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# UNITED STATES ATOMIC ENERGY COMMISSION

VOLUME II

In the Matter Of:

J. ROBERT OPPENHEIMER

HISTORIAN'S OFFICE  
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UNITED STATES ATOMIC ENERGY COMMISSION  
PERSONNEL SECURITY BOARD

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

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,  
DR. WARD V. 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  
ALLEN B. ECKER, Counsel for J. Robert Oppenheimer.

P R O C E E D I N G S

MR. GRAY: I would like to call the proceeding to order.

The Chairman of the Board has a few observations to make, and I have a few questions to ask on behalf of the Board.

I should like to read again for the record a statement which I made yesterday, that the proceedings and stenographic record of this Board are regarded as strictly confidential between Atomic Energy Commission officials participating in this matter, and Dr. Oppenheimer, his representatives and witnesses. The Atomic Energy Commission will not take the initiative in public release of any information relating to proceedings before this Board.

The Board views with very deep concern stories in the press which have been brought to the attention of members of the Board. I personally have not had time to read the New York Times article, but I am told that both the Nichols letter to Dr. Oppenheimer, of December 23, and his reply of March 4, are reprinted in full. Without having any information whatsoever, I have to assume that this was given to the New York Times.

DR. OPPENHEIMER: It says so in the paper.

MR. GRAY: I do not suggest that represents a violation of security. I have a serious question about the

spirit in keeping with the statement we made for the record yesterday about these proceedings being a matter of confidential relationship between the Commission and the Board representing the Commission, and Dr. Oppenheimer and his representatives and witnesses.

We were told yesterday before this hearing began that you were doing all you could to keep this out of the press. You said you were late yesterday because you had fingers in the dike, I believe was your expression, which I found somewhat confusing against subsequent events in the day when you say that you gave everything that you had to the press. We agreed yesterday that it would be very unfortunate to have this proceeding conducted in the press. There was no dissent from that view which was expressed, I believe, by all of us.

I think that it should be perfectly apparent, particularly to the attorneys involved, that this Board faces real difficulties if each day matters about this proceeding appear, not on the basis of rumors or gossip, but on the basis of information handed directly to the press. I think it only fair to say for the record that the Board is very much concerned.

I should like to ask some questions for the record about the authorized spokesman for Dr. Oppenheimer. I assume in addition to Dr. Oppenheimer that Mr. Garrison, Mr.

Silverman and Mr. Ecker are actively and officially associated in this proceeding.

I should like to ask who else is working on this who may be talking to the press?

MR. GARRISON: Mr. Chairman, perhaps you could let me answer that question by a little history. The letter from the Commission was given on December 23. I came into the case early in January. Almost immediately, or perhaps the middle of January, it became quite apparent from inquiries that Mr. Reston addressed both to the Atomic Energy Commission and to Dr. Oppenheimer, that he already had information that clearance had been suspended, and that proceedings were going forward against Dr. Oppenheimer. He was most anxious to obtain background information from us.

We explained to him the nature of the proceedings and our earnest desire that this not be the subject --

DR. OPPENHEIMER: May I correct that. Was this your conversation with Reston, because I believe the initial conversations were with me. He called and he was very persistent in calling. I tried to evade it. I knew what it would be about. After about five or six days of persistent telephoning, he talked to my wife, and said that he had this story and he wished I would talk to him.

I talked to him on the phone. I said I thought it contrary to the national interest that the story should

be published, that I did not propose to discuss it with him, but if the time came when it was a public story, I would be glad to discuss it with him.

That was mid-January. I don't remember the date. I am depending on counsel's memory. I believe that was the substance of our talk. He told me two things. First, that my clearance had been revoked. That was the story he had heard. That this had been cabled, telegraphed and broadcast to Submarine Commanders throughout the Fleet and Army posts throughout the world, and second, that Senator McCarthy was fully aware of this and thought I ought to know that. That was the end of that discussion.

I was given to understand by proffers of kindness but not other sign that the Alsops knew the situation. Later this was confirmed by one of the prospective witnesses.

MR. GRAY: You did not talk with either one of the Alsops?

DR. OPPENHEIMER: I have not talked to either one of the Alsops until very recently, and I will describe those conversations. This was long ago, and it was my affair, and I thought my memory would be more vivid than yours.

MR. GARRISON: Why don't you tell of your conversation with the Alsops?

DR. OPPENHEIMER: That is not until very recently. Stewart Alsop called co-counsel, that is Herbert Marks,

whose name should be in these proceedings -- when would that have been, Saturday, Friday -- quite recently, saying that they had the story and were frantic to publish, and that I should call Joe Alsop, who is up in Connecticut at a rest home.

MR. GARRISON: In Garrison, New York.

DR. OPPENHEIMER: I did call him there. I put on my spiel, the thing that I have said to everyone, that I thought this story coming out before the matter was resolved could do the country no good. Either I was a traitor and very, very important secrets had been in jeopardy over the last 12 years, or the government was acting in a most peculiar way to take proceedings against me at this moment. This is the impression that I feared would be made. Neither impression could be good. Having both of them could be only doubly bad.

Therefore, not as far as I was concerned, but as far as what I thought was right, I urged Joe Alsop to hold his story, not to publish it. We did not discuss any substantive things except that Alsop told me how apprehensive he was that Senator McCarthy would come out with it. I believe that was all I said to Joe Also. He said he thought I was making a great mistake, but I said it was my mistake.

I recognized of course that he could publish any moment that he wanted to.

MR. GRAY: May I ask, as of this time or ten o'clock yesterday morning, had you given the New York Times these documents?

MR. OPPENHEIMER: These documents were given to Reston by my counsel Friday night, I believe, without any instruction as to what he was to do with them, as background material.

MR. GRAY: So that you knew when you made the statement here yesterday morning that you were keeping the finger in the dike that these documents, dated December 23 and March 4, were already in the possession of the New York Times.

DR. OPPENHEIMER: Indeed we did.

MR. GARRISON: Mr. Chairman, they were given to Mr. Reston with instructions not to be used unless it became essential for the Times to release the story because others were going to do likewise. We hoped even as of yesterday -- the last word we had with Mr. Reston was after lunch -- we hoped even as of yesterday that this could be held off, although I told you at the start that it might be only a matter of hours.

MR. GRAY: You didn't indicate to me in any way -- if you attempted to do so, it is a matter of my misinterpretation -- that you had given documents which relate to these confidential proceedings and are part of these proceedings.

You mentioned Mr. Marks. Who else is authorized to speak for you, Dr. Oppenheimer?

MR. GARRISON: No one else. Mr. Marks is not counsel of record in this proceeding. He has been associated with us from the start because of his knowledge of past history. I am still seeking his guidance and help.

MR. GRAY: He is assisting, I take it, in preparing these documents which you present?

MR. GARRISON: No, we did all that work ourselves.

MR. GRAY: May I ask specifically for the record who prepared the excerpts about which I asked the question yesterday?

MR. GARRISON: We did in our own office. I did. Mr. Ecker worked on them.

MR. GRAY: I should like to know, Mr. Garrison, why it was yesterday that not one of the three of you could answer the question as to whether these paragraphs were consecutive or came from consecutive pages. It is apparent that someone else had prepared them.

MR. GARRISON: No, Mr. Chairman.

MR. GRAY: I have drawn a conclusion. If I am wrong --

MR. GARRISON: I am sorry that such thoughts should even occur to you. What happened was that some weeks ago I went through Dr. Oppenheimer's writings and I marked

particular sections and passages from a lot of them that seemed to me to be worthy of presentation to the Board, and I asked that they be extracted and copied out. I have not been over them for some time. To be frank with you, I have had so much else to do.

MR. GRAY: My point in raising all this is that if there are a good number of people who are not appearing here who are going to be talking to the press, I would like to know what control or lack of control there may be in this situation. That is why I am raising this thing.

MR. GARRISON: Yes.

MR. GRAY: I think these stories are very prejudicial to the spirit of inquiry that I tried to establish as an atmosphere for this hearing as we started yesterday. I would very much regret that what would appear to be to the Board possible lack of cooperation in conducting these proceedings in the press if that were prejudicial to what are the basic fundamental issues involved.

MR. ROBB: Might I ask a question, Mr. Chairman?

MR. GRAY: Yes.

MR. ROBB: I don't think we have identified Mr.

Marks.

MR. GARRISON: Mr. Herbert S. Marks, former general counsel of the Atomic Energy Commission, and a lawyer in Washington.

MR. GRAY: He is an attorney and member of the District of Columbia Bar?

MR. GARRISON: Yes.

MR. GRAY: And do I understand he is of counsel to Dr. Oppenheimer?

MR. GARRISON: He is associated with us as counsel.

MR. GRAY: In the relationship of lawyer and client, is that correct?

MR. GARRISON: Yes.

Mr. Chairman, may I just say another thing about the problem that we faced. Mr. Reston from the middle of January has had the Alsops, and I don't know who else busy gathering information from anybody they could find and had developed so much of the story when Mr. Reston talked with us on Friday that it seemed to us that if the story had to break that rather than half a story or two thirds of it or a quarter of it in fragments with constant demands afterwards from the press for the rest of it, that it was better that the basic documents be there for all to see.

This was not a happy decision or a pleasant one for Dr. Oppenheimer, believe me, to have the letter of charges displayed for the American public. It was something no man would ever wish to do. It was not until Mr. Reston told us yesterday afternoon that the thing absolutely could not hold, the stories were going to be published, Alsop said

the same thing, that we said all right, go ahead then and print the documents.

Now, it is not our purpose to make any press comments upon this case. It is not our purpose to release any transcripts. If you will observe the Reston story, I am sure you will see that we have tried to avoid any kind of special pleading. Dr. Oppenheimer has made no statement. We are not trying to try this case ourselves in the press. I assure you with all earnestness that is true. I feel absolutely certain that it is better in the long run for the government, for this Board, and for us, that there be no suspicion about what is the scope of this case, whether the H-bomb is in it, and all those kinds of questions that would arise if the actual facts had not been disclosed.

MR. SILVERMAN: May I point out, if I may interrupt, there was an item in the Reston story, however, it is understood that he, Dr. Oppenheimer, also put in evidence another secret document in the form of a memorandum. We haven't the faintest idea what they are talking about, nor did we give them any such information.

MR. GRAY: Who is "we". Who actually handed the documents to Mr. Reston?

MR. GARRISON: I did myself, Mr. Chairman, personally.

MR. ROBB: Did he also get a copy of this

autobiography?

MR. GARRISON: No.

MR. ROBB: Mr. Garrison, may I ask another question? Didn't I understand you to say yesterday morning that explaining your tardiness at the hearing that you had been engaged in a press conference?

MR. GARRISON: No, I had been engaged in threshing this problem out among ourselves, because the calls were coming in and putting us under the greatest pressure. In fact, right along we have been under pressure to make statements, to initiate statements of our own and come forward with information. It has been a very, very difficult undertaking, Mr. Chairman.

MR. GRAY: I am quite aware of that. On the other hand, you are quite aware also that the members of this Board have been under pressure, and that we have I believe without fail said we will not discuss it. That will continue to be our position.

MR. GARRISON: I should also like to say that we did not disclose to anybody -- when I say "we", I mean every one of the counsel to my knowledge, and Dr. Oppenheimer -- the names of this Board or where the hearings were being held or anything else.

MR. EVANS: Where did they get it?

MR. GARRISON: I don't know. I have no idea.

DR. EVANS: They called me up about 1:30.

MR. GRAY: They called me, too, but I didn't answer the phone.

I would like to move to another point, if I may. I am sorry we are keeping Dr. Kelly waiting. This has to do with the schedule of hearings. You left a suggested type-written schedule with us yesterday which was not made a part of the record. I think I should say that the Board cannot accept this as a schedule. I repeat, indeed, if it is necessary to repeat, that this is to be a fair inquiry, that Dr. Oppenheimer will be given full and adequate opportunity to make any presentation he has, and to present such witnesses as he desires, but as far as the schedule is concerned, the Board feels that it is up to Dr. Oppenheimer and counsel to furnish the witnesses and information for the Board.

We propose to sit from nine, if it is desired by Dr. Oppenheimer and his counsel, or from 9:30 to 12:30 and from 2 until approximately 4:30, give and take a little because of circumstances. Frankly, I think the Board is unwilling to commit itself to a schedule which I am sure means that we will have some witnesses on a certain day who will be through and then there is nothing more for the Board to do or for a part of the day. I should like to suggest, Mr. Garrison, that we inform you again that we will meet

and we will hear the witnesses and some approach be made to this problem from the point of view of the convenience of this Board and not the convenience of the witnesses as would be true in most proceedings in the American tradition. If it seems to be necessary to hear a witness at a particular time in accordance with some prearranged schedule, some days in advance, I think you should be warned that the witness will probably be asked under oath whether this is the only time that he could appear, if we run into a situation where we must recess or delay proceedings because of a witness who has said, "I can come on a certain date."

We understand fully that Dr. Kelly can only be here this morning. We are very glad to hear him and we will hear him. Then I would very much prefer, and the members of the Board would, if we could receive the remainder of Dr. Oppenheimer's presentation, and proceed with whatever period it seems desirable of questioning Dr. Oppenheimer, and then try to move forward with receiving testimony from the witnesses.

So I don't think that we wish to commit ourselves to a schedule which draws it out precisely as this is drawn. I am hopeful you will find that we will be reasonable and fair in hearing the witnesses.

MR. GARRISON: Mr. Chairman, pursuant to your wishes that you expressed informally to us yesterday, I arranged for Dr. Bush to appear instead of this morning on Monday

afternoon, the 19th, and I have arranged with Mr. Gordon Dean to appear Monday morning the 19th, in lieu of Wednesday afternoon.

MR. GRAY: I would say, Mr. Garrison, that is quite all right with the Board. This is a part of your responsibility of keeping witnesses and whatever else is to be presented to the Board moving along as we sit and are available to hear them.

MR. GARRISON: I have no doubt that we shall fill the afternoon session on the 19th, so that there will be no waste time of the Board, because there are still several witnesses whom we have contemplated calling and we have not had a chance yet to talk with them.

MR. GRAY: All right, sir.

MR. GARRISON: For example, Mr. Conant, Mr. Bradbury, and several others. If you will indulge me, I would like to say one other word about counsel, because I think there has been some mystery, perhaps, created by Mr. Marks' relationship to the case. Mr. Marks is an old, very dear and very personal friend of Dr. Oppenheimer. They both came to see me when I was asked to serve as counsel. I am serving without fee in this case as a public service. To the best of my knowledge, Mr. Marks is serving without fee in this case as a gesture of very deep friendship and admiration for Dr. Oppenheimer. We have been working together, he and I,

as one would work together in a matter of this sort without any really formal relationship except that it was understood that I would in effect try the case, conduct the proceedings and have the final decision and responsibility. He is now simply going about his law practice, and as I feel that I use his advice and need him, Dr. Oppenheimer leans very heavily on his opinions, we meet together and talk things over. It is that kind of a relationship.

It never occurred to me that it would be necessary or that I would be not frank with the Board in not entering his appearance here today, because actually we are the counsel conducting this proceeding, and I have the final decision. But I want you to be quite sure that Mr. Marks is not authorized by me to talk with the press or to exercise himself in any fashion on this matter. He is a friend and advisor and associate in that sense.

DR. OPPENHEIMER: He is sometimes authorized to talk to the press in specific ways and with a specific message.

MR. GARRISON: Both he and I have had conversations with Mr. Reston and Mr. Alsop and other newspaper men have called him up, but what I am trying to say is that Mr. Marks is not sitting in his office at my request conducting press conferences to spread information about this case. You can be just as sure as that --

MR. GRAY: But he is authorized to speak to the

press, at least those were Dr. Oppenheimer's words.

MR. GARRISON: He is not authorized to conduct press conferences. He cannot avoid inquiries when they come to him. As far as I know, Mr. Chairman, we are all going to be battered -- I was called at quarter to seven this morning.

MR. GRAY: You cant avoid the call. But I can say to you on the basis of personal experience that it is possible not to talk.

MR. GARRISON: That is what all of us have pledged each other to do, that is, not to talk.

MR. GRAY: As of what time did you take that pledge?

MR. GARRISON: We decided when the documents were made public that ends this matter as far as we are concerned.

MR. GRAY: Fine. I am sorry we kept Dr. Kelly waiting. Would you get him in, if you are ready now to present Dr. Kelly.

Whereupon,

MERVIN J. KELLY

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

MR. GRAY: Mr. Kelly, do you wsh to testify under oath. You are not required to do so.

DR. KELLY: I would be glad to testify under oath.

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

Mervin J. Kelly, do you swear that the testimony you are to give to the Board shall be the truth, the whole truth, and nothing but the truth, so help you God?

DR. KELLY: I do.

DIRECT EXAMINATION

BY MR. GARRISON:

Q Dr. Kelly, you are the President of the Bell Telephone Laboratory in New York City?

A I am.

Q And in 1950 to 1951, you served on a Research and Development Board panel under Dr. Oppenheimer's chairmanship?

A That is correct.

Q You had met Dr. Oppenheimer before that time?

A Oh, yes.

Q Could you say when you first met him?

A It was at either a National Academy meeting -- what is this thing in Philadelphia we belong to -- the American Philosophical Society meeting in Philadelphia shortly after the war, late 1945, or early 1946. Oppie was addressing a meeting there at that time.

Q Would you tell the Board very briefly about your work with Dr. Oppenheimer on the Research and Development Board panel?

A The Research and Development Board has had an Atomic Energy Standing Committee. At that time Robert LeBaron,

Mr. William Webster was the head of the Research and Development Board. At Mr. Webster's request or suggestion Mr. LeBaron formed a panel in the late fall of 1949, as I remember. I had a letter from Mr. LeBaron in early November concerning serving on the panel, in which he told me that Dr. Oppenheimer was to be the Chairman. I accepted membership and then had relations with Dr. Oppenheimer from then on about it.

We had our first meeting early in December. The committee had nine members, three military, three of the more academic scientists and three of the less academic. General J. McCormick, who was then the military officer in the AEC, reporting to the General Manager, in charge of military programs, was ex officio and at all meetings.

The group was made up of Dr. Oppenheimer as Chairman, Dr. Bacher, then of Cal Tech. He had been on the Commission. Dr. Louis Alvarez of the University of California. Professor Charles Lauritsen of Cal Tech. Professor Walter Whitman of MIT, and myself were the civilians. The three military members were General K. D. Nichols of the Army, Admiral W. S. Parsons of the Navy, and General R. C. Wilson of the Air Force.

The general charge to the committee was for it to view the status of atomic research in the Commission and its progress, the state of the stockpile, with the knowledge of

the weaponry to come up with recommendations for the scope and emphasis in the military applications of the Research and Development Program.

MR. GRAY: Dr. Kelly, may I interrupt for a moment. I am afraid I failed to tell you that in the event that it is necessary for you to discuss any restricted data, I would appreciate your letting me know that you propose to do so.

