with the beginning

and hit a few points that occur to me.

When the General Advisory Committee first was assembled, at its first meeting early in 1947, it was apparent to us largely from the reports which Dr. Oppenheimer presented to the General Advisory Committee, but also reports we received directly from the Director of Los Alamos, that the Los Alamos Laboratory was in a state of very considerable disruption. The end of the war had brought about the desire on the part of the scientists there, a large number of them, to return to their universities or their industrial positions, and to resume their normal scientific careers and a very large number of course did that.

This left the top level positions of Los Alamos, many of them vacant. They were quickly filled by bringing up younger men, but these were men with lesser experience and less maturity. The departure of many key scientists of course left the laboratory in a state of demoralization.

There had been a year's lag between the end of the war and the passage of the Atomic Energy Act, a year in which uncertainty about the future of Los Alamos and the atomic energy project was current. The members of the Los Alamos Laboratory did not know what their future was to be as individuals or their function in atomic energy work. This was true of other laboratories, too. Therefore, the General Advisory Committee considered this as an important function in

getting started and this came in a question asked by the Atomic Energy Commission: How can we restore, reestablish, strengthen the Los Alamos Laboratory as an effective weapon development laboratory.

It was evident at that time the most important thing that the Atomic Energy Commission faced was how to bring the atomic weapons work back to full strength. It was evident to us that peacetime applications of atomic energy were somewhat remote, would be somewhat difficult to proceed with at that time and that in view of the shortage of raw materials, the shortage of scientists, it was clear that the weapons program was the most important program to push forward, and the major job was how to strengthen Los Alamos, get better men there, and give them who were there the maximum amount of scientific help.

Repeatedly this question came before the General Advisory Committee in session after session during those two years. It was always evident that the Chairman of the General Advisory Committee was among the most insistent, that this was our job, to help Los Alamos and strengthen the weapons program at Los Alamos.

A special weapons committee was appointed, a subcommittee of the GAC, which I was not a member of, which paid visits to Los Alamos following the weapon program. Dr. Oppenheimer and Dr. Rabi and Dr. Conant were on the committee,



and have or will tell you more about the work of that committee.

The objective of all members of the General Advisory Committee, especially under the leadership of our Chairman, was the strengthening of the U. S. military position in the field of atomic weapons, and doing this by using our scientific experience and technological work in process in Commission laboratories bearing on the weapons program especially at Los Alamos.

It was also evident to us that a critical bottleneck in the production of more and better atomic weapons was the availability of raw materials, plutonium particularly. So we discussed and made recommendations to the Commission at various times at various meetings for the expansion and improvement of the production facilities at Hanford. We felt it was quite important to increase the rate of production of plutonium and to expand the neutron yield of the Hanford reactors, and to increase the plutonium production there.

At various times we made recommendations, some of which eventually were adopted; others were not.

These matters of improving our weapons position and our fissionable materials position engaged a very large section of the attention of the General Advisory Committee during those days. We discussed also how the general scientific picture of the country would be strengthened especially in the nuclear physics and nuclear science areas

through the Atomic Energy Commission support of scientific activities, through a fellowship program and so on. But never far beneath the surface of our discussions was the question of military strength of the United States in the atomic weapons field.

I may say that throughout the discussion on the General Advisory Committee we had many long and earnest discussions. We usually met for three days at a time and often went through the evenings, always informally in the evenings if not formally, and it was a very hardworking committee. Always was the feeling of urgency and of concern that we should advise the Commission properly in ways that would strengthen the United States.

There were disagreements at times, of course, among members of the committee. That is the reason you have a committee rather than one person, so that different points of view can be represented. These points of view were brought forward frankly and given full discussion in all cases. But in the end almost invariably the recommendations of our committee were unanimous.

There were occasional minority reports. These were never suppressed. But they were also written up when they seemed important and wished by the minority members and sent to the Commission along with the majority report of the members of the Committee.

This is the general tone and tenor of the discussions of our committee.

Q Do you have any comment on Dr. Oppenheimer's part in all this?

A Even if Dr. Oppenheimer had not been officially elected chairman each year, and if I may say so, he resigned or attempted to resign each year, feeling that a new chairman should be elected, the committee unanimously rejected his recommendation every year, and asked him to continue to serve as chairman. He was so naturally a leader of our group that it was impossible to imagine that he should not be in the chair. He was the leader of our group first because his knowledge of the atomic energy work was far more intimate than that of any other member of the committee. He had obviously been more intimately involved in the actual scientific work of the Manhattan Project than any other person on our committee. He was a natural leader because we respected his intelligence, his judgment, his personal attitude toward the work of the Commission, and the committee. Of course, without saying we had not the faintest doubt of his loyalty. More than that, we felt, and I feel that there is no one who has exhibited his loyalty to this country more spectacularly than Dr. Oppenheimer. He was a natural and a respected and at all times a loved leader of that group.

At the same time I should emphasize that at no time

did he dominate the group or did he suppress opinions that did not agree with his own. In fact, he encouraged a full and free and frank exchange of ideas throughout the full history of the committee. That is the reason we liked him as a leader, because though he did lead and stimulate and inform us and help us in our decisions, he never dominated nor suppressed contrary or different opinions. There was a free, full, frank exchange, and it was one of the finest committees that I ever had the privilege to serve on for that reason.

Q Coming now to the October 29, 1949 meeting at which the question of the crash program for the H bomb was discussed at great length, do you recall how the topic of the so-called crash program for the H bomb came up to the GAC?

A This is a matter of recollection of a particular thing that happened. I will have to tell it in rather general terms though I am sure the records of the committee must be available to you.

It is my recollection that as the committee assembled for this meeting, we were informed by the Chairman that a question which was before us for consideration was whether a large undertaking should be initiated by the United States.

Q You say the Chairman?

A The Chairman stated to the committee.

Q The chairman of --

A The Chairman of our Committee, Dr. Oppenheimer,

stated to the Advisory Committee that a matter we should consider was the question of whether the United States should embark upon a large production program aimed at the production of hydrogen weapons, and the particular version of the hydrogen weapon which was then called the Super. This production program involved first --

Q May I go back a minute to ask you whether the members of the AEC met with the GAC before you went into your meeting? Let me ask you the question, are you talking now about the meeting of the GAC members themselves, or are you talking about the beginning of the session which, as I understand it, the practice was that the members of the AEC met with the GAC.

A I am sorry I don't recall that particular meeting. Sometimes we met with the members of the Atomic Energy Commission at the beginning of our session, sometimes in the middle or at the end, or sometimes several times. I just simply do not recall whether in this particular session we met with the Commissioners first. I am sorry I do not recollect that. I do have a vivid recollection of Dr. Oppenheimer presenting to us the question, when the Commissioners were not present, only the committee was assembled. Dr. Oppenheimer presented to the committee this question: Shall we advise the Commission to embark upon this program? This proposal involved the construction of large reactors designed for the production of tritium.

At this point, Mr. Chairman, I am not sure whether what I want to say contains restricted material or not.

Q I think I could perhaps just ask you a few questions that will avoid that, because we have had quite a little testimony about what happened, and I want to bring out just a few points.

I would like to ask you a few questions about the report itself. I understand about the report, but what I want to ask you about is the two annexes, one signed by yourself and Dr. Conant and Dr. Oppenheimer and Mr. Rowe, as I recall, and the other by Dr. Rabi and Dr. Fermi. Perhaps I have left out somebody of the majority. Do you recall who drafted the so-called majority annex?