THE WITNESS: I don't propose to say anything here that in a closed hearing is not perfectly all right, whether the people are cleared or not.

MR. GRAY: All right, sir.

THE WITNESS: I was stating the scope of the examination as requested by Mr. LeBaron. I think I had completed by saying that we were going to look at what the military applications of the Research and Development Program should be in the light of advancing knowledge in the atomic area, and the stockpile and the military situation. We had about six days in December of meetings and went over this whole matter. It was the first time that I had seen Dr. Oppenheimer in action in an operating sense in a responsibility of this kind.

He was an unusually able chairman. I have been on lots of committees and chairman of some, and I would put him right at the top in his patience in developing views and getting the views of everyone, and promoting full discussion,

and yet giving the minimum of waste time for busy people that goes with committees of that size.

We came up, after much discussion, with very common views because it was in an area where, excepting for the enemy situation, there was generally a background of factual knowledge to work on.

After we had gotten to where we had a commonness of view as to what we should say, the program should be in scope and emphasis, Dr. Oppenheimer undertook the job of preparing our report, which was an aid to all of us. I remember his staying on in Washington between meetings and beyond meetings for drafting the report. He drafted a report which with very minor modifications, I would say, all of us could sign as representing fully our own views as to what the military emphasis in research and development should be.

This was just at the threshold of the time where atomic basic knowledge had reached the point that it was possible to consider versatility. By that I mean extending the range of weapons well beyond that of the large free falling bombs. So this was rather a critical time.

That opportunity for extending the scope of weapons, that is, the range of versatility in military action was a thing that needed very careful weighing and was weighed and our report encompassed the views on how that should be broadened. As a matter of fact, I know from my participation

in the program that what happened in the succeeding years was very much along the line or substantially identical to the charter that we suggested as the Research and Development Programming Plan.

Mr. LeBaron wrote me, and no doubt other members of the committee afterwards, expressing appreciation and stating the way that it had been accepted favorably in both the Commission and the military. Throughout this, Dr. Oppenheimer was one of us in views, that is, had common views with us, as to the best military use of the fissionable materials and the kind of weapons that should be put into development, and in discussion there was every evidence of his dedication to the best use of this kind of power in the national interest possible. Any divergence in views as they developed were detailed and no greater difference in his views on that from one of us to the other than there would be between any two of us.

BY MR. GARRISON:

C Did you ever deduce that Dr. Oppenheimer ever overstated, in your opinion, the need of continental defense as distinguished from the production of offensive weapons and plans?

A Quite the contrary. Dr. Oppenheimer's views on continental defense are so close to those that I have held from my close contact with it that I could not distinguish a

difference.

In the late fall of 1952, Secretary Lovett asked me to head a civilian committee made up principally of top business leaders, such as Bob Wilson of Standard Oil, and top educational people, to survey the continental defense problem and to put it in proper perspective with the rest of our deterrent efforts. General McCormick, who had then come over into the Air Force, I succeeded in getting as a secretary to my committee.

During the progress of the committee's work which was in the first several months of 1953 -- the committee was then operating under Secretary Wilson, but Mr. Lovett had cleared with him when he appointed us in November that he wanted us to continue because it was going into the new administration of Mr. Wilson -- and a number of times General McCormick for me, as I had a lot of other responsibilities, saw Dr. Oppenheimer. I know particularly of two visits. I remember two visits to Princeton where he discussed with Dr. Oppenheimer the evolving report and views. Of course, this could be said to be hearsay, but he recounted to me Dr. Oppenheimer's comments which were wholly favorable and differed only in insignificant detail. Dr. Oppenheimer felt it was a constructive judgment, which was in general, that while the country had not given proper emphasis to continental defense relatively, yet that our chief deterrent was strike,

and that nothing should be done in bringing up to a proper level a continental defense effort that would weaken our strike. That was the general philosophy.

We recommended certain organizational and planning and procedural things to unify the program, but placed it second to strike in the general program of our best defense, and best deterrent aspect.

With the discussions that General McCormick had with him I could distinguish no difference. In fact, he spoke very complimentary, so General McCormick related to me, of the direction our thinking was taking.

I do not find the time to do a lot of talking about these things that are directly concerned, but in the Lincoln Summer Study, two of my members were on that study, and I know from them that the views of Dr. Oppenheimer, who was there occasionally and others of the academic side, were very strong for looking into the Arctic line and the kind of implementation that was then in <sup>breadboard</sup> ~~Brass Board~~ state, but in proper perspective.

I have since hear Dr. Oppenheimer discuss the defense aspect at closed meetings in the Council of Foreign Affairs -- and this is in relatively recent months -- and found his views there in general accord with the ones I have held and pushed for a stronger continental defense, better organized, unified, but done not at the expense of

our strike power.

Q What would you say as to Dr. Oppenheimer's reputation for straightforwardness, directness, veracity?

A Among his peers, he is, first, known and recognized for his accuracy of thought and cleanness of expression. His words are considered generally well weighed and meaningful because of their accuracy and temperate. I would know of no one that knew him as well as I that would feel that he overstated his position.

As to his veracity and dedication, I know of no one in the program, with the high clearances that he has had, and that I have, Q and top secret, everything he has done and said gives a full appearance to a great dedication, as full an appearance as any of us that are in and still cleared.

Q Would you say that as Chairman of this panel he made a contribution to the national welfare?

A I am sure that he did. In the form that he writes all of his things, getting the views of the full committee that he shared, as to what the forward looking program should be, getting it clean, orderly and well placed was a great contribution, as anyone working in the atmosphere of the Pentagon knows the great need for, that is, of getting direction and aim and purpose well spelled out. It was in this report of the panel which was his fine, clean writing, but which was the views of all of us which he shared.

Q What have you to say as to his reputation for integrity and patriotism and your own personal feeling about that?

A Among his peers, those who know him and know his work, I would say his reputation is the highest. As to my own personal belief, I know of no one in the program that I would have any more confidence in their integrity and dedication than I would of Dr. Oppenheimer.

Q What would you say as to the competence of the setup at Los Alamos and Sandia to handle the whole program during the years while Dr. Oppenheimer served on the General Advisory Committee, roughly 1947 to 1952.

A I have known the situation there intimately since January 1949. That was my first entrance broadly into the atomic weapon area. During the war we had quite a good sized job at the laboratory in an area that did not concern Los Alamos directly, or Dr. Oppenheimer, and that was the research and early development of the membrane used at Oak Ridge for diffusion, a very difficult physical chemical job. In early 1949, the Commission asked me to make a study of the Los Alamos-Sandia combined operation and make recommendations as to any organizational changes. They had in mind not a complete satisfaction of the applied end of the weaponry, that is, after the nuclear job was completely done, the clothing of that with all the aerodynamic, electronic

and <sup>radar</sup> ~~sonar~~ gear to make the completed weapon. That, as well as the nuclear, had been up at Los Alamos up until maybe a year or two, I was in in 1949, and then that part of it that had to do with the weaponry, exclusive of the explosive unit, was moved to Sandia to be close to the military people. But both operations were under Dr. Bradbury, and that was a contract with the University of California.

There was some question within the Commission, and Dr. Bradbury himself, as to the operations in Sandia. So I spent the greater part of three months looking searchingly at Los Alamos and at Sandia, and reported orally -- I made the stipulation to the Commission that I must do it orally, as I could not take the time for a polished, finished report -- giving my judgment of the very high competence of the Los Alamos operation, and the quality of the people in the program, the way they were attacking them, and while the buildings were temporary in the facilities for doing it.

The applications end of clothing the unit that has the explosive with the required aerodynamic and electronics, I found was not up to the capacities of the country in that kind of applied science and technology. So I recommended that part of the job be given to an industrial contractor, as there were components of engineering judgment and background at high levels that just were not in the program, and also knowing how to recruit the kind of people to build

such a staff.

That recommendation was acted upon and Mr. Truman requested the A. T. and T. that we accept that Sandia operation, and a subsidiary corporation of the Bell System has been formed to do that.

The technical, the whole research and engineering side of it is my direct responsibility. I spend one week in five -- in fact, I am going out theretomorrow -- so I have known the program intimately since 1949. I would say that the overall integrated program is the finest expression of American scientific and technical ability, and that we are where we are in the weapons program because of that plan for doing it, its competence and its relative freedom to operate as scientists and technologists do in our society, relieved from a lot of restrictions that come in from Civil Service, and other kinds of handling.

As I say, the only blemish on that program in 1949 was the inadequacy of the applied technology having to do with the aerodynamics, electronics and so on.

Q Based on your knowledge of Dr. Oppenheimer, your experiences with him, and his reputation as you know it, do you believe that his clearance would be clearly consistent with the interest of national security?

A To the very best of my knowledge, I sincerely believe that, and I think that his absence from the programs

and from the councils would be a distinct loss.

There is one observation, as I told you, that I would like to make, if this is an appropriate time, that I think is pertinent to the aspects of the problem that I can't testify directly on.

When scientists and applied scientists look into the crystal ball in the early stages when there is not enough known about the facts of nature, you can find quite wide and honest diversity of views which clear up and views become substantially common when enough knowledge of nature's laws and behaviorisms in the area come to light.

Taking an example, I was thinking last night from my earliest entrance into science at the graduate level in 1914 and 1918, I was Milliken's research assistant in Chicago. As I did, I did a great deal of the oil drop experimentation that he was doing, first to establish that there was an electron with a unique charge, and only one electron. During the early years of that there was quite a school of thought that there was not, that there were electrons of various sizes. I remember a distinguished professor at Vienna whose name has slipped my mind, that published greatly on the sub-electron. By 1917, there was enough accumulation of the facts that agreed there was only one electron, which is our primer today.

In this atomic area, as you know the Atomic Energy

Commission has not been blind through the years to the civilian application for power and of course have been looking at power applications for military with more vigor in the earlier stages of it than they were at the direct civilian economy applications. But until the last year or so there were competent applied scientists who knew all of the facts that had evolved certainly up to a year and a half ago, and some of those that were right in the middle of it were of the views that the civilian applications, while certainly important to humanity, had a distant date because of economic considerations that you measure in decades

One of the ones who was right in the program and so had all of the knowledge from that side that I frequently talked with about it in the last year and a half has changed his views completely, and says that he has and he now feels confident that economic power will be with us in a decade. Yet until there was more information that came from his programs, showing what economic factors could be, he was of the belief that it was a few decades at least away.

DR. EVANS: You say you did work with Bob Milliken?

THE WITNESS: Yes, I did all my graduate work with Milliken from 1914 to 1918, and then came to the Bell System, and have been there ever since.

MR. GARRISON: That is all of Dr. Kelly unless the Board would like to ask questions.

## CROSS EXAMINATION

BY MR. ROBB:

Q Dr. Kelly, may I ask what is your field?

A I got my doctorate with a major in physics and minor in mathematics, and came to the Western Electric Laboratories in New York, and which later became Bell Laboratories in 1925 as a research physicist, and did my productive work as an applied scientist in the field of electronics. Since about 1936, I have been one with increasing scope of the technology that have looked at what others have done rather than doing it myself. So over the whole field of telecommunications and science and technology, I would say that I am expert.

Q Are you what is described as a nuclear physicist?

A No, I am not a nuclear physicist. I have kept very conversant with it as an interested scientist, but there was in my student days and my active days, there was nuclear physics, and as it evolved, I followed it closely. I have a number of nuclear physicists in my staff, among them Dr. Fisk, who was the first research director of the Atomic Energy Commission, but knows as a participant the nuclear fission field quite well. I have never practiced it, though.

Q You would not offer yourself as an authority on nuclear physics?

A No, just as one with an understanding of what

others have done, but not as an authority, because I have not practiced it. Because again I limit myself in the amount that I look at.

Q And by the same token, I assume you would not offer yourself as an authority on the Super bomb or the thermonuclear weapon?

A No, that is right.

Q Who are the leading authorities in the country on the thermonuclear weapon?

A I would say that the outstanding nuclear physicists that are in the program, such as Bradbury and his immediate staff, and Edwin Teller, and and Johnny von Neumann, would be names that would first come into my mind.

Q Dr. Lawrence?

A Yes. Again Dr. Lawrence is not a participant in the sense these men are, but has a great understanding and came up through nuclear.

Q I was not limiting myself to those who are not participating.

A He would be one of great standing and the head of the laboratory doing a great deal in that field.

Q Dr. Alvarez?

A Dr. Alvarez, who was on this committee, is another, yes.

Q Of course, Dr. Oppenheimer.

A Dr. Oppenheimer, Teller, Bradbury and von Neumann. Those are the first names that would come to my mind, but these that you add are in the same ball park.

Q Probably Dr. Oppenheimer would be preeminent, would he not?

A He would certainly be in the first four.

Q Whether he would be first or fourth, you would not want to say, but he would be in the first four.

A That is right. I would not be able to judge. I don't know that anyone could, because there are different qualities to it.

Q Dr. Kelly, in this report that you spoke of that your panel made in 1950, would that have been the report dated December 29, 1950?

A I would expect without referring to the notes that would be right. We finished our deliberations about the 22nd or 23rd, as I remember, and my letter from Mr. LeBarron is dated January 30, . He talks of the report having been received and studied. That is January 30, 1951. So certainly it was issued some time after December 22 and before January 30th.

MR. GRAY: What was the date you mentioned?

MR. ROBB: December 29, 1950.

BY MR. ROBB:

Q Do you have any way of establishing that?

A I could easily get it from the Department of Defense.

Q Perhaps I can be of assistance. In your discussions in that panel, Doctor, did you and your colleagues discuss the so-called Super weapon, the thermonuclear bomb?

A No, we did not. It was not in the area of our cognizance. It was a research thing where it had not even been proven that it would be, and it was not in a stage where military application could be considered. So there was no discussion in committee at all about it.

Q Would you say that again?

A It was not in a stage of development where as corresponded to the fission weapons you could be talking about military applications knowledgeably and the different ways that you would use it. All the discussions, the formal discussions of the committee--if there were any others, it was individual and separate from the meetings I attended -- was about fission and not fusion.

Q In other words, you felt that the fusion weapon was something in the future, is that correct?

A That is correct. We were working for the Department of Defense, and not the AEC, and it was not ready to be considered at that stage.

Q Did you make any comment in your report on the matter of thermonuclear warheads or fusion weapons?

A I have not seen the report since it was issued. I

would feel confident it was not there because it was not a matter of discussion. If it was, that is four years ago. I can't remember. It is three and a quarter years ago.

MR. ROBB: Mr. Chairman, I would like to read the witness something from the report, which is classified.

THE WITNESS: I have Q clearance; I can look at it.

MR. GRAY: In that event, those who are not cleared in this hearing room will necessarily be excused.

DR. OPPENHEIMER: Since this is a report I wrote, is this one I may listen to?

MR. ROBB: Absolutely, Doctor.

MR. GARRISON: Mr. Chairman, we hoped that this might not arise, but if it is the feeling of the Board that it is important to its own understanding of the case to put this kind of question, of course it is entirely acceptable to us, and we shall withdraw.

MR. GRAY: I believe that would be best, Mr. Garrison  
(Counsel for Dr. Oppenheimer withdrew.)

(Transcript pages 199 through 201, being classified, appear in a separate volume.)

MR. GRAY: Would you excuse me --

MR. ROBB: I think counsel can come back now.

MR. GRAY: That is what I was thinking. I don't want them excluded any more than necessary.

(Counsel for Dr. Oppenheimer returned to the hearing room.)

THE WITNESS: It appears there is a reference to the thermonuclear job as being more than just in the future and my comments, Mr. Garrison, were that is a complete blank in my memory, and I have not attempted to get a copy of that and read it before coming here. What I said was that the thermonuclear had not reached Sandia at all. While I knew the general situation and had not tried to follow it, so if it was discussed in the committee -- I first said I had no memory of it, and I still haven't -- but it must have been discussed, but I don't retain it. But at any rate, the thing it says there about the time of its development would have been a thing that I in signing it would have had to count on Dr. Oppenheimer, Dr. Alvarez and Dr. Bacher as the nuclear physicists who would know and whose judgment I would have respected. But I can't recount because I don't remember any of the discussions between the three.

BY MR. ROBB:

Q Dr. Kelly, were Dr. Alvarez and Dr. Bacher at that time, that is to say, 1950, close to the program of the

Atomic Energy Commission?

A Dr. Bacher had only recently resigned -- I think it must have been within the year -- from the Commission and gone out to Cal Tech. So he was pretty well up to date.

Q How about Alvarez?

A Alvarez was in the Radiation Laboratory and was very knowledgeable on nuclear phenomena generally, but what he would have known about this particular thing, having that knowledge, I would not know. He could well not be all current, but still capable of being so if he was given information. But Bacher certainly would have known, because he would have been a part of the deliberations. Alvarez may have known, but I don't remember what part he had in the program at the time, other than being at the Radiation Laboratory at Berkeley.

Q Doctor, would you search your memory, please, and, sir, tell us was there any discussion in your meetings at that time as to whether or not the Atomic Energy Commission had the capabilities, the personnel, and so forth, to develop the thermonuclear weapon?

A Any discussion of the thermonuclear problem is out of my mind. I have to say frankly that it was such a small part of the whole, and was so distant from the things that the committee itself could get hold of -- I mean that the military could get hold of in the time immediately ahead --

that it has not stuck with me as one of the more than minor things there. I just can't say.

Q In other words, Doctor, is it fair to say that the thermonuclear problem, if we can call it such, was not a major part of your discussions and was not considered at that time to be important? Is that correct?

A It was not considered at that time to be ready with enough knowledge about it to consider the emphasis in the military application area.

Q I see.

A It had not reached that state of development. I knew from visits from time to time up to Los Alamos and I had heard some discussions from Teller and others of the pros and cons about the development as people will discuss in that stage when there is insufficient data. Whatever discussion there was in this committee, I will have to say, not having refreshed my memory without reading it, I can't remember and would have said there was not discussion.

Q Was there any discussion that you can recall of a second laboratory?

A No, not in this committee at all.

Q Doctor, when did you say you first met Dr. Oppenheimer?

A It was at a meeting after the war in Philadelphia where he addressed either of those two societies that we belonged to. I can't remember which it was. It was very

close after the war, because it had to do with these atomic problems, as I remember.

Q I am not pressing for the exact date.

A I would guess 1945 or 1946. It might have been even early 1947. I cannot remember without refreshing my mind. Do you remember when you made that talk in Philadelphia?

DR. OPPENHEIMER: May I answer?

MR. GRAY: Yes.

DR. OPPENHEIMER: This was a joint meeting of the Philosophical Society and the Joint Academy of Sciences in mid-1945.

MR. ROBB: We will give you the award for memory.

DR. OPPENHEIMER: I made the speech.

THE WITNESS: He made the speech. That is the first time I met him. I knew him by name.

BY MR. ROBB:

Q How frequently have you seen him since?

A It would average four or five or six times a year. Since I am only testifying directly as to one occurrence, this is the one occurrence where I had business relations, common obligations with Oppie, but I would see him at scientific meetings or at Universities four to six times a year, I would say would be a proper average.

Q But the occasion about which you testified was your intensive experience with him.

A That is right. This was one where I saw him in detailed action and taking a leadership as a good chairman should take.

MR. ROBB: I think that is all I care to ask.

MR. GRAY: Dr. Kelly, I am sorry, I don't think I can ask this question, because it involves the quotation.

May I ask this question: If there appeared in a report which you signed material which was not reflected in the discussions, would you have raised the question at the time?

THE WITNESS: Yes, I would be very meticulous about signing a thing if I didn't have views of my own from my own knowledge to substantiate it. I would have asked afterward, or I would have had assurance from discussions that I do not now remember, that is, I would not have signed with that in there at the time I signed the report without a feeling that it reflected the judgments of experts in that area that I respected.

MR. GRAY: I understand that, and I think that is quite appropriate, as you have said earlier, that you would have relied upon the three members of this committee who were particularly qualified in certain areas. I am afraid I perhaps did not phrase my question adequately.

I have no question about the reliability or your sense of dependence and confidence in the individuals concerned. My question really is, is it possible that this

report could have reflected discussions which the committee did not actually engage in?

THE WITNESS: I can't imagine that, because again knowing myself, I am confident that as of the time I signed it, I would not have signed it with something in that I either had not heard discussed and felt satisfied with or raised questions about. But my mind is just blank on that, because it was such a minor thing of the things to get hold of with the military. You must remember in a thing like this you had the combinations of expertness. There were questions talked about in there about tossed bombing. *Lauritsen*  
~~Larsen~~ would know a lot about it. But Alvarez or Bacher would not know anything about it. So it was a combination of expertness in different areas adding up to the total. It just happens that my memory over the years has just dropped out completely whatever their discussions there were, even to the point of a comment as to the fusion weapon. In so far as the military could do or the programming could do at that time it is somewhat gratuitous because it just was not ready for the military to get hold of.

MR. GRAY: You felt as a committee member for one reason or another the military was not asking you to consider thermonuclear weapons.

THE WITNESS: That is right. In the scope of the things that the military themselves would be concerned with,

which really was the things at hand in the next year or so -- there had been a meeting two years before, or a study of this kind two years before -- it just was not in that ball park.

MR. GRAY: Were you engaged in the earlier study?

THE WITNESS: No, I was not in the earlier study. It was referred to. I don't remember what was in it but we had before us in the committee the study of the two years before. I remember having read it then, but I don't remember a thing that was in it now.

MR. GRAY: Thank you, sir.

DR. EVANS: Dr. Kelly, were you surprised how quickly they did develop the thermonuclear weapon after they started on it, or were you not?

THE WITNESS: Sir, I was very much surprised. As a peripheral person on that and hearing the discussions about it before there was data up at Los Alamos and -- they were not discussions like this was business, because I would not have been in them -- but these were discussions preceding cocktail parties on the Hill where Teller and others were engaged in speculations. The general views I had of the discussions there was that it was a long hard row.

MR. GARRISON: What year was this?

THE WITNESS: This was along in the 1950-51 time. I can't place it closer than that. I was up on the Hill --

MR. ROBB: May I interpose that you are in Washington. You are talking about the Hill. You mean on the Hill in Berkeley, California?

THE WITNESS: Down in Sandia we always speak of Los Alamos as on the Hill. I would go up to Los Alamos about every other or every third trip to Sandia. At one of those in the early days of the nuclear physicists considering the structure and the problems involved, I remember a lot of cryogenic questions, just hearing those as a peripheral person cleared to hear it -- the judgments I got and I well remember it was a thing we would not have to worry about for quite a while. "We" meaning the Sandia Corporation.

DR. EVANS: If you had to venture an opinion on it, your opinion would have been that it would have taken two or three years or longer than that?

THE WITNESS: That is right. Frankly I was and am greatly surprised at the tempo of advance and I believe that all in the program are somewhat surprised at some of the simplifications that are coming to light after you get hold of the things physically and can see them.

DR. EVANS: Would you put the Englishman, Chadwick, in that list of people that know about it?

THE WITNESS: Of course, Chadwick was out of the program. This is not the kind of thing that we can discuss with Englishmen after the Atomic Energy Act. I was not

directly in the program during the war. But Chadwick, John Cockcroft and among the names I would first mention in England of nuclear physicists who are very knowledgeable. But what they know about bombs, I don't know. While I see them at least once a year, we don't talk about bombs, because it is illegal.

MR. GRAY: Do you have any further questions?

MR. GARRISON: No.

MR. GRAY: Thank you very much, Mr. Kelly. We appreciate your being here.

MR. ROBB: Mr. Chairman, would it be in order for counsel to suggest a five minute recess.

MR. GRAY: Yes, we will now take a short recess.

(Brief recess.)

MR. GRAY: The proceeding will begin again.

Whereupon,

J. ROBERT OPPENHEIMER

resumed the stand as a witness, and having been previously sworn, was examined and testified further as follows:

DIRECT EXAMINATION (Continued)

BY MR. GARRISON:

Q Dr. Oppenheimer, would you care to make a comment about some of the matters touched on by Dr. Kelly in his testimony?

A If the Board would permit it, I would like very much to comment on it. This panel meeting about which Dr. Kelly has told you I referred to yesterday.

MR. GARRISON: Could I interrupt a minute, please?

The Board will find the reference to this panel on the second page of Roman II, Membership on Government Committees, No. 5(b).

THE WITNESS: It was next to the last item in my testimony yesterday just before I told about Vista. I told you the personnel and the critical atmosphere of the war. I would like to stick as much as I can to non-classified things.

I believe I told you yesterday two things about the period of this report. One was that it was the period after Chinese intervention in Korea when general war was very much in everybody's mind, not as a remote but as an

2 immediate thing.

The second was that it was a low point in the prospects on the Super. What you have heard read reflects that opinion. Dr. Kelly would certainly not have been more than a bystander in the formulation of this opinion. As he said, this was not his job. But the impression created in his testimony seems to me to need amplification.

Bacher was a member of the Atomic Energy Commission until sometime before. He was a continued consultant to Los Alamos and spent a good deal of time there.

General McCormick was the Director of the Division of Military Applications to the Commission, and was responsible for Los Alamos, received regular reports from the laboratory, talked with everyone involved that he wished to talk with and was well informed.

He is not a nuclear physicist, but he knew the views of nuclear physicists.

Lauritsen is a nuclear physicist. His whole life has been spent in nuclear physics except that part spent in atomic development. He was a consultant during the war and has been very close to the program of all forms of atomic development.

Alvarez is a nuclear physicist of distinction and was, I believe, one of the initial promoters of the crash program for the Super, and has always had a great interest

3 in the work.

General Parsons was a member of the evaluations group at that time. He had been at Los Alamos. His job was to keep in touch with current developments.

General Nichols -- his status at that time I have forgotten but I think he was in Research and Development in the Army.

All of these men had access to every document and report that existed and were knowledgeable not as to deep problems of contemporary physics, but as to the practical problems and evaluations which were current in the various places where work was going on or evaluation considered. Berkeley was one of them and Alvarez was there. I, therefore, think that there was a very substantial group of people, McCormick, Parsons, Bacher, Lauritsen, Alvarez and myself, who knew what was believed at that moment and who had a chance to evaluate it critically.

Any judgment that was expressed about the thermonuclear program could have been expressed only with the consensus, the complete agreement of all members of that committee who knew about it and the undertaking on the part of those who didn't.

One other thing. Walter Whitman was a member of the General Advisory Committee and had complete access to all reports and so on, and he was, I think, a member of the

4 committee.

The only thing I wish to protest is the suggestion that I was the only person competent to judge and that I sneaked a conclusion into the report that had not been thoroughly hashed out. I also concur with Dr. Kelly's statement, of course, that his primary interest was in other aspects of it.

Do you wish to question me about that at all?

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

MR. ROBB: No, not at this time.

DR. GRAY: I think not, Dr. Oppenheimer. Would you proceed?

BY MR. GARRISON:

Q Would you tell the Board now, Dr. Oppenheimer, about your appointment to the General Advisory Committee in 1946 and then something about its personnel and its purposes?

A I think I did describe my appointment which was in late 1946. Our first meeting was in early 1947. I was held up by bad weather. I think Dr. DuBridge and I were both held up by bad weather and arrived late for the meeting.

MR. GARRISON: This is on the first page of Roman II, Item 4.

THE WITNESS: When I arrived I found the other members of the committee had held a meeting and elected me chairman. After consultation with the Commission itself, I

5 accepted that position. We agreed that we would elect the chairman at every subsequent meeting, that is, the first meeting of each year. I was re-elected at first without any concern on my part, but later with great concern. I will come to that when we come to that time in the history.

I think you have the names of the members of the committee.

DR. EVANS: Yes.

THE WITNESS: It is in my letter. It would only bore you to repeat the names.

MR. GARRISON: They are right before the committee.

MR. SILVERMAN: These were not all members at the same time.

THE WITNESS: No. But I think that is spelled out in my answer. It is obviously an eminent committee and a varied committee. I can assure you that it was not a committee that regarded itself as subject to manipulation, or that it was subject to manipulation.

BY MR. GARRISON:

Q What was the statutory function of the committee?

A The law spells out that it is to advise the Commission on the scientific and technical aspects of research, development, production, materials, something along those lines, a rather clear mandate.

We, of course, from the very beginning recognized

6 with relief that the job of decision making, the job of negotiation with other parts of the Government, the job of management, the final job of determination, rested elsewhere. It rested with the Commission, with the Department of Defense that was to establish military requirements, or rather, with the President who on the advice of the Department of Defense was to establish military requirements; with the Congress that carried out the appropriations. Our job was limited to advice.

A scientific advisor has, I think, one overriding obligation. It is his principal one in which he is delinquent if he fails, and that is to give the best fruits of his knowledge, his experience and his judgment to those who have to make decisions.

He must attempt to study the problems that are put before him, to analyze them, to relate them to his own experience and to say what he thinks will happen and what he thinks won't happen; what he thinks experiments mean; what he thinks will happen if a program is developed along certain lines.

It is not possible to give this advice except against a background. That background is the kind of questions you ask. Very often the things that are assumed in the questions you ask rather than state. If you are on your toes sometimes you can say that the question is not asked in

7 the right way. That a different question should be asked. But by and large you will find yourself advising on what concerns the people to whom we are feeding advice. This through the years changed a great deal.

I have already testified that as of early 1947, the prospects of any meaningful international action in the field of Atomic Energy were largely gone. The problem that we faced then was to devise a program which would regain some of the wartime impetus and vigor, and above all to make available the existing knowhow, the existing plant, the existing scientific talent, to make this available in the form of actual military strength.

It was not so available as of the first of January 1947. I need not go into the classified details. They are certainly available to you if you want them.

In the period characterized by the Russian bomb and the war in Korea and the Chinese intervention, the background of many questions was immediate readiness for general conflict, or the best we could do with regard to that.

In the last days of my service on the general advisory committee, one of the obvious questions was this: Since things are going quite well for us, what can we do, what should we do, to be prepared against enemy action? No doubt the enemy will have sometime or other similar success.

These changes in the nature of the background were

8 always there and I don't want to pretend that scientific advice in practical matters is like doing an experiment just for the purpose of satisfying your curiosity.

The GAC did not, strictly speaking, abide by its terms of reference. I would say in two or three ways it did not. In the first place in the early days we knew more collectively about the past of the atomic energy undertaking and its present state, technically and to some extent even organizationally or some parts of it, than the Commission did.

The Commission was new; its staff needed to be recruited. We knew about Los Alamos; we knew about Sandia, we knew about the Argonne Laboratory at Oak Ridge, and it was very natural for us not merely to respond to questions that the Commission put, but to suggest to the Commission programs that it ought to undertake; to suggest to the Commission things that needed doing of a technical sort.

Very frequently we would be asked, what will be the best way of organizing this; what will be the best conditions for recruiting scientists and for making their work productive? We never regarded that as a serious violation of our terms of reference.

As time went on and the Commission through its Staff and actually in its membership knew more and more about the program, we tended to let the questions come from them. We would be confronted by great piles of documents and

9 sometimes a set of questions about them at the beginning of every meeting. We would try to answer their questions rather than digging up from our own experience things that we knew.

This transition took place as the members of the committee became more remote from direct active participation in the program and as the Commission's understanding of its problems improved.

Sometimes the Commission would address to us questions which were not obviously related to scientific and technical advice. I would mention at the least three.

The Commission reviewed with us its security procedures, the procedures, I think, under which we are now sitting. I believe their interest in doing that was to find out whether these would seem fair and reasonable to scientists. I don't believe we responded in writing to that, but we probably said that this looked like a very fair set up.

The Commission reviewed with us very often the hassle about the custody of atomic weapons. The Act provides that the President shall arrange their transfer from the Commission to the Military Services. This involved, I guess, both technical and political problems. We in this case confined ourselves to talking about the technical problems and pointing out that there were much more important political ones which it was not our job to pass on.

The very broad terms -- and this, of course, I am

10 coming to in a good deal more detail -- in which the Commission addressed to us the question of the Super Bomb was another example, I think, where it did not consult us purely on the technical problem, but asked advice in which supposed technical competence and general good sense were supposed to be blended.

I haven't got all the examples, and I know many times we bowed out and did not answer the questions which were not technical and scientific. Often we were seduced into answering them.

The committee, during my chairmanship, met about thirty times in regular stated meetings. I think the most impressive thing -- maybe we did some good -- but the most impressive procedural thing is that the committee had nine members; that means 270 attendances, and I believe there were not more than five, or something close to that number of absences. That is, almost always everybody would be there and it was a rare meeting where two people, if there was such a meeting, would be absent. There were occasions where a member was abroad, as in the case of Dr. Seaborg in our meeting in October, 1949. But they were not frequent.

This active interest and participation, I think, shows that the members of the committee, whatever the truth was, felt that what they were being asked to do was important to the nation and they had a contribution to make.

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We had several sub-committees appointed very early in the game, that is, into the natural divisions of the problem: A subcommittee on weapons, with Dr. Conant of Harvard as chairman; a subcommittee on reactors, of which, I think, Dr. Cyril Smith was chairman; and a subcommittee on research, of which Dr. DuBridge was chairman.

We also had an ad hoc subcommittee which lasted only a limited time to consider the problems of the best possible way in which existing or shortly to be available plant and existing raw material could be used to increase the quality and usefulness of the product, here, I think, only from the point of view of weapons; that is, how did you operate this plant? Did you operate them in parallel; were they independent units, and so on. That was under the chairmanship of Fermi, who was from the University of Chicago.

The committee as such had some foreign relations.

BY MR. GARRISON:

Q By "foreign relations", you mean with other agencies of Government?

A Thank you; with other agencies of Government.

We met quite frequently, especially in the early days, with the military liaison committee. It was usually present during our final report to the Commission.

The committee, at least once or more than once, appeared before the Joint Congressional Committee. Its

12 members appeared in open sessions during the spring of 1949 and in secret sessions.

We once, I think, called upon the President and wrote him an unclassified progress report. At the end of my service we wrote him a top secret progress report which I sent over and talked over with him when I visited him.

But by and large our relations were only those established by law to advise the Commission and we stuck pretty closely to that.

There is an important qualification to this. Many members of the committee were consultants to one or another of the laboratories. Rabi, for instance, was a founder of Brookhaven and very much interested in it. Fermi was a consultant to Los Alamos. So was Von Neumann, who came on later.

Many of the members of the committee had connections with Oak Ridge and the Argonne Laboratory. In addition to that we were, of course, a part of the general traffic of scientists. We knew each other. Therefore, we had another function besides advising the Commission on technical matters, and that was to represent to the Commission when it was a clear and obvious thing, the views of our colleagues and to represent to our colleagues the views of the Commission.

I mean by this, those who were engaged in the work, if the matters were classified; those who were not engaged in

13 the work if it were such a thing as the support of basic science or a fellowship program or anything like that.

We got our information initially because we had it in our heads and had some reports left over from earlier times, overwhelmingly from Commission sources, but to some extent also by direct visits to the laboratories and by calling in directors of the laboratories, by calling in staff from the laboratories, so we tried to keep up to date.

I think we had Bradbury on very many times to tell us about the weapons work in the early days. Our Secretary was John Manley, and he was Associate Director of Los Alamos, so he would bring a report to us, sometimes semi-official and sometimes informal, of what was going on.

We consulted with the directors of all the laboratories at one time or another, and where relevant, with the people in charge of production plants.

We did one other thing which perhaps was not quite within the terms of the statute. Occasionally we would propose for the Commission, or rather, prepare for the Commission a statement of views which we would authorize them to make public. These were non-classified statements in hearings before the Congress or in anyway that they wanted.

I remember one such occasion when we thought a public statement would be desirable to set the atomic power

14        problem in some kind of perspective so that people would not expect that coal and oil would be obsolete the day after tomorrow. We drafted a statement of this kind. First it was secret and then we got all the secret stuff out of it and handed it to the Commission. It used it in some way -- I think not a terribly effective way -- in a report to Congress. I think it was in regard to the use of isotopes, the fellowship programs, the promotion of basic research. We wrote several documents for the Commission to us if it would do them any good.

BY MR. GARRISON:

Q        When you say, Dr. Oppenheimer, that the committee acted beyond the statutory frame of reference, what you really mean, I take it, is that you did not act in violation of the statute?

A        Oh, no.

Q        But that it simply came about that the Atomic Energy Commission looked to your committee for help and guidance in ways that perhaps had not been foreseen?

A        That is exactly right. The Commission relied on us very heavily, especially at the beginning, and relied on us for lots of things that were not provided for in the Act; where we felt we could help them we did. Our concern was to give them every possible encouragement and support.

Q        And then, as you testified a little earlier, as

15 you testified a little earlier, as the Commission became more and more expert in its own field there was correspondingly less dependence for this kind of assistance from the committee?

A That is right.

Q Now, would you tell the Board something about what the committee actually did and begin with the first meeting?

A My recollection is not clear as to what happened at the first and what happened at the second meeting, but I think this is perhaps not too important.

Very early in the game we thought it important to see whether we agreed or had any views at all about what the job of the Commission was. That, of course, was the Commission's business to determine, but the nature of the advice we gave would be dependent on that.

Without debate -- I suppose with some melancholy -- we concluded that the principal job of the Commission was to provide atomic weapons and good atomic weapons and many atomic weapons. This referred to atomic explosives. There are other things, like the atomic submarine that you can call an atomic weapon, but that is not what we had in mind.

We thought it had three other undertakings. We thought from the first that however remote civil power might be, the Commission had an absolute mandate to do everything

16 it could economically and fruitfully to get on with the exploration of it. We thought that the Commission needed to respond to requests from the military and needed to alert the military establishments to other applications of atomic energy of military use, of which propulsion, radiological warfare may be two examples. I won't attempt to evaluate them at this moment.

The third thing that we felt -- and it was not really third in our feelings, but simply in a budgetary and practical way -- was that the Commission had a mandate to stimulate basic science in this country: The training of scientists; I guess just the acquisition of knowledge is what the law states.