A I think it went something like this. May I go back just a moment? After this question was posed by Dr. Oppenheimer to the committee for its consideration -- and I will not attempt to state the full technical content of that question at the moment -- Dr. Oppenheimer asked the members of the committee if they would in turn around the table express their views on this question. The way in which the committee happened to be seated at the table, I was either the last or the next to last to express my views.

The Chairman, Dr. Oppenheimer, did not express his point of view on this question until after all of the rest of the members of the committee had expressed themselves.

It was clear, however, as the individual members did express their opinions as we went around the table, that while there were differing points of view, different reasons, different methods of thinking, different methods of approach to the problem, that each member came essentially to the same conclusion, namely, there were better things the United States could do at that time than to embark upon this Super program.

Q These discussions I take it ranged over several days.

A This particular phase was in one session in one half day. Later after we had gone around the table and expressed our opinion, we then elaborated and explored, wrote up drafts, argued about them, redrafted and so on, for at least two days. But to get the problem before us, the Chairman simply asked each member of the committee to make a brief statement, and I suppose each person took five to ten minutes or thereabouts to express his views.

After they were all on the table, the Chairman said he also shared the views of the committee. We then discussed the question of how to state our views and our recommendations most effectively to the Commission.

It was on this subject of how our general conclusions could be most effectively and clearly stated that a very substantial discussion went forward for the next day or two.

It is my recollection that Dr. Conant and myself

and possibly at least one other were on one committee to make a draft, and that Dr. Rabi and Dr. Fermi were asked to make another draft. These two groups retired and prepared their respective drafts, and came back to the committee meeting and read them.

We criticized each other's draft, made suggested changes and discussed the question at greater length and eventually came out with these two versions.

Q There has been testimony here as to the views of different members of the GAC. I don't want to ask you to attempt to reconstruct in detail the majority annex which is not in the record, but I would like to have you state to the Board as simply as you can your recollection of the position which you held at the time on this subject, how you felt about it, and why.

A Recalling as nearly as I can, projecting my thoughts back five years or four and a half, it went something like this: First, though I was not intimately familiar with the technique of the atomic and hydrogen bomb design, it was my impression that the Super design, which was then being considered, was a design which it was not clear would be operable. At least it was in too early a stage to embark on a large and expensive program. In other words, there were technical reasons why a crash program at that time seemed unwise.

Secondly, it was clear in my mind that the fission



weapon program was progressing quite well, that better designs of fission weapons had been developed over the two or three years immediately preceding that time, that both larger in point of view of energy and smaller fission weapons had been evolved, and were designed and still further progress was rapidly being made. That we were, in other words, rapidly attaining a position of great strength in the fission weapon field. That some of these fission weapons were very much larger in their energy release than the original fission weapons exploded over Japan. That very much more efficient ways of using our fissionable material had been found so that our stockpile with a given number of pounds of fissionable material had greatly multiplied, and was in the process of being further multiplied.

Therefore, it was to the best interest of the United States to proceed as rapidly as possible to continue this development and improvement of our fission weapons so that our stockpile would be more effectively used, and our weapon strength would be further increased for a variety of military purposes. Small weapons for tactical purposes, and very large weapons for strategic purposes.

MR. ROBB: Is this the majority report?

THE WITNESS: This is my view as I recall it at the time.

MR. ROBB: This is the separate opinion of Dr. DuBridge,

and the other gentlemen who joined with him?

MR. GARRISON: I am not asking him to recollect in detail the precise order of language and so forth in the majority report.

MR. ROBB: I understand. I want to have it clear in the record if we can which particular report he was talking about.

THE WITNESS: As I understood the question, it was to give my own views as to the hydrogen weapon at the time. To some extent these were reflected in the report, to some extent they were not.

MR. ROBB: I see.

THE WITNESS: If we made any mistake in our reports, the mistake was in not amplifying and giving our views. I think we made our reports too brief, and therefore they were not understood. Therefore, much of what I am saying is opinion I held as I recall it, and I am not sure just how much was written down. Only a small part of that actually. Therefore, there were technical reasons for not thinking that the Super was ready for production. There were important reasons for thinking that there were more fruitful things at Los Alamos, and the other laboratories could proceed on the fission program.

The fission weapon program was that such that a very large destructive power was in our hands, and it was not

clear to me that the thermonuclear weapons would add in significant ways to that destructive power.

Finally, there was a question of whether the United States could not find a better way of strengthening, rather than deteriorating its moral position with the rest of the world. It seemed to me and to some other members -- I think all of the members of the committee -- that if the United States, instead of making a unilateral announcement that it was proceeding with this new and terribly destructive weapon, should instead say to the world that such a weapon may be possible, but we would like to discuss methods of reaching agreements where no nation would proceed with the design and construction of such a weapon.

It seemed to me at the time that the moral position of the United States in the face of the rest of the world would be better if we took that kind of a stand rather than making a unilateral announcement that we were proceeding with this new weapon of mass destruction. That as I recollect it was the background of my thinking at that time.

I must say that I cannot claim credit for originality in these thoughts. These thoughts evolved from my discussions with the other members of the committee. But as nearly as I can reconstruct my thoughts at that time, that is it.

BY MR. GARRISON:

Q After this October meeting you had another meeting in

the first week in December and resumed the discussions, did you not?

A Yes.

Q After President Truman's direction to proceed with the program, did the GAC under Dr. Oppenheimer's chairmanship cooperate and try to carry out the President's directive, and if so, in general what did it do?

A During the October meeting and during all the time immediately following that before our next meeting, I should make it clear that the only objective of the committee -- and I am confident, of its chairman -- was to increase the strength of the United States. All of the arguments and recommendations were aimed at that end. There was not the slightest question about that in any of our minds. If there were differences of opinion, these were honest differences of opinion as scientists and had nothing to do with our objectives in improving the position of the United States morally and physically. Though our recommendations as transmitted to the Commission were not accepted by the President of the United States, when we next met the announcement of the President of the United States was made, as I recall, during our meeting, and it was then clear to us that the decision of the United States had been made, that it was our job then to collaborate and cooperate fully in carrying forward this decision.

From that time forward I recall of no argument within

the committee but that we had only one duty, and that was to implement the decision of the government in proceeding with this project.

Q Did Dr. Oppenheimer agree with that?

A Fully and completely.

Q Do you want to say anything more about what the committee itself actually did to help implement the program?

A This was a matter mostly of technical assistance to the Los Alamos laboratory in which I personally was not competent to participate. By discussing the program with the members of the Los Alamos Laboratory and others like Dr. Hans Bethe, I think substantial assistance was rendered by members of our committee individually and collectively to the program. I think a conspicuous piece of assistance to the thermonuclear program was a conference which Dr. Oppenheimer called at Princeton, I believe in June of 1951, at which time the purpose of this conference was to review the entire technical status of the thermonuclear program.

The members of the General Advisory Committee were all invited to this conference, and the members of the Commission. In addition, a number of the key staff members of Los Alamos including Dr. Bradbury, consultants of Los Alamos, including Dr. Bethe and Dr. Teller. This conference lasted two/ or three days, I have forgotten which, and was a long and extensive and intensive examination of the technical problem of the thermonuclear

program.

There were many technical ideas which had been considered which were then being considered and being examined, and these were all laid out, and discussed in great detail, with an attempt to find out where is the best and most promising line of procedure with what was known at that time.

I believe that this conference was a held at a critical time and was a critical and important assistance to clarifying ideas of the technical problems involved, and illustrating the next steps in the theoretical and experimental program of the laboratory. At various times during the months and years that followed, we were asked to give technical opinions on various aspects of the thermonuclear program and we did this as earnestly and carefully as we could.