At that time there existed in the Office of Naval Research one very good government agency which was promoting basic science in many different fields with great forethought, wisdom and skill. Some of the things the Office of Naval Research did touched on the field that the Commission was in on atomic science. We never had any feeling that it was bad for the ONR to be in that. But this was to come up over and over again and I will return to it a little bit later.

These were the principal themes that occurred to us at the first meeting and the one that separated itself by urgency and importance in our own minds was the weapons field.

17

Q That required attention first of all to the state of affairs at Los Alamos?

A Yes. I think perhaps I should say that we did at one early meeting consider whether Los Alamos was the right place for weapons development.

Q This is now 1947?

A This would be early 1947. It was set up during the war for reasons which I went over yesterday. It is remote. It is expensive. It doesnot have very free access to a university or laboratories not under its control. There could have been arguments that a fresh start with something of the vigor that Los Alamos had when we began it might have been desirable.

We concluded at the first meeting that this was impractical; that Los Alamos had proved itself and its survival value by being there, by having a good staff, it was working on atomic bombs. It was not only working on atomic bombs but doing a lot of miscellaneous physics and chemistry. But it existed and the notion of starting up something else or tearing this down seemed to us full of dangerous delay.

So our first set of recommendations to the Commission was addressed -- I think there were a lot at one time -- but at any rate first among the recommendations were the recommendation to get Los Alamos going as a really first rate place.

18

The Commission had asked us either at our first or second meeting to review the report I described yesterday on the job in atomic energy which we had written for Mr. Stimson's panel. They asked us the question: Have any of these objectives been attained? They had not been. The time was rather short. The objectives were not easy. I think we said strictly speaking now has been attained. There are some now that ought to be added that have come up in the meantime. That report was not entirely complete.

We suggested that every inducement be made available to make work at Los Alamos attractive in the way of salaries and housing, but above all in the morale sphere in the sense of giving the men who were there the feeling that they were doing something vital for their country and in getting abroad in the country the sense that Los Alamos was not something left over from the last war, that work on the atomic bombs was somehow not an entirely creditable occupation, but quite the contrary feeling that there was nothing the nation needed more.

This did result in vast building programs at Los Alamos, in the expansion of the laboratory, in the availability to the laboratory of a great many people who were not trafficking there at earlier times. People go out now for the summer months and have been for the last five or six years and they come as consultants.

19

There is hardly a clear and qualified scientist in the country who is not available to Los Alamos for consultation or for such things as he is good for.

They have established a scheme of sub-contracting which enables them to draw in even further resources than they can put on this relatively limited mesa.

I am not going to take all the recommendations of our early meetings. In the first place I have not looked them up and I don't have them in mind. I will rather follow the weapons themselves.

There had been, I think, some thought about weapons development after I left Los Alamos. There was one meeting which I could not attend on the thermo-nuclear program, and there were lots of things left over from the wartime to get people interested in making better weapons, better here meaning a whole lot of things. It means obviously getting more bang for a buck. It means more economy in the use of fissionable material. It means getting weapons which give you the maximum versatility in the kind of delivery system we have, so you don't have to use very big bombers and so on.

It means versatility in the size of weapons and their explosive effects. It means the ability to use the fissionable materials that are produced in some reasonable proportion to how they are produced and in some reasonable recognition of overall economy of neutrons and production

20 facilities.

Very early in the game it became clear to us that nobody was going to pay attention to improving weapons. All that happened is that there were lots of blueprints and lots of models lying around and the only way to get this business really moving was through a testing program. The pay-off with atomic weapons is to see if they really work as we think they do.

Sometimes you do this test to prove out a model which is essentially what you think is right. Sometimes you do it in order to see, as well as you can by experiment, how things are working in the explosion and guide you in future design. Good tests usually combine these features.

I believe we were extremely strong in urging that a test facility be established. I know that we worked quite hard to get accepted the initial Los Alamos program for the Eniwetok tests which were a little more ambitious than was generally approved and where we felt they were really very much needed.

We were worried about the test site out in the Pacific as the only test site because of the cumbersomeness and the long advanced planning that was required. But the problem of getting a continental test site was one to which we could not contribute much except to say that it was very much needed and that we hoped it would be available.

21

MR. GRAY: May I ask, when you say "we", you are always referring to GAC?

THE WITNESS: For this field I am talking about the GAC. There were points on which we had differences of opinion. They were not very frequent. I believe in the weapons field they were not very major.

There were differences of opinion about the proper way to get reactor development going and perhaps some difference of opinion about the value of various forms of military propulsion. What I am reciting now I believe to be unanimous.

BY MR. GARRISON:

Q Dr. Oppenheimer, in all of the recommendations that were made throughout these years from 1947 to 1952, during which you were chairman, did you concur in those recommendations yourself personally? I mean to say that if there were differences of opinion, were there any instances in which recommendations were made in which you did not concur?

A I think there may be that there were, but I don't remember them. They were not on points that seemed of great importance.

MR. GRAY: May I ask as a matter of practice if the committee made a report and then if members had some difference of view they were reflected in a separate memorandum?

THE WITNESS: The way it worked is the following:

22

Maybe I had better go back to procedures. The meeting was generally opened by a meeting with the Commission, sometimes with the military liaison committee, at which the Commission would discuss with us what was on its mind, what advice it wanted.

There would be a period of briefings in which documents were brought in and the Staff came and very often members of the various laboratories came and told their story. Usually there was more to consider than could be adequately considered in a two or three day meeting.

We then would go into executive session, go over the program aloud and being to talk about questions. Sometimes it was clear that the answer was obvious. Sometimes it was very tough. Sometimes we felt that the right answer would be very difficult for the Commission to carry out and we had the problem of giving our advise to the commission in a way which was both honest and useful.

When we were about clear as to what we had to say we would met again with the Commission, and occasionally with the military liaison committee, and at that point I would usually sumarize out loud what our thoughts were and a record would be made of that. If I knew of divergences of opinion, I would call on those who had any divergent opinion to express their differences; If I didn't know about any, in any case I would go around the table asking for

23 comments. There almost always were some comments because I had forgotten something, or I had given an emphasis which was not right, or some one wanted to strengthen what I had said.

This oral report I then made the basis of a letter to the Commission which was our immediate report to them. This was circulated to the members of the committee who could approve it and it was brought up for approval and amendment at the subsequent meeting as to whether it was an adequate expression of the Commission's views.

I remember one instance in which there was a dissent -- one and only one instance -- from my representation of the view of another member who said I had not gotten it straight and who wrote a letter amplifying.

We also, not always, but normally kept minutes. I say not always because I have the impression that the most controversial meeting in the light of history, that of October 1949, minutes were not kept. The meeting was too hectic, or something. The secretary never wrote them up, but wrote notes afterwards. You know that better than I do.

The reports of the Commission, of course, though they usually were top secret or often top secret, were the Commission's property, and if it wanted to send them over to the Joint Congressional Committee, or the military liaison committee or anyone else, that was fine with us.

24

The minutes of the meeting, which often told what kind of hassles we had, what kind of arguments or considerations, we made available to the Commission to throw whatever light they could on what we knew and what we thought, but we asked them not to distribute the minutes since they identified individuals as saying this or that.

I think this is how the record was kept.

BY MR. GARRISON:

Q I wanted the Board to be sure, Dr. Oppenheimer, that when you recount, as you are about to do -- and, indeed, as you have already begun doing -- some of the important things that the committee recommended to the Commission and urged upon it in the national interest, they were all actions in which you yourself wholeheartedly approved.

A If I had dissented, I would certainly have said so.

Q So that the Board can understand that, you were really talking as much about your own views and contributions as you are about other people.

A Yes, although I need to make one point clear. It is very important for a chairman to get everybody into the act and not to dominate a meeting. I think my normal practice was to bring up a question and then see what other members of the committee would say. I would not wish to testify, and I can't testify, that the views which I came out of the meeting with were always the same as the views I went

25 into the meeting with. This was a matter of discussion. Sometimes new facts were brought to light, sometimes we learned things we had not known before; sometimes people talked me out of what I originally thought. But I certainly never incorporated in a report anything different than I thought was the best advice that I would give at that point.

Q You have spoken now about the stress which the committee laid on the importance of tests for the development of atomic weapons. Do you want to say something about some of the other aspects of weapon improvement which you pressed for in those days.

DR. EVANS: Pardon me, but may I ask one question about these tests before you leave that?

Dr. Oppenheimer, were there what we might call bad tests that did not come up to your mathematical calculations?

THE WITNESS: I am not sure whether the answer to this is classified or not?

DR. EVANS: May be I should not ask it.

THE WITNESS: The Security Officer has left, but I will take the chance.

DR. EVANS: I will hold the question.

THE WITNESS: All right. The answer is of some interest, but not, I think, in connection with whether I am fit to serve the country.

MR. GARRISON: If the Chairman would like, we would

be glad to step out.

THE WITNESS: Let us not have any more classified stuff than we have to.

I ought to say that at our first meeting or two, I don't remember which, we brooded to a very considerable length about the thermo-nuclear program. I think the state of affairs was that not much was known about it, it had not been pursued very vigorously, and the unknowns overwhelmed the knowns.

BY MR. GARRISON:

Q Just to recapitulate, the work in the thermo-nuclear field began when at Los Alamos?

A The theoretical work began in Berkely in the summer of 1942. The thermo-nuclear work was pursued merely as a theoretical job and not a developmental job. I think it would naturally have been somewhat intensified after the war with the view of making better measurements and better calculations because it was one of the interesting things to do.

The question we tried to ask ourselves was: Is there enough in this so that it ought to be pushed, or is it something that will be a distraction from the very immediate job of getting some weapons into the places where they are needed? Our answer was, I think, the following: That is was a very interesting problem or set of problems; that

if work were going on at Los Alamos it would attract first rate theoretical physicists and that the probability was that if people studied the thermo-nuclear problems at Los Alamos this would help the other program rather than hurt it because it would have the effect of increasing the brains and resources of the laboratory.

I will have to give you a complete review of the thermo-nuclear thing, but this was our initial recommendation.

We made a number of other observations relevant to the weapons program. I think one of the important ones -- I am not sure we were the first to do it -- was to keep asking the Commission not how many bombs should they make, because that was not our job -- that was the job of the military establishment -- but what were the real limits on how many they could make. How much material could be made available? Because even though very great strides were made between 1947 and 1949 in the effectiveness with which material was used, there was still the question: Is the plant we have being used in the best possible way? Is there any inherent limitation on the plant? Is there enough raw material to sustain more plant? Is there anyway in which you can relieve the limitation on raw material? Does this come back to a dollar limitation?

We addressed to the Commission from time to time questions intended to make clear to the military establishment

that the requirements they were placing for atomic weapons were perhaps all that could be done right then with existing plant, raw material, operation and bomb design, but by no means all that you could do if you really set to work on it.

The very large expansion programs which, of course, were not approved or formulated by us were certainly in part stimulated by the set of questions. There have been several expansion programs and the whole atomic weapons capacity has risen enormously. It took quite a while for this to take hold, but I think we started on it fairly early.

We were very concerned -- I think probably this concern reached its maximum during the Korean War, but started earlier and continued later -- to adapting atomic warheads so that they could be used by a variety of carriers. This sometimes meant developing designs which were not from the point of view of nuclear physics the most perfect design because you had to make a compromise in order to get the thing light or small or thin or whatever else it was that the carrier required. But experience showed that almost every improvement that you made in trying to make, let us say, a physically smaller atomic bomb was reflected in an improvement in the performance of the larger ones.

So as this thing began to unroll you could not really tell whether an effort aimed at making an atomic bomb that you could shoot out of a machine gun -- to take an

obviously unclassified example -- would not also help the very large bombs which are the most efficient.

This had something to do with trying to bring together the enormous program of which our chairman surely knows a good deal of missiles and the adaptation of weapons plans and missile plans. In this connection we welcomed the building up of Sandia that Dr. Kelly has described to you, and tried generally to get as much coordination between the hardware side, the military application side, and the development of the atomic explosives themselves. I believe we were rather early in this preoccupation, which later became quite general.

We were concerned with flexibility and made a number of recommendations to the Commission, which I need not spell out, the purpose of which was to be sure that if during a war you found out bombs you had were not exactly the one you wanted you could do something about it. We felt that no amount of crystal balling would make it certain that your stockpile corresponded to what you really needed in combat.

We suggested a variety of devices by which you could take advantage of what you learned in combat and come up quickly with what you needed.

I have listed these as some of the things about weapons. I have obviously left the hydrogen bomb for a

separate item. I might run rather briefly through the other aspects of the Commission's work that I have mentioned.

The war almost stopped the training of scientists in this country and this started up again at an accelerated pace under the G.I. Bill and the rest of it. But it was very clear that there were not enough people in the country to do the things that were needed. The couple of billion dollars which we now spend on research and development is not all spent on the salary of scientists, but it is very often bottlenecked by scientists.

It seemed to us that the source of all this was universities and university training. It seemed to us that the source of all this was the research in universities, in other words. It seemed to us that the source of the good work that had been done in the war was not in applied science but in the pure scientists who had learned their stuff in the hardest of all fields, the exploration of something that is really not known and really new.

We encouraged the Commission to take a number of steps which we thought would help this. They have, first of all, their regional laboratories, of which Brookhaven is a good example, Argonne is a good example, Oak Ridge and Berkely. There we tried to get the Commission to do something which was only partially successful but has been quite successful in Brookhaven, and that is to separate as sharply as

possible the secret and sensitive things which ought to be guarded and restricted, and the things that are just published all the time in the journals, and therefore, to make it possible for these facilities to serve as wide a group of people as possible without involving delays and clearance procedures and in order to maintain really secure the things that were secret.

We tried very hard to get the Commission to support work which was not directly obviously related to the practical applications of atomic energy. There were arguments in those days that the Commission was so shorthanded, so in need of physicists that the best thing they could do was to make it hard for physicists to get jobs so that they would come and work in the various laboratories. We thought that was quite wrong; that the best thing they could do was to support physics in the universities, that this would provide the young men -- and it has, of course -- who would be able to man their various laboratories in the years to come and they should do at least as well as the Office of Naval Research in those fields of science which by statute they were supposed to be responsible for -- atomic science and chemistry, physics, geology. They have done this and any one who picks up a contemporary physics journal will see in it innumerable example where it says that this work was supported by the Atomic Energy Commission.

The level of activity in physics, especially, but also in chemistry has been very much raised by their efforts and the number of people practicing has been enormously raised. What is more than that, if you now go to a contemporary Atomic Energy Commission Laboratory, a lot of the bright ideas and a lot of the best work is done by men whose names were not known seven or eight years ago and who have precisely come up through university training in the meantime. This is true of Los Alamos and it is true of all the others.

I think on this we probably pushed the Commission and they regarded us as people who were after all largely professors and university presidents and we were pleading a special interest. We did plead a special interest, but we believed it to be in the national interest, too.

Where possible in basic science we urged the Commission to make its unclassified facilities available on a world-wide basis. A good many scientists from friendly nations have come here to do experiments, to learn techniques and also to teach us what they knew, and there are magnificent examples of international collaboration that have taken place in the Commission's laboratories. I think the most striking is probably known to you.

In 1947, I guess, the big accelerator at Berkely started operation. Maybe it was 1946. People immediately looked to see whether the new high energies that were being provided

were creating mesons which we knew were created in the cosmic rays, but which were not artificially created before. They looked for months and months and the reports were negative. This seemed very puzzling from the point of view of the theory.

A young Brazilian who had been studying in England arrived at the Radiation Laboratory, knew the technique used there, exposed a few photographic plates and there were the mesons. This is a small illustration of the need from the scientific point of view of the international collaboration.

I think I need not point out that it is also a very limited but a very healthy element in the general structure of our alliances and in the good feelings that exist between people in other countries and here at home.

The Commission has, I think, and we so represented it, an obligation to make available to industry and to technology and medicine those facilities which by statute it and only it can operate. It has fulfilled this very well. The distribution of isotopes had been begun by the Manhattan District. It has been enormously expanded and speeded up and improved by the Commission. This is one example.

The use of reactors for both secret and non-secret work is another example. I don't know how much you have found it profitable to leaf through the General Advisory Committee reports. I am sure you will find in them just

countless occasions where either in general terms or in specific terms we tried to steer the Commission on a course which would enable it to do the maximum for American science.

I am not so proud of our record in the reactor program. This we never managed to give as effective advice about as I wished. We worried a lot about it and you will find that if the advice was not good it was at least copious.

I think one reason for the difficulty is that progress in reactor development, whether for civil or military purposes, is a very expensive thing. It is the kind of thing you don't do in a small university laboratory. It is a big industrial enterprise. It may cost 10 million dollars, it may cost 50 million dollars. It is not something you can just try out for size.

We found it very hard to compose the conflict between the need for an orderly and comprehensive and intelligible program of reactor development and the inevitable enthusiasm which groups would get to have for their own pet baby and which maybe was a reactor which was not especially illuminating from the point of view of the program as a whole. We thought at one time that this could be helped by centralizing the reactor development work and so recommended to the Commission. This was one of the recommendations which was opposed. Fermi thought this was bad advice. In any case, it never happened. So we don't know whether it would have

been good or not. We tried very hard to get some kind of policy committee of the people who knew about reactors, and that was formed, a committee of Oak Ridge and Argonne and General Electric scientists, so that they would get some agreement and not all push their own babies.

We strongly urged the Commission to get somebody in Washington who was an expert in reactors and it turned out to be the Director of Reactor Development, Dr. Hafstad, who held that job from the beginning. I am not clear that he will be on any of your lists.

What in the end happened was that we began to sort out better and the Commission began to sort out better what the reactors were for, and therefore have more rational criteria of which ones to build. They were for production, the production of materials for bombs. They were for military propulsion. They were for learning about reactors so that you would know how to build the next ones better. These three purposes I think we recognized in 1947 or 1948.

After that I think the Commission's program began to take extremely good shape and we have moved very far. We always liked the submarine reactor, not only because it would be a useable thing in warfare, but it looked close enough to civil power, relevant enough to civil power, to be of interest from that point of view, too.

I believe we dragged our feet very much on the initial

plans for flying aircraft with nuclear power. It seemed to us a very long range thing, and one that ought to be approached in the spirit of research rather than have a definite development and commitment. When I last heard about it, this was the state of affairs.

BY MR. GARRISON:

Q This brings us logically to the report on the H-bomb in the fall of 1949. I don't know whether the Board would think this was an appropriate point to adjourn or whether we should go ahead and start on it.

MR. GRAY: I think we should start on it, Mr. Garrison, if you don't mind.

BY MR. GARRISON:

Q The story begins, I take it, with the Russian explosion of an atomic bomb on September 23, 1949?

A I don't think the story begins there. I will go back a little bit. We can begin in the middle and go both backward and forward.