Our objective was always to help the Commission in its work and since its job was to carry forward this program we considered it our job to help. In this, as in every other matter, the Chairman was our leader in this effort.

Q Mr. Walter Whitman testified this morning about visiting SHAEF in connection with the Vista Report. I believe you were the head of the Vista Project?

A Yes, sir.

Q And you accompanied Mr. Whitman and Dr. Oppenheimer and Dr. Lauritsen on this trip to Europe?

A That is correct.

Q I don't want to go into the details because there was a good deal of testimony about it. I would just ask you in a general way whether Dr. Oppenheimer contributed in any respect to the usefulness of this project?

A I think if I may, I would like to say a word about the Vista Project. This was a project which the Air Force, the Army and the Navy asked the California Institute of Technology to undertake, to examine some of the problems being faced by the Air Force and the Army, particularly the collaboration between the Air Force and the Army in tactical air operations. It was broadened to include the general operations of the Army and Air Force problems.

A substantial group was assembled at the California Institute of Technology during the summer of 1951 to examine these problems. We made extensive trips to Army, Air Force and Navy installations, had a very large number of Army, Navy and Air Force officers visit the Institute to discuss and give us information and background on these problems. As the late summer came along, the group which had been assigned under the chairmanship of Dr. Robert Serber, then a member of the California Institute staff, to examine the use of atomic weapons in Army and Air operations and had made substantial progress in their thinking about the tactical uses of them.

This was on the potential battle of Western Europe.

In order to make our problem definite, we examined what might happen in Western Europe in case of a Russian invasion into that area and what weapons and techniques the Army, Navy and Air Force could use in combating such an invasion.

One group, as I say, under Dr. Bacher had the special responsibility in asking to what extent and in what way and under what conditions would atomic weapons be useful in such a battle. By the end of the summer a fairly complete chapter of our final report had been prepared on the subject.

Dr. Bacher and Dr. Christie and the others on this group suggested to me that it would be useful to think Dr. Oppenheimer could be invited to come out and spend a little time with the Vista group to consult further on this subject. At our invitation Dr. Oppenheimer did come to Pasadena, and we discussed this subject at great length. He was of assistance in taking the draft of the chapter which had already been prepared and discussing the best method of presenting it, and threshing out further ideas and assisting the group in clarifying this idea and preparing a final draft.

During the course of the Vista discussions, many problems came up in regard to the battle of Western Europe where we did not have the information about organization, forces, the NATO structure and the NATO problems, and we thought it would be helpful, after assembling our own ideas, if we could go over to Europe and consult the leaders, General Eisenhower



and the other leaders, of the U. S. Forces in Europe, to get the information which they had available and to discuss with them their thoughts about the battle of Western Europe, if it should occur.

I think it was during a discussion at which Dr. Oppenheimer was present, at which we were exploring ideas with John McCone, who either at that time still was or had just retired as Assistant Secretary of Air Force under Mr. Finletter, John McCone urged this trip and offered to assist us in arranging it, and it was finally arranged through the Secretary of Defense, Mr. Lovett, that a group of the Vista Project people headed by myself and after some discussion the other members of the group to include Dr. Oppenheimer and Dr. Lauritsen, to go to Europe, and Dr. Lovett offered the facilities of the Department of Defense to make this trip possible, appointed Mr. Whitman, who was then Chairman of the Research and Development Board, to make all the administrative arrangements and to accompany us on behalf of the Secretary of Defense.

The four of us then went to Paris in the fall of 1951, I think November. We went to Paris. We saw General Eisenhower on two occasions and we went up to Weissbaden and met with General Norstadt and Air Force officials. We went to Heidleberg and met with U. S. Army commanders, returned -- I am sorry, General Norstadt has headquarters not at Weissbaden,

but at Fotenbleau, south of Paris, where General Norstadt was located, and we discussed things with him there.

Through all these discussions with the Army as to their problems with tanks, mines and the possible manouvers which they would undergo to avoid atomic attacks by the Russians, the ways in which they would try to focus and funnel and channel an enemy attack so that it would be a good atomic target, the problems which the Army faced in laying mine fields, and so on, the problems which the Air Force faced in having enough air planes, the right kind of airplanes, cooperating with the Army and so on, in all these discussions all four of us took an active part. I felt these discussions were very illuminating. They helped us firm our own ideas that went into the final Vista report.

General Eisenhower's thoughts were particularly helpful. We had lunch with him and a long discussion with him on the general problem of the defense of Western Europe. It was obvious that the group was well picked, I felt. Dr. Lauritsen and Dr. Oppenheimer and Mr. Whitman were all important contributors to the effectiveness of our discussions.

Q To what extent, if you know, and if you don't, tell us, have the recommendations of the Vista Report been put into effect?

A To what extent have they been?

Q Yes.

A It is rather interesting that Dr. Lauritsen is at this moment engaged with a committee which has been established at the request of the Chief of Staff of the Army to examine into this question of how effectively the Army is implementing the recommendations of the Vista Report. He is now visiting Fort Monroe and Fort Bragg, I believe, in an attempt to find out, which of the Vista Report recommendations are being implemented and how effectively. A report is to be made to the Chief of Staff of the Army on this subject within the next two weeks.

Q In general has there been a movement toward the equipment of the forces in Western Europe with atomic weapons?

A I think in the field of atomic weapons the Vista Report was one of the first reports strongly to emphasize the potential importance of atomic weapons in tactical air operations. There had been many in the Army and in the Air Force who advocated the use of atomic weapons for this purpose and a number of officers discussed the use of atomic weapons with the members of the Vista staff. We became convinced that atomic weapons were available and more of different varieties would be available in the future small enough to be carried by small aircraft, and therefore useable in tactical operations, close-in operations against enemy tactical installations, troop concentrations, tank columns, supply dumps, tactical air fields, and so on.

We strongly advocated that the Air Force and the Army examine more carefully this potentiality of using the smaller type of atomic weapon, smaller in physical dimension, as well as in yield, for these tactical air operations. We were not allowed to say in the report how much fissionable material the United States possessed at that time, though some members of our group knew. We were therefore not able to illustrate quantitatively the argument which we felt was sound, that the time had approached or would soon be here when there was sufficient fissionable material so that the strategic needs could be met and also material be made available for tactical uses. That time is certainly here now.

We felt that it was here in 1951, at least when plans should be made for using some of our supply of fissionable materials in tactical operations. This was one of the features of our report, that is, advocating in some detail how it could be done, and under what conditions and how must economically and effectively our fissionable material could be used in assisting in the defense of Western Europe.

Q Without going into details and without touching upon classified material, have those recommendations been carried out to any extent?

A To the best of my knowlege, and I am not fully aware of the present plans -- I will be in Paris again next week and may find out more about it -- the idea of the tactical use

of atomic weapons has now long been accepted as an important and essential tactical idea and operation. I believe that the Vista Report had some influence on this. Whether it did or not, at least by now tactical air operation with the use of atomic weapons is an accepted technique and doctrine.

Q You have read General Nichols' letter of December 23, 1953. You have read the items of derogatory information in it. Assuming that those items of derogatory information were true and without saying whether they are or not, what would your opinion be as to the loyalty of Dr. Oppenheimer, except for the hydrogen bomb allegation which I left out for purposes of this question.

A You prefer to leave them out.

Q Yes. I think that is of a different character.