In September of 1949, I had a call from either General Nelson or Mr. Northrop. They were involved in the detectbn net for Soviet atomic explosions or anyway for foreign atomic explosions, and they said that they had something very important.

A little later I came down to Washington and met with a panel. I see it says in my summary that this was

advisory to General Vandenberg. I never was entirely clear as to who the panel was supposed to advise.

MR. GRAY: This appears in the exhibit?

THE WITNESS: That is right. This was Admiral Parsons, Dr. Bacher, Dr. Bush.

DR. EVANS: Where is that?

MR. ECKER: It is Item 6, II.

DR. EVANS: Yes, I have it.

THE WITNESS: I think I had seen a good deal of the evidence before the panel was convened. In any case, we went over it very carefully and it was very clear to us that this was the real thing, and there was not any doubt about it. We so reported to whomever we were reporting. I think it was General Vandenberg. This was an atomic bomb, or at least it could well have been, and there was no reason to doubt that it was a good one.

Yesterday you read evidence that in 1948 I was not thinking it would come so soon. That was the prevailing opinion. In every meeting of the General Advisory Committee nearly we had a briefing on what was called atomic intelligence. It is common knowledge that prior to the Soviet explosion, the earliest possible date was considerably later than the actual explosion and the probable date quite a lot later. The fact is we didn't know what was going on. So this came as an immense shock, and to everyone involved clearly meant

some re-thinking of many aspects of United States policy.

I went over to the State Department where the question was being discussed -- I was asked to go over by the Under Secretary -- should this be publicly announced by the President and I gave some arguments in favor of that.

I don't know who finally resolved the matter, but the President did make a public statement. I was taken up to hearings before the Joint Congressional Committee. General Vandenberg certainly appeared and probably Admiral Hillenkoette and other people whom I have forgotten. The committee was quite skeptical as to whether this was the real thing.

MR. GRAY: Is this the GAC?

THE WITNESS: No, the Joint Congressional Committee. They were quite skeptical and I was not allowed to tell them the evidence. It was understood that this was to be kept secret. All I could do was just sound as serious and convinced and certain about it as I knew how. I think by the time we left the Joint Congressional Committee understood that this event had been real. I do remember Senator Vandenberg's asking me, and it was the last time I met with him -- he became ill not long thereafter -- "Doctor, what do we do now?" I should have said I don't know. I did say we should stay strong and healthy, and we make sure of our friends. This was immediately before the General Advisory Committee meeting.

The committee had a whole lot of stuff on its docket. I have forgotten the details. There was a docket for us. We disposed of that business, and we talked about this event. At that point Dr. Rabi returned. He had been in Europe on the UNESCO Mission. He read about this in the newspapers. The President had announced it. He said very naturally, "I think we ought to decide what to do. I think we ought to advise the Commission." I opposed that. I think most all other members of the committee did on the ground that it might take a little while to think what to do and also on the ground that many of the things to do would be done against a framework of governmental decision as to which at that point we could only speculate.

During October or late September, I think October, a good many people came to see me or called me or wrote me letters about the Super program. I remember three three things. Dr. Teller arrived. He told me that he thought this was the moment to go all out on the hydrogen bomb program.

MR. GRAY: May I interrupt? I am sorry. This is following --

THE WITNESS: Following the GAC meeting of September and prior to the meeting in October.

MR. GRAY: Yes.

THE WITNESS: Dr. Bethe arrived. I think they

were there together or their visits partly overlapped, although I am not sure. He was very worried about it. He will testify.

BY MR. GARRISON: About what?

A About the thermonuclear program, whether it was right or wrong; what his relations to it should be. I assume he will testify to that better than I can. It was not clear to me what the right thing to do was.

MR. ROBB: You say to you or to him?

THE WITNESS: To me. I had a communication. I can't find it as a letter, and I don't know whether it was a letter or phone call. It was from Dr. Conant. He said that this would be a very great mistake.

BY MR. GARRISON:

Q What would be a great mistake?

A To go all out with the Super. Presumably he also will testify to this. He did not go into detail, but said if it ever came before the General Advisory Committee, he would certainly oppose it as folly.

The General Advisory Committee was called to meet in Washington, and met on two questions which were obviously related. The first was, was the Commission doing what it ought to be doing. Were there other things which it should now be undertaking in the light of the Soviet explosion.

The second was the special case of this; was it

crash development, the most rapid possible development and construction of a Super among the things that the Commission ought to be doing.

Now I have reviewed for you in other connections some of the earlier hydrogen bomb tale. The work on it in the summer of 1942, when we were quite enthusiastic about the possibility, my report on this work to Bush, the wartime work in which there were two discoveries, one was very much casting doubt on the feasibility, and one which had a more encouraging quality with regard to the feasibility. Of the talks with General Groves in which he had indicated that this was not something to rush into after the war. Of the early postwar work, prior to the establishment of the Commission. Of our encouragement to the Commission and thus to Los Alamos and also directly to Los Alamos to study the problem and get on with it in 1947 and 1948.

The GAC record shows I think that there were some thermonuclear devices that we felt were feasible and sensible and encouraged. I believe this was in 1948. But that we made a technically disparaging remark about the Super in 1948. This was the judgment we then had. I remember that before 1949 and the bomb, Dr. Teller had discussed with me the desirability of his going to Los Alamos and devoting himself to this problem. I encouraged him to do this. In fact, he later reminded me of that, that I encouraged him in

strong terms to do it.

Now, the meetings on --

BY MR. GARRISON:

Q The meeting of October 19?

A The meeting of October 19, 1949. Have we the date right?

MR. ROBB: October 29.

THE WITNESS: October 29. I think what we did was the following. We had a first meeting with the Commission at which they explained to us the double problem: What should they do and should they do this? We then consulted a number of people. We had in the intelligence people. I ought to say that never during my chairmanship of the GAC, never at any time when I had access to it, did I learn of any intelligence that the Russians were working on hydrogen weapons. It may have been true, but there was no evidence. We always pressed the officers to find out.

We had consultations not with the Secretary of State, but with the head of the policy planning staff, who represented him, George Kennan. as to what he thought the Russians might be up to, and where our principal problems lay from the point of view of assessment of Russian behavior and Russian motives. We had consultations with the Military Establishment, General Bradley was there, Admiral Parsons, I think General Hull or General Kyes, head of the Weapons Systems Evaluation

Committee, General Nichols, probably. I won't try to recall all. Also Mr. LeBarron.

Prior to this meeting there had been no great express of interest on the part of the military in more powerful weapons. The atomic bomb had of course been stepped up some, but we had not been pressed to push that development as fast as possible. There had been no suggestion that very large weapons would be very useful. The pressure was all the other way; get as many as you can.

We discussed General Bradley's analysis of the effects of the Russian explosion, and what problems he faced, and with the staff, of course.

Then we went in to executive session. I believe I opened the session by asking Fermi to give an account of the technical state of affairs. He has always been interested in this possibility. I think it occurred to him very early that the high temperatures of a <sup>fission</sup> fusion bomb might be useable in igniting lighter materials. He has also an extremely critical and clear head. I asked others to add to this. Then we went around the table and everybody said what he thought the issues were that were involved. There was a surprising unanimity -- to me very surprising -- that the United States ought not to take the initiative at that time in an all out program for the development of thermonuclear weapons.

Different people spoke in different ways. I don't know how available to you the actual record of this conversation is or even whether it fully exists. But there was not any difference of opinion in the final finding. I don't know whether this is the first thing we considered or whether we considered the Commission's other question first. I imagine we went back and forth between the two of them.

To the Commission's other question, were they doing enough, we answered no. Have you read this report, because if you have, my testimony about it will add nothing.

MR. GRAY: I believe that the report with two --

THE WITNESS: Annexes.

MR. GRAY: I don't know whether they are actually annexes, but two supplementary statements, I don't know whether that is in one page signed by two people or two separate sheets.

THE WITNESS: The report itself you have.

MR. GRAY: The report is available.

BY MR. GARRISON:

Q I think you better say what you recollect of it.

A I recollect of it that the first part of the report contained a series of affirmative recommendations about what the Commission should do. I believe all of them were directed toward weapons expansion, weapons improvement and weapons diversification. Some of them involved the building of new

types of plant which would give a freedom of choice with regard to weapons. Some of them involved just a stepping up of the amount. I don't think that this expressed satisfaction with the current level of the Commission effort.

On the Super program itself, I attempted to give a description of what this weapon was, of what would have to go into it, and what we thought the design would be. I explained that the uncertainties in this game were very great, that one would not know whether one had it or not unless one had built it and tested it, and that realistically one would have to expect not one test, but perhaps more than one test. That this would have to be a program of design and testing.

We had in mind, but I don't think we had clearly enough in mind, that we were talking about a single design which was in its essence frozen, and that the possibility did not occur to us very strongly that there might be quite other ways of going about it. Our report had a single structure in mind -- or almost a single structure -- whose characteristics in terms of blast, of damage, of explosive force, of course, and certainty we tried in the report to describe as faithfully as we knew how. I think in the report itself we were unanimous in hoping that the United States would not have to take the initiative in the development of this weapon.

There were two annexes, neither of which I drafted.

There is nothing of restricted data in those I believe, but perhaps we can't read them into the record anyway. Are there any restricted data?

MR. ROLANDER: I think the question raised is whether other security information might be divulged.

THE WITNESS: How many bombs we have and so on?

MR. ROLANDER: Yes.

MR. ROBB: Perhaps Dr. Oppenheimer could give us his summary.

THE WITNESS: It is a long time since I read them. This ought to be in the record, ought it not? Could you let me read them?

MR. ROBB: They have been available to Dr. Oppenheimer ever since the letter was sent to him. I think that was clearly understood, was it not, Doctor?

THE WITNESS: I was told by counsel that I would be allowed --

MR. ROBB: Any reports that you had prepared?

THE WITNESS: That is right.

MR. ROBB: So far you have not come down to avail yourself of it.

THE WITNESS: I see. They are not here?

MR. ROBB: We have extracts of them, yes, sir.

THE WITNESS: I would think I might read the two annexes and paraphrase them.

MR. GRAY: I think I am going to ask that we recess now, because there is not another matter to bring up not related to the testimony. I think in the meantime, Mr. Robb, the Chairman would like to be advised about this.

MR. ROBB: The security aspect?

MR. GRAY: Yes. So we will recess now until two o'clock.

(Thereupon at 12:25 p.m., a recess was taken until 2 o'clock p.m., the same day.)

(Classified portion of Dr. Oppenheimer's statement, pages 258 to 265, inclusive, appear in separate classified document.)

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thing: if this affair could have been averted on the part of the Russians, I am quite clear that we would be in a safer world today by far.

MR. GRAY: Would you repeat that last sentence. I didn't quite get it.

THE WITNESS: If the development by the enemy as well as by us of thermonuclear weapons could have been averted, I think we would be in a somewhat safer world today than we are. God knows, not entirely safe because atomic bombs are not jolly either.

I remember a few comments at that meeting that I believe it best that people who are coming here to testify speak for themselves about; I am not sure my memory is right--comments of Fermi, of Conant, of Rabi, and of DuBridge as to how they felt about it.

MR. GRAY: How many members of the GAC are being called by you--the members of the GAC at that time?

THE WITNESS: For or five, I think.

MR. GARRISON: Mr. Conant, Dr. Dubridge, Dr. Fermi, Dr. Rabi, Mr. Rowe, Mr. Whitman, Professor Von Neumann--

THE WITNESS: He was not there.

MR. GRAY: It is a substantial membership.

MR. GARRISON: We have a statement from Mr. Manley that we will probably introduce in written form to avoid the necessity of calling him from the State of Washington.

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THE WITNESS: I do not think we called Dr. Cyril Smith, but I will testify that he was an ardent signer of these documents.

MR. GARRISON: Mr. Seaborg was away.

There were meetings after this.

THE WITNESS: Yes. I think we have to keep strictly away from the technical questions. I do not think we want to argue technical questions here, and I do not think it is very meaningful for me to speculate as to how we would have responded had the technical picture at that time been more as it was later.

However, it is my judgment in these things that when you see something that is technically sweet, you go ahead and do it and you argue about what to do about it only after you have had your technical success. That is the way it was with the atomic bomb. I do not think anybody opposed making it; there were some debates about what to do with it after it was made. I cannot very well imagine if we had known in late 1949 what we go to know by early 1951 that the tone of our report would have been the same. You may ask other people how they feel about that. I am not at all sure they will concur; some will and some will not.

In any case, after this report, we had a series of further consultations. I remember that almost immediate afterwards, I consulted with the Secretary of State--I think

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I consulted with him twice, perhaps alone and once with the head of the Policy-Planning Staff--and we talked about this problem.

I remember that the Commission called us down sometime after our meetings, October 29 meeting, called only those members of the Committee that were nearby, those on the East Coast--Conant, Buckley, Rabi and me, five of us-- and we went into it in a more informal session and that is the first time that I became aware of a division of opinion in the Commission and presumably we explained what we had in mind. There is no record of that meeting, or at least I have no record of it, and I have forgotten the details. I know they had another GAC meeting before the President's decision was made, and the Commission asked us to amplify those points. Presumably that was done and presumably you have access to those records, and I have no vivid recollection as to what was said.

In addition to that, toward the end of the period during which the President which making up his mind, I was called by the Joint Committee to come and explain what we had in mind. I was out in California at the time, but when I got back, I did appear before the Joint Committee. This was immediately before the President's decision was made, and I knew how a decision was coming out, but I tried to explain what we had in mind as well as I could. That testimony

gll

is presumably also available to you. It is a fairly long statement, question and answers from the senators and congressmen, and I think it stresses the same points as our first report; that is the impression I have. It is not accessible to me.

In any case, the GAC which had a habit of always being around when something was happening was in Washington when the President issued his announcement saying that we were going ahead with it.

MR. GARRISON: The date of that was when?

THE WITNESS: January 29, 1950. I remember two things: One is that in the relatively short interval between October 29 and January 29, the technical prospects for doing what we were planning to do had deteriorated. This was to continue for a long time, and the essential points had not yet come up. By that time, we were also quite worried how to carry out the Presidential directive. I believe that our report of that meeting, January 29, 1950, said something like this: we are not going to go into the question of the wisdom of the decision. We now have to look at how to carry it out, and we pointed out that there were several things that the Commission needed to get very busy on if the program was to match. It had to make certain materials available in order to support the Los Alamos efforts, and it had to rearrange its programs in certain ways in order

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to get on with the job, and I think it was probably at that time that we got into the details of the Savannah River Plant. The dual purpose of this seemed just right in view of the great technical uncertainties which were both qualitative and quantitative which then existed.

I believe that in every subsequent GAC report where we gave advice on the thermonuclear program, on the super part of it or the other parts of it, that the problem before us was what to do and how to get on with it, what made sense and what did not make sense, and that the morale and ethical and political issues which are touched on in these two annexes were never again mentioned, and that we never again questioned the basic decisions under which we were operating.

We tried, I think, throughout to point out where the really critical questions were. There was a tendency in this job, as in many others, to try to solve the easy problems and try to leave the really tough ones unworried about, and I think we kept rubbing on the toughest one, that this had to be looked into. That was done not completely; perhaps it is not absolutely done completely today, but the situation developed in a most odd way because, by the spring and summer of 1951, things were not stuck in the sense that there was nothing to do, but they were stuck in the sense that there was no program of which you could see the end.

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Now, different people responded differently to that. Teller also pointed out quite rightly that there were other possibilities that might turn up and other people took a very categorical view that the whole business was nonsense.

MR. GARRISON: Scientifically nonsense.

THE WITNESS: Scientifically nonsense. I believe my own record was one that it looked sour but we have had lots of surprises and let's keep open-minded.

I was under very considerable pressure to report in bleak terms through the General Advisory Committee to the Commission and to the military on the prospects. I remember General McCormick saying that we had a duty to do this. At a later time, I remember Admiral Parsons saying that we had a duty to do this to the military rather than to the Commission. We were in somewhat of an uncomfortable position. We recommended against this; it was not going well, and we didn't quite think that it was right for us to say how badly it was going on the ground that this might not be credible, might not be convincing.

What we did do was hold a meeting--perhaps this was the Weapons Subcommittee of the GAC--out at Los Alamos at which we had talks by the people working on the job--Wheeler, Teller, Bradbury--I will not try to list them all--but, anyway, the people who were really doing the work, and we kept a transcript of these talks. We showed the transcript to the people

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whose views were represented and we asked them to edit the transcript and transmitted this transcript to the Commission, not as a report of ours but as a firsthand report of how things looked. I think this would have been in the summer of 1950 or it may have been somewhat later.

At the same time we went over the program with Los Alamos, there were weapons testing programs, their calculation programs, and I believe you will hear evidence that at least some people out there thought we were just the opposite of harmful but quite helpful in connection with this job.

We also kept in touch with and tried to help the production activities of the Commission, some of the engineering activities that went along with the basic research and development. It was partly, I think, in response to the sense that a report on this matter also needed to be available in military circles that the hydrogen Super bomb was included in this report of the panel that we heard of this morning; it was toward the end of 1950, but it was all a part, that part of the advice or which seems to me is most central and basic and inescapable responsibility which is to tell what he knows of what is going on and what he knows of the truth. I feel that in this we did our duty rather well.

There are things that you probably want to question me about in some detail in the General Manager's letter. They have to do with unauthorized distribution of reports.

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We have an affidavit which we will introduce later which throws some light on it. To me, it was an utterly mysterious document. I did, of course--I won't say of course--in fact did show various GAC reports from time to time to a very few people who were actively engaged and responsibly engaged in the program. The purpose of this was certainly not to persuade them to come over to my views but to elicit their views and have a discussion.

I showed some of the reports on the Super to Von Neumann at the Institute who is a very close friend and a very responsible man and whom I knew to be a great enthusiast for this program. I had no notion at all that this was going to change his mind but I thought it right to show them and he certainly was pleased that I did.

I showed nothing at Los Alamos. I wasn't there, and you will have a record of what happened, which I think will satisfy you as to why some of these documents were made available and how little that involved me--at least this is the story that I think will emerge.

It is also alleged that I kept people from working on the hydrogen bomb. If by that it is meant that a knowledge of our views which gotto be rather wide-spread had an effect, I cannot deny it because I don't know, but I think I can deny that I ever talked anybody out of working on the hydrogen bomb or desired to talk anybody out of working on the hydrogen

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bomb. You will have some testimony on this, but since I don't know who the people are who are referred to in the General Manager's letter, what I say might not be entirely responsive. I know that in one case there was a very brilliant young physicist called Conrad Longmire. I think he was at the University of Rochester. In any case, he had applied to come to the Institute, and we granted him a membership there, and he said that he would like to go to Los Alamos for a year and I said, "Find; go do that, and you can have your membership here at any time you want it," in an attempt to make the decision easy for him, because he didn't want to give up his Institute membership. I don't know but that there are other cases. Longmire is still there.

There are times when they communicated with me saying that it would be nice for him to spend a year at the Institute, but he has not come yet. I think we will have to get into the details if there is anything about my slowing down the work on the Super, because, as a general allegation, I find nothing to take hold of there.