A It has always been, ever since reading this letter of General Nichols, difficult for me to see how any of the allegations therein had any significant relevance to the question of the loyalty and integrity of Dr. Oppenheimer. Some of the statements made in that letter having to do with acquaintances and associations and friends Dr. Oppenheimer has said were, of course, true.

Q May I just for a moment remind you that the Atomic Energy Act requires the Board to consider character, associations, and loyalty. Having this frame of reference that the Board here must consider, the character, associations

and loyalty of Dr. Oppenheimer, in determining whether or not his continuance of his clearance would endanger the national safety, having in mind the past associations set forth in the letter, having in mind what you know about Dr. Oppenheimer's character, having in mind what you say that the continuance of his clearance would to any degree endanger the national safety?

A In no degree whatsoever.

Q On what do you base this judgment?

A In the first place, these associations that are mentioned were those of many, many years ago. As I understand it, they have largely long since been terminated, in at least one case by death. In the second place, these were rather natural associations of a person who had strong human interests, interests in human rights and human liberties and human welfare, who had strong revulsions against the growth of dictatorship in Germany, Spain and Italy, and who wanted to express his opposition to such violations of human liberty as he regarded these dictatorships. He therefore found himself among others of like minds, some of whom it turned out were possible members of the Communist Party. But this was only a natural exhibition of his deep interest in human beings and in human liberty and had nothing to do with his devotion to this country, or nothing adverse to do with this country.

In the second place, it seems to me that to question

the integrity and loyalty of a person who has worked hard and devotedly for his country as Dr. Oppenheimer has on such trivial grounds is against all principles of human justice. It seems to me whatever his ideas and associations were in 1935, is quite irrelevant in view of the last years since 1941-42, during which he has shown such a devoted interest to the welfare, security and strength of the United States. Whatever mistakes, if they were mistakes, and I do not suggest that they were, that were made in the Thirties have well been washed out and the value of a man like Dr. Oppenheimer to his country has been adequately and repeatedly proved.

It would be in my opinion against all principles of justice to now not recognize the way in which his loyalty has been proved in a positive way through positive contributions. Furthermore, this country needs men of that kind, and should not deprive itself of their services.

Q I think I should put this question to you because it is something that I want you to bear in mind when I ask you to give me your final judgment.

You are familiar with the Chevalier incident as recited in the Commission's letter.

A That is my only familiarity, what I read in the letter.

Q Supposing that it had been shown here that after Dr. Oppenheimer had had the conversation with Chevalier that for several months he did not report the incident to security

officers, that after he had heard from the security officers at Los Alamos that they were concerned about espionage at Berkeley that in his next trip to Berkeley he told the security officers about Elteton, did not reveal the name of Chevalier and declined to do so. Supposing it was further established that he told the security officers that his friend whose name he would not reveal had contact with the Russian consulate and that there were microfilm facilities for transmitting information, and that the friend had approached three different persons, two or three, three, I think, and suppose that these were untrue statements about the consulate, the microfilm and the three persons, suppose that he was again urged after having been urged by the security officers at Berkeley to reveal the name of his friend, he was again urged by Colonel Lansdale and again declined, he was again urged by General Groves and said he would not do so unless ordered; General Groves said he didn't want to order him to do it, asked him to think it over; General Groves saw him again and said he would have to order him if he would not reveal the name, and at that point Dr. Oppenheimer revealed the name of Chevalier.

I am not trying to ask you now to do anything more than to assume that you had that set of facts before you. Would your conclusion still be the same as you have expressed it here to the Board?



MR. ROBB: Mr. Chairman, I don't object to the question, but I wish it to be recorded that my failure to object does not imply or import that I endorse the complete accuracy or fullness of the hypothesis stated by Mr. Garrison.

MR. GARRISON: I quite understand that. To carry it further I would have to read the whole testimony.

MR. ROBB: I understand. I don't want to debate it.

MR. GARRISON: I want to give Dr. DuBridge the nature and character of the problem.

THE WITNESS: May I ask one question on your assumption? In what year was this supposed to have taken place?

BY MR. GARRISON:

Q 1943. You would regard that seriously, I take it?

A I would want to examine this situation very seriously and what you said about the assumption obviously does not include all the facts. I assume therefore you wish me to answer this from the point of view of my knowledge of Dr. Oppenheimer's character and integrity, and my statement would be without hesitation that I would say that these acts which he is supposed to have committed in no case stem from any disloyalty to the United States, but possibly a mistaken but nevertheless a sincere and honest belief that this was the best thing to do at the time. I just know that Dr. Oppenheimer is loyal to his friend and loyal to his country, that he is honest, but has a humane feeling, that if he did these things

it was with a sense that a loyalty to a friend was important but was not in conflict with any loyalty to the country at that time.

Q Do you think that today if he were asked by security officers to reveal information which they believe to be important for the security of the country, that he would decline to do so even if a friend were involved?

A I am sure that at any time if he had felt a loyalty to his country was involved, he would have done what seemed to be the proper thing to reinforce that loyalty.

Q I am asking you today, leaving aside whether he thought that his friend was innocent or not, if he were told by security officers that in their judgment the interests of the country required knowledge which he had about a friend, would he put the interests of his country ahead of the friendship?

A I am confident that he would. We have all learned a great deal about security problems in the last ten years.

MR. GARRISON: That is all.

#### CROSS EXAMINATION

BY MR. ROBB:

Q Doctor, do you think that loyalty to a friend justifies the giving of false information to a security officer?

A I would not wish to do that myself.

Q You would not do it, would you?

A I don't think so.

Q In fact, you can't conceive of any circumstances under which you would not?

A I wouldn't say that

Q It is hard to think of any?

A First, it is hard to project ourselves back ten years as to what the situation was like then. None of us had any very keen appreciation of the problems of security and secrecy at that time or what was involved. I cannot say under no circumstances would I be reluctant to give away or give information about a friend if I were personally convinced that this information had nothing to do with the country's welfare. I would try to cooperate with security officers under all conditions but I cannot say that under no conditions would I be reluctant to give such information.

Q That was not quite my question. My question was whether or not you would feel that loyalty to your friend justified you in lying to a security officer.

A No, I would not feel so.

Q The standards of honesty were the same in 1943 as they are now, weren't they?

A Presumably.

Q Doctor, I was interested in your discussion of the

Vista matter, As I understand it, what was it called -- a committee?

A It was called a project.

Q That project took place in the summer of 1951.

A That is correct. Our report was completed in early 1952.

Q You said Dr. Oppenheimer was not there when the project commenced, is that it?

A That is right. He was a member of the staff of the project only for a relatively short period.

Q I believe he came out in about November?

A I believe it was before that, but I do not remember the dates.

Q I don't know exactly either.

A I think it was the latter part of the summer, September.

Q Do you recall it was Chapter 5 of the report that dealt with atomic bomb matters?

A That is correct.

Q Did Dr. Oppenheimer prepare an introduction to that chapter?

A Dr. Oppenheimer collaborated with the other members of the committee that were responsible for Chapter 5 in developing Chapter 5. He did not write either the first or the last draft of that chapter. He assisted in the preparation

of one or two intermediate drafts.

Q Was there a time in November when the group was reviewing the report as a whole with you presiding?

A Immediately after our return from Europe?

Q No, sir, I am talking about before you went to Europe.

A We had weekly meetings reviewing various chapters and various parts of the report. I don't know which one you are referring to.

Q I realize it is hard to project yourself back.

A We had many meetings and I was Chairman of most of them.

Q Perhaps I can refresh your memory. I am informed that on November 13, 1951, when the group was reviewing a draft of the report that you announced that Dr. Oppenheimer had prepared a portion of the introduction to the report. Do you remember that?