MR. GARRISON: May I ask the Board if it would suit your convenience to ask Dr. Oppenheimer questions that you have in mind about any of these portions, or would you rather do it at the end?

MR. GRAY: I think we would rather do it at the end. I have not consulted with Mr. Robb about it.

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MR. ROBB: I think it would be preferable to ask the questions at the end.

MR. GRAY: I think that would be preferable to get the continuity of Dr. Oppenheimer's testimony.

THE WITNESS: I think it would be fair to say that between the first of 1950 and early 1951, my attitude toward this object was that we didn't know how to make it, and it was going to be very hard to make, but we had been told to do it and we must try.

In the spring of 1951, there were some inventions made. They were not discoveries, really; they were inventions, new ideas, and from then on it became clear that this was a program which was bound to succeed. It might not succeed at first shot; you might make mistakes, but for the first time it was solid. It was not on the end; it wasn't so that every time you calculated it it was yes or no, but it came out that you knew that you could do not. It was just a question of how rapidly and how well and I am amazed at the speed at which this actually went after we learned what to do. Ulam and Teller had some very bright ideas; why none of us had them earlier, I cannot explain, except that invention is a somewhat erratic thing.

Teller had been working on this from 1942 on, his heart was in it, but it wasn't until 1951 that he thought about how to do it right.

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Now, I have a few matters here which came in between. During the doldrums of the H-bomb, the war in Korea broke out, and a large part of GAC's and other committee's attention was, as I say, devoted to the very immediate and the very obvious, and, I would say, to using an atomic explosive not merely in a strategic campaign but also in a defensive or tactical campaign, and I think the record will bear out that that is what we were spending most of our time worrying about. That is the origin of the panel Kelly talked about this morning, the origin of the exercises which led to the development of a tactical capability in Europe, the origin of one at least of the threads, one at least of the reasons for the very great expansion in the atomic energy enterprise to support a much more diversified use of weapons, even leading some people to suggest--I think this was Gordon Dean--that maybe the atomic weapons on the battlefield would be so effective that it would not be necessary to use them strategically. I have never really believed that that was possible or believed that a sharp distinction between the two could be maintained or made intelligible.

In the late summer and autumn of 1950, I had an obvious personal worry. I had made as chairman, and had participated in, the recommendation against the development of the Super. The Super was a big item on the program. It wasn't going very well, and I wondered whether another man might not

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make a better chairman for the General Advisory Committee. This was particularly true since there were three new members added to the Committee -- Witman, Murphree and Libby -- and I felt a little uncomfortable about continuing in that office. I discussed it with several physicists. I remember discussing it with Teller and Bacher. Teller says that he does not remember discussing it with me. The general advice was: let's all stick together as well as we can and don't resign and don't change your position.

MR. ROBB: What was that date?

THE WITNESS: In the summer of 1950.

When I got back in the autumn of 1950, the first meeting, I went to see Mr. Dean, who was Chairman of the Commission, and Commissioner Smyth and told them about my problem and they said that obviously the chairman should be someone who would be comfortable with them -- what would be their suggestions? They protested in very forceful terms that I should not quit as chairman, and that they would be very unhappy if I did, that I ought to carry on.

I also took the thing up with our Committee, but our Committee was not a very responsive group when it came to electing other chairmen, and I got noplacement. I did not feel that I ought to resign as chairman or refuse to serve. I thought I ought to do what was comfortable for the Commission and the Committee, and I tried to ascertain what that was.

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MR. GARRISON: How about your Princeton meeting?

THE WITNESS: We are still on the subject of the H-bomb and its consequences.

In the spring of 1951, I called -- I am not sure whether I suggested it or whether Commissioner Smyth suggested it but we consulted about it -- a rather large gathering for a couple of days at the Institute in Princeton, and we had there, I think, all five commissioners, the general manager and his deputy, the head of the Division of Military Applications, Bradbury and his assistants, Teller, Von Neumann, Bethe, Bacher, Fermi who was no longer a member of the Committee, and Wheeler and one of his assistants, the people who were working on the program, and we had a couple of days of exposition and debate. I chaired the meeting, and I suppose I did the summarizing. It was not the full General Advisory Committee -- the Weapons Subcommittee, essentially; the Secretary of the Committee was there and he took some notes but he did not write up an official report. At that time, I think we did three things. We agreed that the new ideas took top place and that although the old ones should be kept on the back burner, the new ones should be pushed. I believe there was no dissent from this; there was no articulated dissent. But later Commissioner <sup>Murray</sup> ~~Murphy~~ asked if this wasn't a violation of the Presidential directive, and I could only respond that I didn't know as to what, but I thought it was

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a good course and, if it was, maybe the President would modify his directive.

At that meeting, I remember no dissent from that but there was a great deal of surprise at how things were changed. Fermi knew nothing of these developments and was quite amazed, and I think for the Commission it was quite an education to see what had happened in the meantime. At least that was the purpose, to get everybody together so that there was a common understanding.

The second thing was to recognize that some materials that had not hitherto prominent, although they had been mentioned, might be handy to have, and the Commission was urged to get started on producing some of these materials. This was something that there was a little bit of objection to on the ground that everything changed so often in the past and maybe change in the future, and why get committed to a cumbersome operation on the basis of the then-existing state of knowledge, but I believe the prevailing opinion, and I know mine, was that the prevailing state of opinion was that it was a lot solidier than anything that had occurred before and that they ought to go ahead and even at the risk of wasting a small amount of money.

The third thing we did was to talk about the construction and test schedules for these things, and there there were differences of opinions, having to do with whether

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the schedule should be aimed at a completed, large-scale explosion, or whether one should be aimed at componentry testing which presumably was supposed to have happened earlier and therefore might be illuminating with regard to the large-scale explosion.

As I say, there was not agreement, but the consensus was that unless the studies of the summer passed out on the feasibility of it, one should aim directly at the large-scale explosion, and the time-scale of that operation from mid-1951 to late 1952 was, I think, a miracle of speed. I know there may be people who disagree, and I think it might have been done faster, but I can only reminisce and say that in the first days of Los Alamos, and in the fall of 1943, Bethe and Teller, two of the most brilliant theorists in this game and in their way most responsible men, said to me: "If we had the material now, we could have a bomb in three weeks." Actually, we were ready for the material just about when it arrived, which was not quite two years later, and the laboratory had doubled every nine months in the interval and everybody was busy; and I think that the estimate of the theorists on how quickly you could do thing that involve engineering and involve new chemistry and involve new metallurgy was likely to be a little optimistic.

I am continually impressed by the speed, sureness, certainty, skill and quality of the work that went into the

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preparation of this first large explosion and the subsequent work to exploit the development there established.

The next thing on which I had notes is that in the autumn of 1951 ---

MR. GARRISON: That was at Princeton?

THE WITNESS: This was the Princeton meeting that I have described. I think it was a very useful meeting. I might have been useful to me if we had made a record of it.

It was largely that it was not a formal-type of GAC meeting and our Secretary did not want to keep a record, but I believe a fairly good account of the substantive findings exist, and I believe Commissioner Smyth knows where to get hold of it. I don't know how to get hold of it.

In the autumn of 1951, there was an international conference in Chicago, and I attended it even though I was called away to testify for money for the National Science Foundation.

While there, I talked at some length with Teller and the summer's work had only made things look tied together. Teller expressed dissatisfaction with the arrangements made at Los Alamos. He didn't think the man whom Bradbury had put in charge of this development was the right man for the job, and he expressed to me the view that Fermi or Betha or I would be the only people that he would be happy to work with. I don't know whether he meant me, but I said, "Well, that is

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fine," and he said that Bethe and Fermi wouldn't; "Would you be willing to?" I won't quote myself verbatim, but I remarked that that would depend on whether I would be welcomed by Bradbury. I had not planned to go back to Los Alamos. It seemed to me a bad thing for an ex-director to return. I was content with my job and work at Princeton, but I would communicate with Bradbury, and I called him and told him of the conversation and he gave no signs of wanting to have the ex-director back, and said that he had full confidence in the present man, and that was the end of that.

I don't believe that it would have been practical. I think you can't make an anomalous rise twice. I think I could create and guide Los Alamos during the war, but I think if I had returned there the situation would have been so different; I would have been ancient and not on my toes anymore, and I doubt if I would have felt appropriate, but, in any case, the success of this would have decisively depended on its being something that was actively in the desires and interests of the director, and it was not so.

The hydrogen bomb was not done, and during the winter of 1951-1952 Los Alamos was working on it, and we kept in quite close touch. Bradbury came in quite frequently. He sent Froman and other people in to report to us, and I want to make it clear that I was not actually calculating out and working on it. I was merely trying to understand

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where the difficulties lay, if any, what the alternatives were, and to form a reasonable judgment so that I might give reasonable advice.

At that time, Teller's unhappiness with the arrangements became quite generally known, and we were frequently asked by the Commission, "Should there be a second laboratory?" We were asked, "Should this work be split off in some way from Los Alamos?" I don't know how many times that came up during the winter of 1951-1952 as an item before the General Advisory Committee.

I think, on this point, we were not unanimous. I think Dr. Libby thought it would be a good idea to have a second laboratory at any time. The laboratory, the purpose of which would be to house Teller and bring you people into the program who were not now working on it, even though this might take some people away from Los Alamos, even though it might interfere with the work then going on. The rest of us, I think, were fairly clear that the things were really going along marvelously well, and that if it was too difficult for Los Alamos to do the whole job, then steps should be taken to get some of their more routine operations moved to Sandia. We talked at great length about the rearrangement of the workload between the two places. Some of the suggestions we made were adopted.

We also talked to Bradbury about making within the

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framework of Los Alamos an advanced development section in which really radical ideas and wild ideas could be thought up and tried out. The Director thought it was feasible if he could get the right man. He tried very hard to get one man for it and, after some delay, this man turned him down, and I don't believe such a reform was undertaken then.

I believe that with the Commission's reluctance to establish a second weapons laboratory, there was some thought that the Air Force might directly establish one, and I think the Commission protested that but this is hearsay.

In any case, during the winter, our recommendations were to fix up Los Alamos so that it could do the job rather than start a separate establishment. Later, in the spring, perhaps in April, we learned that there had been some preliminary talks toward the converting of the Laboratory at Livermore which had been engaged in an enterprise related to atomic energy, of which we the members of the GAC took a rather sour view of converting this, in part, so that it could not more weapons testing work with a special eye to the thermonuclear program. This we liked and this we endorsed.

The Laboratory at Berkeley had often been involved in the instrumentation of weapons tests, and it seemed that this was a healthy growth which wouldn't weaken Los Alamos, which would bring new people into it where there was an existing managerial framework and where the thing could occur gradually,

g27 and, therefore, constructively, the notation of setting out into the desert and building a second site like Los Alamos and building a laboratory around Teller had always seemed to us to be something that was not going to work, given the conditions and given the enormous availability to Los Alamos of the talent that was needed for this problem.

In any case, the Livermore Laboratory was established sometimes perhaps in the summer of 1952, and has played its part in the subsequent work at the time when my clearance was suspended, the major and the practical, and the real parts of the program were still pretty much Los Alamos doing, but it was my hope, all our hope, that both institutions would begin pulling great weight. There had also been no serious friction between them.

MR. GARRISON: Did you tell the Board that Dr. Teller was in charge of the Livermore Laboratory?

THE WITNESS: My understanding is that the director is Herbert York, but that this part of the Laboratory's work was under the scientific direction of Teller. I think the Board probably knows that better than I do at this point.

The Super also--well, it was no longer the super-- I forgot one thing, and it may be of some slight importance. This goes back--and I am sorry to have a bad chronology here--

MR. GRAY: I think the record should show that Dr. Evans has just stepped out of the room.

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MR. ROBB: Dr. Evans has just stepped back into the room.

THE WITNESS: At the time that the H-bomb problem first came up -- I forgot to say two things.

I spoke of my later feeling that I should perhaps not be the chairman of the General Advisory Committee myself -- but two things happened much earlier. I had some talks with the Secretary of State, too, I think, and so had Dr. Conant. Dr. Conant brought back, and so did Mr. Lilienthal, from the Secretary of State two messages; one was a message to Conant and me, for heck's sake not to resign or make any public statements to upset the apple cart but accept this decision as the best to be made and not to make anykind of conflict about it. That was not hard for us to do because we hardly would have seen any way of making a public conflict, and the second part of the message was to be sure to stay on the General Advisory Committee; and that is what both of us did.

There was another item. He recognized, as has Mr. Lilienthal and as would any other sane man, whether or not a hydrogen bomb could be made, how soon we made it, the Russian possession of an atomic bomb raised a lot of other problems, military and political and upset a great many things.

The Government had been saying we had been expecting it, but now here it was -- with regard to the defense of Europe with regard to the usefulness of atomic retaliation in special

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conflicts, and I was called in to help in the preparation of the Security Council paper which was prepared that spring on the subject of which essentially was rearmament and the subject of which was how to solidify our alliances and increase the over-all military power of the United States.

MR. GRAY: This was the spring of 1950?

THE WITNESS: This was the spring of 1950, in NSC 68 or 69, and you probably remember the number better than I do.

It was quite a fine thing and we all thought it was right, but it was the outbreak of the war in Korea that made it possible to do something about it.

In any case, it needs to be testified by me that I was very aware of the fact that you couldn't, within the atomic energy field alone, find a complete or even a very adequate answer to the Russian breaking of our monopoly. I don't think I had a major part in this paper. It took months of staff work to do it. I wouldn't be surprised if -- I don't know whether I had any part -- but, in any case, I approved and helped with some parts of that and its purpose was the build-up which started some months later after Korea.

MR. GRAY: Is that a good breaking point? Shall we take a five-minute recess?

THE WITNESS: It is fine since that is out of order and I apologize for putting it that way.

(Brief recess.)

PAPICH  
BOWN  
FLSAFG

MR. GRAY: Shall we proceed?

THE WITNESS: I have a few more words on the hydrogen bomb which are not very major. The hydrogen bomb once it looked like it got in Dr. Kelly's province, of course, came out in the Research and Development Board committee on which I served. I think the last meeting I attended on the Committee on Atomic Energy just before the Board was dissolved and things organized, we were asked to help find some way of getting very rapidly an emergency capability based on the 1952 test. We did so do. I don't mean that we were the only people that did this. We did relatively little, but that I do remember.

I would like to summarize a little bit this long story I think you will hear from people who believed at the time, and believe now that the advice we gave in 1949 was wrong. You will hear from people who believed at the time and who even believe now that the advice we gave in 1949 was right. I myself would not take either of these extreme views.

I think we were right in believing that any method available consistent with honor and security for keeping these objects out of the arsenals of the enemy would have been a good course to follow. I don't believe we were very clear and I don't believe we were ever very agreed as to what such course might be, or whether such a course existed. I think that if we had had at that time the technical insight

that I now have, we would have concluded that it was almost hopeless to keep this resource out of the enemy hands and maybe we would have given up even suggesting that it be tried. I think if we had had that technical knowledge, then we should have recommended that we go ahead full steam, and then or in 1948 or 1946 or 1945.

I don't want to conceal from you, and I have said it in public speeches so it would not make much sense to conceal from you the dual nature of the hopes which we entertained about the development of bigger and bigger weapons, first the atomic bomb, and then its amplified version, and then these new things.

On the one hand, as we said at the time, and as I now firmly believe, this stuff is going to put an end to major total wars. I don't know whether it will do so in our lifetime. On the other hand, the notion that this will have to come about by the employment of these weapons on a massive scale against civilizations and cities has always bothered me. I suppose that bother is part of the freight I took into the General Advisory Committee, and into the meetings that discussed the hydrogen bomb. No other person may share that view, but I do.

I believe that comes almost to the end except for one thing. I know of no case where I misrepresented or distorted the technical situation in reporting it to my superiors or those to whom I was bound to give advice and

counsel. The nearest thing to it that I know is that in the public version of the Acheson-Lilienthal Report, we somewhat overstated what could be accomplished by denaturing. I believe this was not anything else than in translating from a technical and therefore secret statement into a public and therefore codified statement, we lost some of the precision which should have gone into it, and some of the caution which should have gone into it.

I am now through with this.

BY MR. GARRISON:

Q Dr. Oppenheimer, you said a little while back that you had shown GAC reports to several people. You mentioned von Neumann. I would like to clear up two things. One, to whom specifically do you recall having shown reports, and secondly, what was the character of these people in relation to the government?

A I will tell you what I remember. I showed our discussion of the reactor development program to Wigner, who was the great expert in the field. I wanted to know what he thought. This may have been in 1947 or 1948. Wigner was, of course, an active participant in the reactor development work of the Commission, fully cleared and with very strong views of his own.

Q He was not at Los Alamos?

A No, his work was at Argonne and Oak Ridge. He was

Director of Oak Ridge, and he lives in Princeton. I did not go to any trouble to show it to him.

I showed the one report that I was reading, the October 29 report, to von Neumann at the Institute. He was one of the experts on the thermonuclear problem. He had talked with me, talked my ear off about it before, and also after. I may have shown it to Bethe but I am not sure.

Q Bethe and he were again both cleared for top secret information?

A Yes. I doubt whether I showed it to Bethe, but I am not clear. I don't recollect. I would not have regarded it as improper. I would have regarded it as consistent with my job of attempting properly to advise the Commission and represent the scientific elite to the Commission -- experts, not elite -- and back and forth. I would have regarded it as proper on occasion and with discretion to show and discuss some of these problems with a cleared person. I am quite clear that a great deal of other showing was done in other ways, but that is something I had nothing to do with.

Q With regard to the item of information in the Commission's letter that you caused to be distributed to key personnel at Los Alamos copies of the October 29, 1949, report with a view to influencing them against the H-bomb program, what have you to say about that specifically?

A Specifically I deny it. I never did anything

about having extra copies of reports made or sending them out or anything like that. I had no desire to influence Los Alamos. I certainly did not succeed in influencing Los Alamos.

MR. GARRISON: May I say to the Board that I would like at this point to read into the record an affidavit from Dr. John Manley. I shall hand the original to the Chairman and then to counsel and copies to the members of the Board, and then I will explain what it is about. I introduce this, Mr. Chairman, at this point because in the latter portion of this affidavit there is an account from Dr. Manley's records of what distribution was made at Los Alamos of the report in question. It will show, I think, conclusively that Dr. Oppenheimer had nothing to do with this.

BY MR. GARRISON:

Q Dr. Oppenheimer, could you just tell the Board in a few words who Dr. John Manley is?

A Before the war he was professor of physics at the University of Illinois at Urbana. I knew him slightly. When I was asked by Arthur Compton to take charge of the bomb work, I didn't know much about experimental things and he asked Manley to be my deputy with regard to that. He was, and we worked very closely together. This would have been 1942-43. He helped build the Los Alamos laboratory. He was in charge of the group at Los Alamos in the physics division of the laboratory.

He left Los Alamos after the war, returned to Los Alamos a year or so later, and became, I don't know how immediately, associate director. First he was in charge of the physics division. At that time, after our first meeting, the General Advisory Committee asked me to invite him to become our secretary. He was our secretary until what would have been 1950 or 1951 -- I have forgotten the date -- at that time he left atomic energy work and left Los Alamos and is chairman of the department of the University of Washington at Seattle. He is not Jack Manley.

MR. GARRISON: Mr. Chairman, I am introducing this in affidavit form for a couple of reasons. One, Dr. Manley is in the State of Washington which is quite a little distance from us. Secondly, the part I want most to draw to the Board's attention when I reach it in the affidavit has to do with an account of records of his. It is a little more precise to introduce it in written form, but needless to say, if the Board would like to have us call Dr. Manley, we would be glad to do so. The program is rather crowded, and so there will be perhaps half a dozen written statements which perhaps we will put in the record.

I would like to read this rapidly to the Board now.

"Statement of Dr. John Manley.

"I live at 4528 West Laurel Drive, Seattle 5, Washington. I am a professor of physics and executive officer

of the Department of Physics of the University of Washington.

"I joined the Metallurgical Laboratory in January of 1942. This was before Dr. J. Robert Oppenheimer had anything to do with it. It was under the direction of Dr. A. H. Compton. In July of that year, Dr. Oppenheimer was selected to head the bomb phase of the project. I recall, for example, the expression of pleasure by Dr. Compton that he was able to get Dr. Oppenheimer to head this portion of the activities. At the same time, I was given responsibility for the experimental phase of the bomb project, Dr. Oppenheimer devoting his time to the overall problems and especially the theoretical aspects. (The first time I ever met Dr. Oppenheimer was in connection with this work in about July 1942. I had nothing to do with the selection of Dr. Oppenheimer for his post.)

"During the period from July 1942 to April 1943 I was responsible for the supervision of the experimental work under the direction of Dr. Oppenheimer with headquarters in Chicago. Although he was in residence in Berkeley at that time, he came east frequently for consultation on the detail work under numerous contracts. I was impressed at that time by his ready grasp of even minor details relating to the program.

"In the latter part of 1942, a decision was made to concentrate this phase of the program at Los Alamos, New Mexico. In this connection, I acted directly as an agent

for Dr. Oppenheimer, who was to assume direction of the laboratory. Among other things I undertook the recruitment of personnel, to go to Los Alamos, from those groups who had already been engaged in experimental work.

"In April of 1943 I joined Dr. Oppenheimer at Los Alamos and assumed responsibility for one phase of the experimental program. During the period from 1942 to 1945 in which I continued to be closely associated with Dr. Oppenheimer, the clarity of the wisdom of the choice of him to lead this project increased. I am convinced that no one of my acquaintance possesses either the necessary broad technical knowledge and quick grasp of details or the sympathetic understanding of people which were so necessary to accomplish the project objective in a remote, isolated and self-contained community. I consider it a remarkable achievement, due in very great part to Dr. Oppenheimer's leadership, that this work was completed in time it was.

"During this period at Los Alamos, though I have no specific knowledge of the detailed matters of security procedures, personnel clearances, etc., I can recall no instance or situation which impressed me as suggesting laxity or slighting of security measures. There were, for example, specific instructions from Dr. Oppenheimer in 1943, when I was recruiting personnel, concerning the secret nature of the project, and during the whole Los Alamos period, very evident

support by him of restrictions imposed on civilian personnel, especially with respect to travel, correspondence., etc. As director of the laboratory, Dr. Oppenheimer was normally the recipient of most of the complaints from civilian personnel about security restrictions -- restrictions on travel, etc., and I was impressed with the effectiveness of the job he did in persuading us of the necessity of these restrictions while in no wise relaxing the restrictions.

"I did not know anything about Dr. Oppenheimer's attitude on the question of employment of Communists, or ex-Communists, or pro-Communists; nor did I know whether any of the people employed were or had been Communists or pro-Communists. In my recruitment work I didn't have occasion to go into this question because (a) security was not my job, and (b) the recruitment that I had to do with was largely confined to individuals who were already working on various phases of the project and so had been cleared. I have not to this day heard any suggestions or even rumor of any security leakage with respect to the atomic weapons program for which Dr. Oppenheimer could be charged with personal responsibility, or for which anyone ever suggested that Dr. Oppenheimer was even remotely responsible -- unless the letter of the Commission dated December 23, 1953, suspending Dr. Oppenheimer's security clearance may be deemed to be such a suggestion.

"Although Dr. Oppenheimer left Los Alamos at the close of 1945, I continued there, and in 1946 was asked by him to spend part of my time as secretary to the General Advisory Committee of the Atomic Energy Commission, of which he was chairman (as such secretary I was not a member of the General Advisory Committee). I accepted this duty and from that time until January 1951 I spent about one fifth of my time in connection with the committee work, being at Los Alamos the remainder of the time, first as a division leader, and subsequently as Technical Associate Director of the laboratory.

"In this period I know of no circumstances in which Dr. Oppenheimer attempted to influence in a direct personal way the course of events at Los Alamos (as distinct from the effect that the recommendations of the GAC might, in normal course, have on the work of the laboratory). In fact, I recall that on occasions when I would discuss laboratory problems with him he would frequently say "But that's a problem for you and Norris." (Norris Bradbury, the Director of the Los Alamos Laboratory). Although Dr. Oppenheimer kept informed on the technical features of all phases of the weapons program and was often most helpful to the Laboratory through the GAC or in personal contacts, I believe that he did not feel sufficiently familiar with the details of the Laboratory operation to be able to advise appropriately on internal questions of use of personnel and facilities.

It should be understood that many of the wartime senior personnel of Los Alamos left at the close of the war, and those of us who stayed on felt a very direct challenge to assume all responsibility for the continuing program relying, of course, on occasion, on the technical advice of those individuals who had participated in the wartime program -- individuals such as Dr. Oppenheimer, Dr. Fermi, Dr. Bethe, Dr. Bacher, and so on. It should also be understood that the Laboratory prepared its own program of activities and submitted those to the AEC for approval. In my own dual capacity as secretary to the GAC and one of the senior members of the Los Alamos Laboratory, I felt a special responsibility for liaison between that committee, so largely composed of former Los Alamos personnel, and the laboratory. It is my belief that this dual function of mine was considered valuable both by the committee and the laboratory.

"Shortly after the end of the war, there was considerable discussion among the people at Los Alamos as to whether it would be wise to continue the Los Alamos Laboratory, or whether it would be better to abandon the Los Alamos Laboratory because of its remoteness and the resultant complexity of the operation. It is my impression that Dr. Oppenheimer was not clear in his own mind as to what he thought would be wiser in the national interest. But it was my impression that there was no doubt in Dr. Oppenheimer's mind

that the atomic weapons program had to be continued, whether at Los Alamos or elsewhere, unless the international situation clearly indicated, by agreement, the abandonment of such activities.

"I should like to comment on the operation of the GAC as guided by its Chairman, Dr. Oppenheimer. A less conscientious committee could have considered only such matters as were presented to it by the Commission. The GAC, however, with many individuals senior to the Commission itself in atomic matters, considered it an obligation to supply such guidance to the Commission as its experience suggested might be in the national interest. Each meeting would be devoted to items specifically requested by the Commission and other items which the GAC deemed worthy of discussion. I recall several instances in which the GAC on its own initiative made recommendations for new programs long before the AEC found it possible to start such programs. The GAC was generally understood to be advisory, not simply in a formal sense to the Commission, but to its divisions and laboratories as well. This was accomplished by discussions with appropriate people in and out of GAC meetings and by visits to various laboratories. It was the method by which the GAC kept in close touch with key people and programs of the AEC.

"I should mention also that there was a very close similarity in the thinking of the members of the GAC and the

top people at Los Alamos on most matters relating to weapons programs, so that if there were a division of opinion or doubt on any particular matter within the GAC, there would normally be the same division of opinion or doubt among the top people at Los Alamos. On the other hand, if there was unanimity of opinion and no doubt as to the proper course with respect to any particular question among the people at the GAC, there would normally be the same unanimity of opinion and lack of doubt as to those matters among the top people at Los Alamos. This was not primarily because either the people at Los Alamos took their lead from the GAC or the other way round (although of course each group normally would be, to some extent, influenced by the thinking of the other group); but the essential reason for the similarity was just that both groups had a common recognition of the national need and the limitations of facilities and personnel.

"This was true with respect to the debate concerning thermonuclear programs which became a subject of vigorous discussion at Los Alamos following the Russian explosion of an atomic bomb in September 1949. This debate continued until resolved by the President's announcement in January 1950. In this period there was, as in the past, informal exchange of views between members of the GAC and the senior personnel of the Laboratory."

Now comes the part, Mr. Chairman, that is particularly

pertinent to the question I put to Dr. Oppenheimer.

"I have been informed that it has been charged that Dr. Oppenheimer caused to be distributed separately and in private to personnel at Los Alamos certain majority and minority reports of the GAC having to do with the thermo-nuclear program. With reference to this matter, the following statements of my own knowledge are made:

"A. On November 10, 1949 while en route from Washington, D. C. to Los Alamos, I received a phone call from Carroll Wilson, AEC General Manager. The substance of this call was that Senator McMahon had requested copies of the GAC papers from the AEC and these had been sent to him. In view of the forthcoming visit of the Senator to Los Alamos, Mr. Wilson wished me to show the documents to Bradbury and C. L. Tyler (AEC Manager at Los Alamos) and discuss their contents. He wished me also to show them to Wally Zinn (Director, Argonne Laboratory), but as I was not carrying the documents, this was impossible. Mr. Wilson also asked if I would go on to Berkeley and talk to Earnest Lawrence (Director, University of California Radiation Laboratory). I replied that since Bradbury would be away from Los Alamos all the following week, and I would be in charge, I could not comply with this request.

"B. Neither Bradbury nor Tyler were available when I arrived at Los Alamos on November 11, 1949, so the

session with them was held the afternoon of November 12. At this session, I showed them the papers which had arrived by courier and tried to supply them with the background discussion which led to the papers.

"C. In view of the fact that Senator McMahon would be in Los Alamos the following week for discussion with senior laboratory personnel (Tech. Board, except Dr. Bradbury, who left, I think, on November 13) I showed and discussed these papers with the following:

"J. M. B. Kellogg - evening November 12.

"Carson Mark - morning November 13.

"Edward Teller - morning November 13.

"Robert Kimball - evening November 13.

"Alvin Graves - morning November 14.

"Darol Froman - morning November 14.

"I would add that I feel quite certain that the papers were shown to other members of the Tech Board who were to be present in the meeting with McMahon though my appointment list does not show this. In each case it was emphasized that the policy question was under consideration in highest governmental quarters and discussion of such matters should be strictly limited to Senior personnel.

"D. The reports to which reference is made in this statement were the majority and minority reports prepared in the GAC meeting which ended October 30, 1949 and the report

of the Chairman, GAC addressed to the Chairman, AEC on this meeting. In addition there was a report prepared by myself as Secretary and directed to the Chairman, GAC. This report was prepared in lieu of minutes for the purpose of setting forth the Secretary's impressions of the discussion of the GAC which led to the committee's documents, in order to provide additional background for interpretation of these documents. Since Dr. Oppenheimer, Dr. Fermi and Dr. Smith were in Washington on November 7, they were consulted on the draft of my report and minor changes were made to represent their views with more correct emphasis. This report was completed and given to the Chairman GAC on November 9th.

"E. The meeting with Senator McMahon for which the 'distribution' of reports as described above was made, took place at Los Alamos November 15, 1949. The purpose of the meeting was to review the Los Alamos program including work on thermo-nuclear weapons. It was not for policy discussion concerning the thermo-nuclear program.

From these items of fact it is clear that (a) revelation of these particular reports was authorized by the AEC in the person of the General Manager that the laboratories at Argonne, Berkeley and Los Alamos be made aware of the GAC recommendations, (b) that the showing of the reports to members of the Tech Board was on the responsibility of Dr.

Bradbury and myself in preparation for discussion with Senator McMahon who had seen them, (c) that the handling of the documents was in accord with established procedures and (d) that Dr. Oppenheimer had nothing to do whatever with this matter.

The discussion as to relative concentration on fission weapons and thermo-nuclear weapons had been a continuing one since 1942. It was recognized that the fission weapon would have to be made before the thermo-nuclear weapon would be possible. But even at the beginning it made an obvious difference in the program whether one were pointing toward a fission weapon, which should itself be used as the primary atomic weapon, or whether one were planning to make a thermo-nuclear weapon. There was also the question of whether it was better, as a military matter to improve and make larger numbers of fission weapons or to devote major time and effort to establish the possibility and practicality of some thermo-nuclear weapon. Wholly apart from the question of whether it would be technically possible to make a thermo-nuclear weapon, it was clear that the making of thermo-nuclear weapons would require the use of the same materials and personnel and money that might otherwise be devoted to making of improved fission weapons. In short, it would be a task comparable with the wartime development of the fission weapon. It was a matter of judgment as to the best way to

utilize the materials, personnel and money as between the fission weapons program and the thermo-nuclear weapons program.

"One of the difficulties that all concerned felt keenly in the effort to make up their minds on this question was that they did not have any really adequate appraisals of the military usefulness of the different weapons, nor were such appraisals supplied by the military.

"It is my impression that the GAC labored under the same difficulties as others on this problem, but that the GAC was certainly as active as any other group with respect to this problem. The GAC, and particularly Dr. Fermi, made an effort to evaluate the relative costs in terms of production facilities of the two types of weapons. It was not a military evaluation of worth.

"I normally attended meetings of the GAC, and it was my observation that Dr. Oppenheimer as chairman took pains on all questions to sound out the views of the other members of the Committee before expressing his own. It was my impression that he did this because he was keenly conscious of the restraints of the chairmanship. It is my recollection that this was the way he conducted the October 1949 meetings that discussed the thermo-nuclear weapons program. The matter of annexing both a majority and minority report to the report of the October 1949 meeting was, as I recall it, at

Dr. Oppenheimer's suggestion and instruction because he wanted to be sure that the report fully reflected the views of all members of the committee. It was in the same spirit that he requested me to prepare a report on the meeting as a supplement to his report and those of the majority and minority.

"I find the suggestion that Dr. Oppenheimer attempted to or did retard the work of the Los Alamos Laboratory in any field, and specifically in the field of thermo-nuclear weapons, preposterous and without foundation. I had no feeling whatever that anybody at Los Alamos was holding back in effort on the thermo-nuclear weapon because of Dr. Oppenheimer's suggestion or example. (Indeed, I had no feeling that anyone was holding back on the work on thermo-nuclear weapons once the President had decided the question by his announcement in January 1950. The work proceeded with willingness and cooperation from all concerned.) I know of my own knowledge that Dr. Oppenheimer never suggested to me that I should refrain from working on the thermo-nuclear weapons program, or that I should go slow on it or anything like that.

"I never observed anything to suggest that Dr. Oppenheimer opposed the thermo-nuclear weapons project after it was determined as a matter of national policy to proceed with the development of thermo-nuclear weapons, or that he failed to cooperate fully in the project to the extent that

some one who is not actively working could cooperate. I do not recall anything in his subsequent conduct of the GAC meetings that suggested to me in the slightest that he was doing anything less than wholeheartedly cooperating. Neither have I ever heard from any scientists that Dr. Oppenheimer was instrumental in persuading that scientist not to work on the thermo-nuclear weapons project.

"I have known Dr. Oppenheimer now since 1942. Until 1951 I worked very intimately and closely with him. I feel that I know him very well indeed. I consider that the work that he has done has been of the greatest possible value to the country; that if comparisons must be made, his contribution has probably been of more importance in the development of the atomic energy program than that of any other scientist in the country and perhaps than that of any other person in the country. I make this statement not only in recognition of the great contribution he made while he was director of the Los Alamos Laboratory, but also from my familiarity with his activities as chairman of the GAC. He took an active part in the many complex problems of the whole atomic energy program. Its achievements are, I think, due in no small part to his activities. He has at all times had the national interest at heart and has never done anything that he thought or suspected might be contrary to the national interest.

"I am absolutely clear that he is in no sense whatever a security risk. I say this both on the basis (a) of the fact that for over 10 years he was entrusted with the most secret information pertaining to the nation's atomic developments, and there was never the slightest leakage of secret information from or through him, or in any way related to him, and (b) on the basis of my intimate personal knowledge of him, his character and his views.

"My attention has been called to the fact that the letter of December 23, 1953 from the Atomic Energy Commission suspending Dr. Oppenheimer's clearance mentioned his having known someone named 'Jack Manley'. I suppose I should record the fact that I assume that I am not the 'Jack Manley' referred to because the letter refers to 'Jack Manley' as a member or official of the Communist Party, and I have never been associated with the Communist Party. I do not recall that I have ever been known as 'Jack Manley'. I do not know who Jack Manley is, nor do I know anyone of that name.

"John H. Manley,

"Sworn to before me this 16th day of February 1954  
Mary E. Mossman, Notary Public."

BY MR. GARRISON:

Q Do you wish to make any comment on that affidavit or does the Board wish to ask any questions of Dr. Oppenheimer relating to it?

MR. GRAY: I am sure my question would be one, which Dr. Oppenheimer could not answer because it relates to the statement of Dr. Manley. I don't know what the significance of this is, but I would read this statement in parentheses on page 10. I don't take it that this refers to Dr. Oppenheimer, but in general it says, "Indeed, I had no feeling anyone was holding back on the work on thermo-nuclear weapons once the President decided the question". I get from that, it seems to me, the inference that there were those who were holding back. I repeat that does not refer to Dr. Oppenheimer in his language, but it seems to me that is a carefully worded observation. This is a reaction to it, however.

THE WITNESS: Do you want to put a question to me about it. I will hazard an interpretation.

MR. GRAY: Yes.

THE WITNESS: The research calculations and experiments that were in course at Los Alamos would not be held back; they would be accelerated because there was a chance of going all out. Some arrangements of an engineering kind, of a production kind, of an administrative kind, you would make if you knew you were trying to make this thing as fast as possible, but you would plan for but not make if you were uncertain as to that.

An example may be the Savannah River plants.

Thinking began on them -- should have begun earlier -- but certainly began on them once the question was raised. The actual letting of the contract for design drawings and so on would presumably have waited the presidential decision. I suppose it is this kind of thing. There was not any retardation compared to what went before. It was a failure to accelerate in those things which involved the commitment of funds.

MR. GARRISON: Mr. Chairman, suppose we get in touch with Dr. Manley and either have a supplementary affidavit or ask him to come on. I think that is going to be a little awkward.

MR. GRAY: May I not at this time, but later consult with the counsel for the Board on this point and perhaps we could pass on. I don't think it is fair to ask Dr. Oppenheimer to interpret what Dr. Manley had in his mind.

MR. GARRISON: I agree with you.

MR. SILVERMAN: May we go off the record for one moment?

MR. GRAY: Yes.

(Discussion off the record.)