A I don't recall the exact incident but it is quite possible I did, because he did prepare a draft of a part of Chapter 5 at that time. It was not the final draft, but it was an intermediate one.

Q I am informed that you stated that you considered that to be a great document, and you felt confident it would be accepted without amendment. Do you remember that?

A No, I don't.

Q I am not trying to lead you into something, but trying to find out whether that coincides with your memory.

A I don't remember that meeting or the statement.

At the time I certainly did have the opinion that the draft that Dr. Oppenheimer helped prepare of the introductory portion of the chapter was a fine contribution to the Vista work. I believed that and I still believe it.

Q Was that draft which Dr. Oppenheimer helped to prepare incorporated in the draft which you took to Europe?

A It certainly was incorporated in it, but I am sure there were probably changes in the wording between that time and the time we went to Europe. In other words, there were continuous changes in the wording of all parts of the report.

Q By the way, at those meetings in November, was General Quesada present?

A General Quesada participated in some meetings.

Q Did General Quesada undertake to make available to your group his report on the so-called Greenhouse test?

A I don't recall.

Q In the draft Dr. Oppenheimer helped to prepare, the introduction to the report, was any reference to thermonuclear weapons made?

A In the introduction to Chapter 5?

Q Yes, sir.

Q This is a matter of record whether there was or was not. I don't recall. Certainly in some drafts, and I believe in the final report there was a reference to

thermonuclear weapons.

MR. ROBB: I might say, Mr. Chairman, I am undertaking to do this on an unclassified basis for the benefit of counsel. I suppose ultimately I will have to ask the Doctor some questions on a classified basis and read some extracts I have here, but I don't want to do it if I can help it, because I want Mr. Garrison to hear it.

THE WITNESS: Do you have notes on it?

MR. ROBB: Yes, I have.

MR. GARRISON: I don't want anything withheld from the Board.

MR. ROBB: No, but I am trying to keep out of the classified area.

MR. GRAY: Let us see if you can do it unclassified.

THE WITNESS: Did I make clear in my answer to that question I don't recall at what stage or what draft reference to the thermonuclear weapons came in, but there was a reference and only a passing one, as I recall.

BY MR. ROBB:

Q Do you recall that subsequent to the November meeting, that draft of Chapter 5 or the introduction to it was amended?

A It was amended many times.

Q Was it amended subsequent to that meeting in November?

A Since I don't recall the particular meeting, I can't answer that specifically. I can not even recall at the moment

the date on which we departed on our trip to Europe. May I ask if that date is available? I don't have that date.

MR. GRAY: MR. Robb, do you have it?

MR. ROBB: I am looking now to see if I can find it.

THE WITNESS: These were matters of continuous study and drafting and redrafting and changing and finally we got a version-which we took to Europe. We redrafted pieces of it on various chapters while we were in Europe as a result of our discussions. We came back and redrafted many parts again in the light of what we had learned, and finally got a report which we all agreed was the best we could do, which was submitted then to the Defense Department.

BY MR. ROBB:

Q I have a note here, Doctor, which may assist you that you returned from your visit to Paris and reported to the Vista group on the 18th of December, 1951. That might help you fix the date when you went to Paris. At your meetings in California in the summer and fall of 1951, did you confer with General Quesada?

A Yes, we asked General Quesada to come and discuss these various matters with us and at our invitation he did come.

Q Did you have any report from General Quesada on the Greenhouse test?

A As I say, I just don't remember. We certainly talked with General Quesada about atomic tests. Whether the



Greenhouse test was specifically reported on as such, I don't recall.

Q Was the Greenhouse test exclusively atomic or wasn't that thermonuclear in part?

A I don't know.

MR. BECKERLEY: The public record is that it included experiments in thermonuclear.

MR. ROBB: The answer was that he didn't know.

(Record read by the reporter.)

THE WITNESS: One reason for not recalling is that I never can remember the code words for these various tests.

BY MR. ROBB:

Q I can't either.

A Whether General Quesada reported or not, we certainly knew through various channels because I was still a member of the General Advisory Committee at the time about the Greenhouse test.

Q Doctor, do you remember -- I don't expect you to remember the date, but I will give it to you to assist you -- on April 30, 1952, having lunch with Dr. Rabi, Mr. David Griggs, Mr. Garrison Norton and Mr. William Burden at Mr. Burden's house here in Washington?

A Yes. I can't confirm the date, but I remember approximately that time and I have only had lunch there once with that group.

Q Do you recall that you and Dr. Rabi on that occasion expressed some opinions concerning H bomb development?

A We had a very vigorous discussion of this question, yes.

Q Would you undertake, please, sir, to give us the opinions that you and Dr. Rabi expressed?

A It is a little difficult to try to recall a conversation of two years ago. If I do recall, they were not substantially different from the ones I have already expressed here previously in regard to whether or not the thermonuclear weapons were important additions or were not to the military potential of the United States, and questions, if so, under what conditions they could be used. If you have any specific questions about the statements I made --

Q I can understand how hard it is to remember. Do you recall you and Dr. Rabi saying in substance that you thought that there were two things that were more important than H bomb development, the first being a concerted effort of the best minds in this country toward peace with Soviet Russia. Do you recall something like that?

A That is quite consistent with what I might have said.

Q Do you recall Dr. Rabi saying together with Dr. Oppenheimer and Dr. Lauritsen that he, Dr. Rabi, would press for action in accordance with plans that they were preparing, and that they were already in touch with the State Department

on the subject.

A I don't recall that.

Q Do you recall anything like that?

A I have a faint recollection at this time that there was a committee at work in the State Department on exploring new approaches to an agreement with Russia. I had nothing to do with that committee. Though it is quite possible that Dr. Rabi said something about it, I am inclined to feel that I probably did not express any opinion about it since I did not have personal knowledge about it.

Q I was not suggesting that you did. I was asking if you recall Dr. Rabi saying something about going to the State Department on the subject.

A It is not impossible that he made such a remark.

Q Do you recall that was Dr. Rabi's feeling at the time?

A I think it probably was, namely, that because of the terrifying implications of A bombs and thermonuclear weapons, it was desirable to make another attempt to find away to avoid using them.

Q Do you recall either you or Dr. Rabi or both of you expressing an opinion that the second thing which was more important than H bomb development was more emphasis on having a good air defense?

A We certainly emphasized the importance of an air defense, yes.

Q I believe at that luncheon meeting you said you had quite a go-around with these gentlemen.

A We had a very vigorous discussion with Mr. Griggs.

Q Yes, you put it more delicately than I did.

A I didn't mean it that way. Our discussion was primarily with Dr. Griggs, who disagreed with Dr. Rabi and myself very violently on some points.

Q Dr. Griggs contended that Dr. Oppenheimer had got the GAC to soft pedal the thermonuclear development, didn't he, and you said that was not so?

A That is correct.

MR. GARRISON: Mr. Chairman, could I just ask what is the general nature of the document that Mr. Robb is reading from?

MR. ROBB: I am sorry, it has top secret stamped all over it, Mr. Garrison.

MR. GRAY: Do you wish to make any point of this?

MR. GARRISON: No.

BY MR. ROBB:

Q That was the bone of contention between you in general, that Dr. Griggs said Dr. Oppenheimer and the GAC had not fully supported work on the thermonuclear and you and Dr. Rabi contended that the GAC had consistently supported it and emphasized it?

A Essentially that is correct. Griggs made what we

considered to be false statements, that the GAC had impeded thermonuclear development. We both emphasized strongly that neither Dr. Oppenheimer nor the GAC had impeded the development of thermonuclear weapons. On the contrary, from almost the opening day of the GAC's existence, its Chairman and its members had recommended to the Commission that thermonuclear research proceed and be implemented and strengthened at Los Alamos. We did not feel at the time that the time 1950 was ripe for the production effort, but we always advocated the research and development effort. Our difference of point of view with Dr. Griggs, as I recall, was that he felt that the thermonuclear weapon development and production was No. 1 priority for the country. We felt that improving our fission weapon program and improving our defenses were just as important, if not more important at that time.

Q This was 1952?

A Yes.

Q Was that the view of Dr. Oppenheimer at that time, too?

A It is a little hard to speak as to what his opinions were at any particular moment. I think in general we have agreed with each other. These were technical matters of priority and I must insist that at all times Dr. Oppenheimer, myself, Dr. Rabi and the others had only one objective in mind; that was strengthening the moral and physical and military position of this country. We had no other thought.

MR. GRAY: Excuse me. At one point I am going to ask for a recess, but I don't want to cut you off in the middle of one thing you want to pursue.

MR. ROEBB: I have one question and then I think we might take a recess.

BY MR. ROEBB:

Q Doctor, you testified that the recommendations of the Vista Report were carried out and are still being carried out, is that right?

A In so far as the tactical use of atomic weapons is concerned. There are some other recommendations which were not. There are others that had nothing to do with atomic weapons which are being carried out.

Q Were those recommendations to which you referred the same as the recommendations in the draft which Dr. Oppenheimer helped prepare in the fall of 1951 at Pasadena?

A I believe so.

MR. ROEBB: This is a good time to stop.

MR. GRAY: Let us take a few minutes recess.

(Brief recess.)

(The following portion of testimony, numbered pages 1747 through 1758, is classified, and appears in a separate volume.)

BY MR. ROBB:

Q Doctor, I want to ask you a couple of questions and I want to assure you that when I ask you, I have not the slightest intention of being offensive or suggesting the slightest impropriety on your part. Did you volunteer to be a witness here?

A I am trying to recall how it came about. I would have been glad to volunteer. I think I probably said to Dr. Oppenheimer or his counsel that if there is anything I could do to help, I would be glad to do so.

Q Did you in that connection with helping undertake to raise a fund to assist Dr. Oppenheimer in this matter?

A The newspaper reports in that connection are mistaken. As near as I can tell, the origin of that statement was that at the Cosmos Club here in Washington one day a few weeks ago, several friends said, "Would it not be nice if Oppenheimer's friends chipped in \$100 each to raise a fund to assist him in the expenses of his hearing?" We agreed that this would be nice, and maybe somebody should see the best way of doing it. The matter dropped there, and that is the last I heard of it until I saw the statement in the paper. I do not know where they got that information that I was organizing a fund. I did not and was not and am not. After the thing appeared in the paper I received many letters, however, with checks from individuals who read it in the paper and sent in

their contributions.

Q I was sure you wanted to have the record clear on it.

A I returned all these checks to the donors.

Q Were the friends you were talking to any of the other witnesses who appeared here?

A Some were and some were not.

Q Who were the ones who were witnesses?

A I do not know who else have been witnesses, as a matter of fact.

Q Could you tell us who the friends were?

A Dr. Rabi, I believe, was present at the time the discussion went under way, and Dr. Bacher.

Q Dr. Fermi?

A Dr. Fermi was not present. Mr. Trevor Gardner.

Q Who is he, sir?

A He is the Assistant to the Secretary of the Air Force for Research and Development. I believe that is his title. He is a civilian engineer who was formerly associated with the General Tire and Rubber Company.

Q Was that the group?

A Dr. J. R. Zacharias of MIT was another member. I think it was actually Dr. Zacharias who raised the question.

Q Was that luncheon for the purpose of discussing this case, if we can call it such?

A No. This was just an informal grouping at the Cosmos



Club. The occasion was the last meeting of the Advisory Committee, ODM, of which I am Chairman. These others that I have mentioned, except Mr. Gardner, are members of that committee and we happened to be in town together. Gardner had at our request appeared before the committee that day to discuss some matters so he joined some of us at the Cosmos Club for dinner, I believe. This was a friendly discussion, wouldn't it be nice if we could help our friend.

Q Yes, certainly. About when was that, Doctor, in March?

A May I refer to my diary?

Q Yes, sir.

A I think I can give you the exact date of that last meeting. I believe it was the 12th or 13th of March.

Q Did you see or talk to Dr. Oppenheimer about that time?

A Did I see or talk to him?

A Yes.

A I believe I called him on the telephone just to ask how things are going and to wish him well.

Q Was he in Washington?

A He was in Princeton. I am sorry, no. I called him at Princeton, but they found him somewhere in Washington and I talked to him on the phone.

Q Did you see him?

A I did not see him.

Q What was the substance of your conversation?

A I just said "Robert, how are things going?" It was only a friendly conversation, attempting to express confidence in him and cheer him up if possible.

Q Did Dr. Oppenheimer tell you how things were going?

A He only said it was not a very pleasant experience that he was going through.

Q Anything more?

A Nothing more relating to the substance of this case.

Q That is what I mean. Substance?

A That is right.

Q What was said about the case in addition?

A Just what I said, as I recall. It was not a very pleasant experience for him to be going through.

Q Would it be on that occasion that you suggested to him that you testify or had you previously?

A I had already previously discussed testimony with his counsel before that time.

Q Have you since discussed your testimony with counsel and with Dr. Oppenheimer?

A Have I discussed the testimony?

Q Yes.

A I have not seen Dr. Oppenheimer just before I came here today. I have discussed of course the testimony with his counsel.

Q You understand I am not trying to pry into your affairs, but I think these are matters which the Board ought to have on the record.

A Yes, sir.

Q Did you discuss the case after that with Mr. Gardner?

A Did I discuss the Oppenheimer case?

Q Yes, sir.

A After that time?

Q Yes, sir.

A I have not seen Mr. Gardner -- I think he did come to Pasadena shortly afterwards -- yes, he did, on another business trip, and I think we probably did discuss it. Mr. Gardner has been very much interested in it, very much disturbed that a man so fine and so loyal should be accused, and he has been very anxious to discuss the case. We did discuss it.

Q Has he been active in assisting Dr. Oppenheimer, do you know?

A As Mr. Gardner been active in assisting?

Q Yes, sir, in any way.

A I do not know whether he has seen Dr. Oppenheimer or not, or his counsel. I just don't know.

Q Has he ever told you that he was doing some work for Dr. Oppenheimer?

A No, he never has.

MR. ROBB: That is all I care to ask.

MR. GRAY: Dr. LuBridge, I am going back now briefly to October 29, 1949. Would you consider the two annexes to the GAC report in conflict with one another?

THE WITNESS: Certainly not. Their conclusions were the same. They were slightly different approaches to these conclusions. Dr. Rabi and Dr. Fermi emphasized one aspect of the argument, and the rest of us emphasized another aspect. It was my feeling that these were definitely not in conflict, but only bringing out different points of view, which led essentially to the same conclusion and recommendation.

MR. GRAY: There is something in your testimony that led me to ask whether we could make this kind of distinction with respect to what we have been calling the crash program. You know what I mean when I say that.

THE WITNESS: Yes.

MR. GRAY: I think this kind of distinction has only perhaps just come clear to me. Could there be a distinction between a crash program for the development of a thermonuclear weapon as distinguished from a crash program for the production of same?