MR. GRAY: Suppose we proceed, and if we wish anything further, I will let you know, Mr. Garrison.

THE WITNESS: I have three other items of national service. As far as I know, they are not controversial. I will

outline them briefly.

In late 1950 and early 1951, Mr. William Golden was asked by the President's office to explore the question, "Is the mobilization of scientists adequate". There was much talk during the Korean crisis of recruiting an emergency office like the Office of Research and Development. He talked with a lot of people, including me. I recommended that there be an advisory group to the National Security Council, if the National Secretary Council and the President wanted it, on technical matters, and there be standby plans for all out mobilization. But in view of the immense expansion of research and development in the Department of Defense, an emergency organization like Dr. Bush's in the last war would just not fit into anything.

After reflection, Golden persuaded his superiors that there should be an advisory committee. It was attached in a rather peculiar way to the Office of Defense Mobilization and the invitations to join it suggested that this committee would be advisory to the Director of Defense Mobilization, the then Mr. Charles Wilson and the President. The chairman of the committee was Oliver Buckley. You have a list of its members.

MR. GARRISON: It is Item 7 on the second page of II of the biographical sheet.

THE WITNESS: During approximately that first year,

the committee met from time to time. It was seldom asked for advice. Dr. Buckley did a great many useful liaison jobs. We proffered very little advice. I think that our only function, perhaps, was to keep some balance between the needs for basic and universal research and training on the one hand and defense research and development on the other.

Dr. Buckley resigned because of ill health and was replaced by Dr. DuBridge, who became chairman in 1952. I don't remember the date.

In the autumn of 1952, we had a two or three day meeting -- probably two days -- at Princeton of this full committee, to see whether we had any suggestions to pass on to the new administration as to the mobilization of science. I think we concluded that we had been of no great use and that as constituted and conceived we should be dissolved.

We suggested some changes in research and development in the Defense Department, and they are pretty close, I think, to what has taken place in the reorganization of the summer of 1953. We also said that somehow or other the Security Council might need and should certainly have available to it technical advice of the highest order, and must have access to the whole community of scientists so that if anything they wanted to know that was relevant to their deliberations, it might be available.

We said in that framework it is conceivable that

another committee might be useful. We scribbled these things down on a piece of paper, and DuBridge was supposed to see that they somehow got to President Elect Eisenhower. The President Elect had a lot of other things to do and we went together, DuBridge and I, to Nelson Rockefeller, who had been put in charge of a committee to suggest the reorganization of the executive branch of the government. We talked a good bit about our good for nothing committee, handed him this memorandum, and he reported to me and DuBridge that they discussed it in the committee and gave it to the President, and thought it made sense. We thought we were dead. We were, but not quite.

In the spring of 1953, I think at the request of Mr. Flemming and Mr. Cutler, we were reactivated and asked to convene. We met several times. The principal problems put before us were the proper use of scientific manpower, the very controversial and tough problem of continental defense, where there were several technical things that we were asked to look into and advise on and report on and I think some other problems, but since I don't have records of the committee I can't detail them.

The last meeting I attended was just before I left for Europe and not very long before my clearance was suspended, and our Principal job there was to make sure that the Council and its staff knew of technical advances which

were useful in early warning and in radar generally and that they understood that some of the arguments against the feasibility of early warning were obsolete, because of discoveries that had been made in the meantime.

I have no further testimony on this committee.

BY MR. GARRISON:

Q Then we come to certain studies of defense that you made or engaged in -- defense against atomic warfare -- perhaps you can say a word to the Board about them.

A Yes. This can be fairly brief.

The Department of Defense adopted during the Korean crisis a practice of letting our large segments of the defense problem as study projects to a university. The university would then call in competent people from the rest of the country. I have referred to Project Vista as one such. There was one under contract I think only with the Air Force at MIT. Its code name was Charles. Its purpose was to have a look at air defense. I had the faintest connection with this. I believe I was present at some of the briefings. It led to the establishment of the Lincoln Laboratory, which is a very large radar and air defense laboratory operated by MIT for the Air Force.

Another such study which I had suggested was set up through the Army and the NSRB, I guess, and that was to have a look at civil defense -- a very tough and unstudied

problem, really. I was not very active. I was on the advisory council or the policy council, but I met rather rarely. I did give one or two briefings and I talked with General Nelson about the problems of writing an effective report. There were a great many recommendations, many of them have been made public. I think those which attracted the greatest attention were that if civil defense was to be manageable at all, early warning and improved military interception, improved over what we then had or were planning, were an essential part of making civil defense manageable. With these conclusions I concurred.

The third item here is that largely growing out of the work of some people on East River, and in particular Dr. Berkner and Dr. Rabi, there came a conviction not only that one had to have a better continental defense, but quite a lot could be done about it.

I was consulted about the wisdom of it, and I agreed to hold a study during the summer of 1952, two months of intensive study, at the Lincoln Laboratory, which would concern itself with both an evaluation of the prospects of continental defense and recommendations of how to get on with the job.

The Lincoln Laboratory was working very hard and very effectively on some aspect of this problem. The notion of the summer study was to look at parts that had not been

adequately dealt with.

I attended the first week and I think the last week of meetings there. Radar is not the subject of my expertness.

The emphasis in the discussions I attended and the final briefings were the moving of the early far north -- supplementing the early warning by a far northern line, the extension of the early warning by seaward flanks.

There was a good deal of argument about interception and what kind of missiles would be used. Whether one could use atomic weapons for that purpose.

There was certainly a great deal of discussion about the gravity of the problem and a great deal of discussion about the two way relations between the Strategic Air Command and the continental defense. On the one hand the early warning, giving the Strategic Air Command a chance, and on the other hand the Strategic Air Command playing an essential part in reducing the severity of the attack by going after enemy bases.

The only part of the work that seemed to me undoubtedly successful were the proposals for early warning, the technical proposals about the equipment and the general schemes about the location of the line and their extension. I regarded and don't know too much about the problems of interception and kill as fairly much unresolved at the end of the study.

These things came back, as I have said, to the

Science Advisory Committee, and we picked up the recommendations there and did our best to explain them.

These almost all have to do with early warning. I believe that I have read in the papers that many steps have been taken to improve the situation. I think it is a very important contribution not to the security but to the deterrent value of our own offensive striking power and a deterrent to attack, at least during the period of limited enemy capability.

Those are the three projects.

The final assignment -- and I assure you it is final -- was of a somewhat different kind. In the spring of 1952, I had a letter from the Secretary of State appointing me or asking me whether I would serve as a member of a panel. The other members of the panel were Allen Dulles, John Dickey, Vannevar Bush and Joe Johnson. The letter appointing us said that it seemed to be time that the delegate who was then Benjamin Cohen, who was representing us in the Disarmament Conference, would like to advise and even more the people in the State Department who were responsible for our policy with regard to the regulation of armaments. We all went to a meeting with the Secretary of State, people of Defense -- it was a great big meeting -- somewhat puzzled as to whether there was any reality to the job we had been asked to assume, but willing at least to listen.

At the meeting it was made clear by the Secretary that he would like any report, any study of the regulation of armaments -- was it a feasible goal, was there any way to go about it, were there any tricks to it, similar to the Acheson-Lilienthal Report of many years before, could armaments be regulated, and he would like us to help the people who were working diplomatically in this field. But he thought in addition that we ought to see whether we did not have something to say and get it written down.

MR. GARRISON: This is Item 8 of the memorandum.

MR. GRAY: Yes.

THE WITNESS: As to the consultations they took place. I saw something of Mr. Cohen and maybe helped in some minor ways, and I think others did. We also talked with people in the Department of State. But there was clearly not much reality to the discussions of disarmament in the United Nations and the most we could do was make a few helpful suggestions which would encourage our friends as to our good faith and interest.

It took a long while for the members of the panel to get cleared. But that happened some time during the summer. We got George Brundy to be our secretary, who is now Dean of Harvard College, but was then professor of political science there. We had a look at what we had been asked to look at. We went over the studies of past efforts of

disarmament. Mr. Dulles remembered them very vividly. It was very clear that you could not negotiate with the Russians much about anything, and that nothing was harder to negotiate about than disarmament, and if you put these two things it just was the bleakest picture in the world of getting anything effective down that line.

We took a look at the armament situation, getting some estimates of the growth of Russian capability and some estimates of our own as a measure for where they might be some time in the future. I think as always we thought we were being careful, but we were a little too conservative in estimating the speed and success of the Soviet program. We became very vividly and painfully aware of what an unregulated arms race would lead to in the course of years. We tended to think in the course of five or ten years, but probably the time was shorter.

Our report was of course classified. We filed it in January of 1953. It had five recommendations, of which two, I think I should not talk about because they had to do with the conduct of our diplomatic affairs and should be regarded as secret. They are not very ingenious.

The other three I embodied in an article that I published in Foreign Affairs. Before publishing it, I took it to the President. He showed it to Mr. Cutler. Mr. Cutler had no objection to my publication. He thought my publication

would be helpful and encouraged me to go ahead with it.

These three were that the people of this country be given a better understanding of the dangers of the atomic arms race, that we attempt either through administrative practice or through revised legislation to work more closely with our allies on problems having to do with the offensive and defensive aspects of large weapons, and three, that we take further measures for continental defense as a supplement to our striking capability.

I was asked to report on these three things before the Jackson Committee, think it was on psychological strategy and so did rather briefly, and I was asked to report on these more or less as an advocate before the National Security Council, asked by the President, and I went to do that. At that time Dr. Bush and Commissioner Dean went with me. I presented the arguments, which I think are in Foreign Affairs, and which are still persuasive to me, in favor of these three steps.

I did mention the diplomatic points at the Security Council, because that was of course not a public meeting.

That brings me to the end of this fairly long spiel I have given you about my connection with the United States Government.

MR. GRAY: Just one question. What was the date of that Foreign Affairs article?

THE WITNESS: It was published in the July issue of 1953. It actually came out a little earlier, in June or something like that.

MR. ECKER: That was submitted to you.

MR. GRAY: I was not sure that was the same one.

THE WITNESS: There are two.

MR. ECKER: Yes.

(Discussion off the record.)

THE WITNESS: Might I put one more statement into the record on my conduct as a part time public servant during these years.

Of course, these things were secret. They were not subject to the scrutiny of the press, and they were not generally open, but they were not secret in the sense that the people did not know what we were up to. We were constantly testifying before Congressional committees, we were writing reports which were very widely circulated. We were under, I would say, a very intensive searchlight of scrutiny. We were always in a position where our advice could be countered, could be overruled or could be accepted. There was no opportunity for conspiracy in these things because the light of criticism was constantly shining on them.

MR. GARRISON: Mr. Chairman, the first letter I should like to introduce into the record is from Gordon Dean, Chairman of the Atomic Energy Commission, to Dr. Oppenheimer

dated June 14, 1952.

"Dear Mr. Oppenheimer:

"I want to express my personal thanks to you for our talk of yesterday concerning the General Advisory Committee and its role as an advisory group to the Commission. It was most helpful.

"I want you to know that I fully appreciate the reasons behind your unwillingness to have your name considered for reappointment to the GAC. I would not have been quite so prepared for this had you not so long ago advised me of your intention to pass the baton on to another.

"It is impossible for me to magnify the contribution which, as Chairman of this distinguished group, you have made to the Commission and the country. It has been a magnificent one and we of the Commission will be forever grateful to you. The period covered by your chairmanship has been one in which this new agency needed very much the wisest possible guidance. This we have received and no one knows this better than myself.

"I am quite aware that there is no one who can adequately take your place, but your willingness to remain as a consultant to the Commission somewhat softens the blow of your departure from the GAC councils.

"With every good wish, Sincerely, Gordon Dean,  
Chairman.

"Mr. J. Robert Oppenheimer, Institute for Advanced

Study, Princeton, New Jersey."

The second letter is signed Harry Truman, The White House, Washington, D. C., September 27, 1952:

"Dear Dr. Oppenheimer:

"Having in mind your strong desire, which you expressed to me last month, to complete your service on the General Advisory Committee to the Atomic Energy Commission with the expiration of your present term, I note with a deep sense of personal regret that this time is now upon us.

"As Chairman of this important Committee since its inception, you may take great pride in the fact that you have made a lasting and immensely valuable contribution to the national security and to atomic energy progress in this nation. It is a source of real regret to me that the full story of the remarkable progress that has been made in atomic energy during these past six years, and in which you have played so large a role, cannot be publicly disclosed, for it would serve as the finest possible tribute to the contribution you have made.

"I shall always be personally grateful for the time and energy you have so unselfishly devoted to the work of the General Advisory Committee; for the conscientious and rewarding way in which you have brought your great talents to bear upon the scientific problems of atomic energy development, and for the notable part you have played in securing for the

atomic energy program the understanding cooperation of the scientific community.

"As Director of the Los Alamos Scientific Laboratory during World War II, and as Chairman of the General Advisory Committee for the past six years, you have served your country long and well, and I am gratified by the knowledge that your wise counsel will continue to be available to the Atomic Energy Commission on a consultant basis.

"I wish you every future success in your important scientific endeavors.

"Very sincerely yours, Harry Truman.

"Dr. J. R. Oppenheimer.

"Director

"The Institute for Advanced Study

"Princeton, New Jersey."

And the final letter is another one from Gordon Dean dated October 15, 1952.

"Dear Oppy:

"I cannot let your departure from the General Advisory Committee go by without expressing again my deep appreciation for the time and talent which you have so generously devoted to the work of the Committee, and for the immensely valuable contribution you have made to the atomic energy program during the period I have been associated with it and before.

"I know that you are as fully aware as I am of the assistance the General Advisory Committee has given to the Commission during these past six, formative years, and of the great scientific and technical strides that have been made in that time. I sincerely hope that some day, when the ills of the world are sufficiently diminished, the complete story of this progress can be told, so that the contribution of you and your colleagues may find its rightful place in the chronicle of our times.

"May I say that I shall always be grateful for your past work on behalf of the program, and for your willingness to continue to advise the Commission on a consultative basis.

"With every good wish, Sincerely, Gordon Dean, Chairman

"Dr. J. Robert Oppenheimer

"Institute for Advanced Study.

"Princeton, N. J."

There are, Mr. Chairman, several exhibits that I would like to introduce at this time having to do with Dr. Oppenheimer's views on the freedom of the mind and the human spirit. I introduce them to show a position which I think could not be tolerated for one moment behind the Iron Curtain.

MR. GRAY: These are to be exhibits?

MR. GARRISON: These will be extracts from original documents which I will hand the Board. One is taken from a

lecture, which one of the three was it, Dr. Oppenheimer, you gave?

THE WITNESS: No, there were six. This is the last one.

MR. GARRISON: Do you want to tell the Board in one minute what those lectures were?

THE WITNESS: Gladly. I was invited a year ago and then again this year to give lectures in England. They are named in honor of Lord Reith. They are on the home program and there is really a large audience, 15 million or something. They are meant to be quite serious. I think the first lectures were given by Russell, called "Authority and the Individual." I called mine "Science and a Common Understanding. I talked about it -- I won't summarize them. That is irrelevant. The principal point was to indicate in what ways contemporary science left room for an integrated human community. Why it was not necessary specialized knowledge led to fragmentation in society. That was about it. The last lecture has something about that in it.

MR. GRAY: My question is whether these are offered as exhibits. We have a couple of earlier documents.

MR. GARRISON: I would like to treat these as the others, to have them available for the inspection of the Board, so you may look at them in the whole.

MR. ROBB: Are those the lectures published in a

publication called "The Listener"?

THE WITNESS: Yes.

MR. ROBB: We have those.

MR. GARRISON: The one I shall read into the record is a very short excerpt from a speech given to the University of Denver by Dr. Oppenheimer February 6, 1947. It is page 8 of the small reprint which I just handed to you. It reads as follows:

"And above all, I think, there stands the great conflict with Soviet communism. There may be people who believe that this (system)" -- the insertion is our own for clarity -- "originated in a desire to provide for the well-being of the people of Russia. . . But whatever its origin, it has given rise to political forms which are deeply abhorrent to us and which we not only would repudiate for ourselves but which we are reluctant to see spread into the many areas of the world where there is great lability. . ."

That word is "lability" and I understand it means flexibility.

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

MR. GARRISON: Of course, Mr. Chairman, it is quite obvious -- there is no mystery about these excerpts -- I have quite plainly selected those which seemed to me relevant and that bore upon Dr. Oppenheimer's attitude toward the problem of our relation with Russia. They don't attempt

therefore to be comprehensive excerpts of the whole speech but simply of those items which seem to me are utterly inconsistent with the notion that Dr. Oppenheimer could be, as depicted in the Commission's letter.

The next excerpt from the Reith Lectures in "The Listener", pages 1076 and 1077:

"It is true that none of us will know very much; and most of us will see the end of our days without understanding in all its detail and beauty the wonders uncovered even in a single branch of a single science. Most of us will not even know, as a member of any intimate circle, anyone who has such knowledge; but it is also true that, although we are sure not to know everything and rather likely not to know very much, we can know anything that is known to man, and may, with luck and sweat, even find out somethings that have not before been known to him. This possibility, which, as a universal condition of man's life is new, represents today a high and determined hope, not yet a reality; it is for us in England and in the United States not wholly remote or unfamiliar. It is one of the manifestations of our belief in equality, that belief which could perhaps better be described as a commitment to unparalleled diversity and unevenness in the distribution of attainments, knowledge, talent and power.

"This open access to knowledge, these unlocked doors and signs of welcome, are a mark of a freedom as fundamental

as any. They give a freedom to resolve difference by converse, and, where converse does not unite, to let tolerance compose diversity. This would appear to be a freedom barely compatible with modern political tyranny. The multitude of communities, the free association for converse or for common purpose, are acts of creation. It is not merely that without them the individual is the poorer; without them a part of human life, not more nor less fundamental than the individual, is foreclosed. It is a cruel and humorless sort of pun that so powerful a present form of modern tyranny should call itself by the very name of a belief in community, by a word 'communism' which in other times evoked memories of villages and village inns and of artisans concerting their skills, and of men of learning content with anonymity. But perhaps only a malignant end can follow the systematic belief that all communities are one community; that all truth is one truth; that all experience is compatible with all other; that total knowledge is possible; that all that is potential can exist as actual. This is not man's fate; this is not his path; to force him on it makes him resemble not that divine image of the all-knowing and all-powerful but the helpless, iron-bound prisoner of a dying world. The open society, the unrestricted access to knowledge, the unplanned and uninhibited association of men for its furtherance -- these are what may make a vast, complex,

ever-growing, ever-changing, ever more specialized and expert technological world nevertheless a world of human community."

MR. GRAY: It is now I think 4:20. I wonder if there are any other exhibits. If not, this would seem to be a good breaking point.

MR. GARRISON: Yes, I think so.

MR. GRAY: Unless counsel for the Board has something to say, we will then recess and meet again at 9:30 tomorrow morning.

(Thereupon at 4:20 p.m., a recess was taken until Wednesday, April 14, 1954, at 9:30 a.m.)