THE WITNESS: Of course, yes.

MR. GRAY: Your position was that there should be no crash program for the production?

THE WITNESS: That is correct.

MR. GRAY: did you favor a crash program for

development?

THE WITNESS: We favored the continuation of the research and development program at Los Alamos. We felt that it was going along pretty well. We recommended against at that time a crash program for production, in this case research and development I use together because both aspects are involved. But the research and development programs were in progress at Los Alamos.

MR. GRAY: That was going as fast as it possibly could?

THE WITNESS: We thought it was going along reasonably.

MR. GRAY: There was nothing that could be done to speed up ~~that~~ to get into a crash production program? I am trying to get it clear in my mind because I am still a little confused by the different points of view that are expressed about this thing. I can understand it better if this is a valid distinction.

THE WITNESS: In my opinion it is. The research program began away back --- there was some talk about thermonuclear programs during the war, as you know. I am informed though I was not present at the first discussion of thermonuclear programs was at a session in 1942 of which Dr. Oppenheimer was in charge in which the ideas of thermonuclear reaction were discussed. When I was in Los Alamos in 1945, the idea of thermonuclear explosions was then described to me in the

general nature of the kind of reaction one might have. At various times we received reports from Los Alamos in the General Advisory Committee meetings as to the progress on research on thermonuclear reactions. It was my impression that this research was going forward, that there were some very difficult technical obstacles, but that the research and development was moving forward. It was not my intention at least in making this recommendation and signing it that this research and development effort should in any way be slowed down, but should be continued --

MR. GRAY: At the same pace?

THE WITNESS: At the same pace, and if possible, expanded if additional people could be found. We did not at any time recommend stopping the effort at Los Alamos.

MR. GRAY: That is clear to me that you didn't stop it.

THE WITNESS: Or slowing it down.

MR. GRAY: Or slowing it up. I am wondering whether it was a matter of discussion in the GAC as to whether something more might be done in research and development short of production than was being done.

THE WITNESS: Again, it is a little difficult to project the opinions back to that time, but as I recollect my own views on it they were that the thermonuclear program was proceeding satisfactorily, that it was a difficult decision of

priority as to whether additional effort -- that means men -- should be transferred into the thermonuclear program as compared to the fission program, which was also proceeding beautifully, and was resulting in substantial improvements in our stockpile position on fission weapons.

There was a delicate balance there as to whether more good people -- it took very good people at that time to make any good contribution to the thermonuclear program -- should be asked to transfer from the fission to the thermonuclear program. I think it should also be made clear that these two programs are by no means independent; that a thermonuclear explosion is quite impossible without an extremely effective and large fission explosion initiating it. The thermonuclear and fission programs were very closely related, and going forward hand in hand as they must necessarily do.

In our opinion it was not a matter of real conflict but there was a matter of balance. We felt that very important fission programs were under way that should not be slowed down.

BY MR. GRAY:

Q And they might have been slowed down by more emphasis on research and development with respect to the other weapons?

A They could have been.

Q I want to discuss a little bit with you, if I may, your views with respect to loyalty. This follows some direct questions put to you

It is my recollection that you stated at one time in the day that you felt that former associations were irrelevant. If that is not a fair summary, I wish you would correct me. In any event, you felt that in this particular situation they are not relevant.

A I was confining my remarks to the particular associations mentioned in the allegation in this case and to the individual in this case.

Q Dr. DuBridge, Cal Tech has a lot of government sponsored research.

A Yes.

Q Is some of it classified?

A There are two parts to our research, if I may explain. One large project which is operating off the campus about five miles at the government owned installation. Cal Tech operates it. That is a classified project on rockets.

On the campus where our students are, we have essentially no classified work in progress. We avoid it on the campus. There are one or two pieces of equipment, wind tunnels, to which classified models are occasionally brought for test and so for a while a classification screen has to be set up around. But by and large, we do not have classified research going on the campus.

Q At the off campus center, which does have classified work, you must have certain employment policies with respect



to people there. I assume you don't knowingly employ a person who is currently a member of the Communist Party?

A Obviously not.

Q That would be pretty clear, I think. Are the prospective employees or personnel on that project asked if they have ever been members of the Communist Party?

A I am not sure I can answer that. I don't know what questions the personnel officer asks. No one is employed on that project, however, until we have received from the Army a clearance saying that this man is cleared for confidential work. This is a project under the sponsorship largely of the Army Ordnance Corps. There is a local ordnance office in Pasadena. All prospective employees are referred to them for screening and clearance. I am sure that they would not clear anybody who was a member of the Communist Party.

Q Currently.

A Yes.

Q Would they clear anybody who had been a member of the Communist Party?

A We had one case a few years ago where they did clear a person who had been a member of the Communist Party. When they found it out, however, they withdraw his clearance.

Q Would you make a distinction between the type of clearance needed for someone who is going to join the faculty on the campus where there is not classified information and

someone who would join the other project where there is classified?

A Yes.

Q You would apply a more rigid test on the off campus center?

A Yes. Further, on the off campus center, we say as a university we are not competent to judge the security risk of prospective employees. We therefore refer these questions to the Army.

Q So, as president you don't take responsibility securitywise for the people employed on that project?

A That is right. We naturally are careful in our employment policies to not get prospective employees referred to the Army that are obvious security risks even to us. We would not employ anyone until we were sure first he was an honest man, second he was an able scientist or engineer, and third, that his former employees and associates felt that he was a good man to work in such a group. We would give this kind of general screening of ability and integrity first. But we would not attempt an FBI investigation.

Q I understand. You get applications for employment at that center, and if you think the individual is a good prospect for employment, you ask the Army to clear him.

A That is right.

Q If you knew that a man was a member of the Communist

Party, would you even send his name over?

A I would not consider it at all.

Q If you knew he had been a member of the Communist Party, would you send his name over?

A If he was an applicant for a job at the classified research laboratory, that is a little difficult, because it would depend a little on the circumstances as to what the man had done in the meantime. Whether he had told us honestly he had been a member and had resigned, or whether he had hidden it and we had found it out in some other way.

Q In the latter case, there would not be much question?

A Yes.

Q But you are not sure about in a case --

A If a man came to us and said, "I was a member of the Communist Party 20 years ago, I resigned for the following reasons", we would probably say, "Well, everything else being acceptable, we will not put you at work, but we will put your name in for clearance, and we will see what the Army thinks of your connection."

Q In testifying about associations earlier today, you indicated an understanding that in a particular case the associations ceased. I believe at least that was true.

Let me say that this Board has reached no conclusion, and I want to make clear that I am trying to establish your philosophy, and not to ask you to pass judgment on any set of

facts.

Suppose some of these associations continued, would that change the answers you gave?

A If they had continued in an active way, and if the associations, the individuals involved had continued themselves an active association with the Communist Party, I would think this was a proper matter to be further investigated.

Q So in that case associations would be very relevant?

A That is correct. If they were continuing, and if the individuals involved were continuing their association with the Party.

Q I have just one final question which relates to your discussion of the atmosphere and times in the late Thirties and early Forties when people were concerned with what was happening in Germany and Spain. You indicated that at least part of this deep concern was a reaction to dictatorship and therefore some people turned to the Communist Party in reaction to revulsion against dictatorship. Wasn't it pretty well understood in this country at that time that the Soviet Union was a dictatorship?

A It is a rather curious situation that the most active verbally opposition to Hitler at that time came from members of the Communist Party. It is now obvious to all of us that this was a piece of hypocrisy, since their own regime was a dictatorship all the time. I think, however, in the early

1930's it was not so clear as it now that the Communist Party in the United States was really a part of the Soviet Government apparatus, nor was it so clear that the type of dictatorship was the same. I think those who thought that were wrong and mistaken, but it was nevertheless true. Wasn't it half a million people voted for the Communist candidate for President in the Thirties, apparently under the illusion that the Communist Party had a solution to the depression problems, or something and we were not aware of the nature of the world conspiracy which was developing at that time. But it is certainly true that I believe many people joined the Communist Party, or became associated with those who were members because the members did express an active opposition to Hitlerism, to Nazism, to Fascism generally and a support of the Spanish Loyalists.

I don't pose as an expert. You asked me a question. I think you will not find that we ever had a time in the political history of this country where a half million people voted for the Communist Party candidate. I believe that you would find that in the depression years, to use the words of the Democratic candidate last year, almost a million people voted against capitalism. Again just to make sure I don't accept that statement of the situation, the vast majority of those were votes for Norman Thomas, the Socialist candidate, and I am guessing -- I don't know whether I am sworn here -- I am guessing that very considerably less than half a million

every voted for the Communist Party. I think we are engaged in an excursion.

A Yes, I think so. I hope my figures there will not be taken seriously. But there was a substantial vote for the Communist Party.

Yes, certainly more than would be true today, I think.

A Yes.

MR. GRAY: Dr. Evans, do you have any questions.

DR. EVANS: Dr. DuBridge, let us go back again to that Chevalier incident. You remember about it. I want to ask you this question. Was it Dr. Oppenheimer's job to decide whether the security of his country was involved, rather than to report the incident?

THE WITNESS: Would you repeat that?

DR. EVANS: Yes. Was it Dr. Oppenheimer's job to decide for himself whether the security of the country was involved rather than report th incident immediately?

THE WITNESS: I think possibly Dr. Oppenheimer was mistaken in his judgment at that time. I am sure it is a mistake he will not repeat.

DR. EVANS: You would not have done it the way Dr. Oppenheimer did?

THE WITNESS: Knowing what I do now, today, I would not. What I would have done in 1940, I cannot say.

DR. EVANS: That is all.

MR. GRAY: Mr. Garrison.

REDIRECT EXAMINATION

BY MR. GARRISON:

Q I have just one question to clear up what may or may not be a misunderstanding.

When you were being asked about the luncheon, I think at Mr. Burden's in Washington, and the discussion with Mr. Griggs, and so on, I think the question was put to you whether you said anything at that luncheon to the effect that you regarded the development of continental defense and of atomic weapons, fission weapons, as more important at that time than the H bomb. I wanted to ask you whether you meant to convey to the Board -- if you did, you should say so -- that you had in mind at that time or indeed at any time that there should be any lessening of the effort to produce the H bomb, or any lessening of cooperation with the letter and spirit of President Truman's "go-ahead".

A It was not my understanding then or now that President Truman's decision meant that no other military program should go forward other than the H bomb program, or that even that the H bomb program would have overriding priority over all others. It seems to me then that of more immediate concern to the strength of the country was the continued development of our fission stockpile and the methods for delivering it, plus the continued development of a method of defending this

country against a fission bomb attack which then was as now certainly possible on the part of the Russians. It was not our thought that giving attention and effort to the fission program or especially to the continental defense program need in any way detract from the essential part of the effort on the H bomb program.

I think what we were trying to get across at that time there were many people, it seemed to us, who were of the opinion that the only thing that could save this country was to get an H bomb right now, and that all other things would sink into insignificance by comparison. I felt that was not a fair evaluation of this country's military situation. That it was important that the fission program go ahead and the continental defense go ahead. The continental defense is now going ahead on a large scale, and it is recognized that it is an important enterprise, and indeed its importance has increased by virtue of the H bomb effort on the part of the enemy.

In other words, we were trying to get a proper balance in the military program of the United States, and arguing for a proper balance.

Q You said H bomb development on the part of the enemy. You don't know personally that they are working on the H bomb now?

A I meant the H bomb because it is my understanding



that the Atomic Energy Commission has detected evidence of a thermonuclear explosion in Russia.

DR. EVANS: Thank you.

BY MR. GARRISON:

Q Is it unclassified to say when?

MR. BECKERLEY: It was announced.

THE WITNESS: It was announced.

MR. GARRISON: When was it announced?

MR. BECKERLEY: August 1953.

THE WITNESS: That is, of course, the time this was being discussed. What I was referring to was also after. I was saying that the continental defense now that is going ahead was even more important because of the thermonuclear explosion by Russia in 1953.

BY MR. GARRISON:

Q I think when I was asking you about your opinions regarding Dr. Oppenheimer's loyalty, when I put to you a very long question about the Chevalier incident, I also asked you to assume that all the derogatory information in the December 23 letter of the Commission was true, leaving aside the items about the H bomb, and you answered the question leaving aside the items about the H.bomb.

I just wanted to make sure -- and I think it is probably sure by now, but perhaps not -- that with respect to the items of information about the H bomb in the Commission's

letter, do you have any opinion with regard to those particular items?

A Yes. In the first place, I think --

Q Let me refer to it a little more explicitly. What I have reference to are the suggestions that Dr. Oppenheimer --

A May I refer to a copy of that letter?

MR. ROBB: Surely.

BY MR. GARRISON:

Q He caused to be distributed and so forth, copies of the report, that he discouraged people from working on the project, and that he delayed the production of the work on the bomb. I am paraphrasing it. You have the exact language there.

A In the first part of this paragraph, which is on page 6 of the original letter, the paragraph starting, "It was reported that in 1945, you expressed the view" and so on, certain statements are made about Dr. Oppenheimer's opinion on the feasibility and desirability of an H bomb program.

Q What I have reference to are the reports at the top of page 7.

A I would like to make a report about the first part.

First, it seems to me that those statements about his opinions, even in so far as they are true, could perfectly possibly and indeed I believe were the opinions of a perfectly loyal American seeking to increase and not decrease the military establishment of his country.

"Further reported that even after it was determined as a matter of national policy to proceed with the development of a hydrogen bomb, you continued to oppose the project and not cooperate fully in the project."

To the best of my knowledge that statement was false. "It was reported that you departed from your proper role in the distribution of the reports of the General Advisory Committee for the purpose of trying to turn such top personnel against the development of the hydrogen bomb." To the best of my knowledge that is false.

I think it is quite probable that copies of GAC reports did reach the top people of Los Alamos as all our reports did by normal channels, but that the Chairman of the Committee departed from his proper role or did this with the purpose of trying to turn personnel against the hydrogen bomb is in my opinion false.

"It was further reported that you were instrumental in persuading other outstanding scientists not to work on the hydrogen project, and your opposition to the hydrogen bomb of which you are the most experienced and most powerful has definitely slowed down its development", that is also false. Quite the contrary, I believe Dr. Oppenheimer's efforts and the efforts of the GAC were intended solely to improve the position of this country, with no other objective, purpose or result.

MR. GARRISON: That is all.

RE CROSS EXAMINATION

BY MR. ROBB:

Q Just to have the record clear, what you have done is to give your opinions without knowing definitely the facts?

A I said to the best of my knowledge in each case.

MR. ROBB: Thank you.

MR. GRAY: Thank you very much, Dr. DuBridge.

(Witness excused.)

MR. GRAY: We are in recess until 9:30 tomorrow morning.

(Thereupon at 6:10 p.m., a recess was taken until Friday, April 23, 1954, at 9:30 a.m.)