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UNITED STATES ATOMIC ENERGY COMMISSION

VOLUME I

In the Matter Of:

J. ROBERT OPPENHEIMER

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Place - Washington, D. C.

Date - April 12, 1954

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UNITED STATES ATOMIC ENERGY COMMISSION

PERSONNEL SECURITY BOARD

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In the Matter of :

J. ROBERT OPPENHEIM :

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Room 2022,
Atomic Energy Commission,
Building T-3,
Washington, D. C.

The above entitled matter came on for hearing before the Board, pursuant to notice, at 10:00 o'clock a.m.

PERSONNEL SECURITY BOARD:

DR. 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.

I N D E X

EXHIBITS	RECEIVED
Oppenheim No. 1	77

WITNESS:	DIRECT	CROSS
J. ROBERT OPPENHEIMER	83	

P R O E C E E D I N G S

DR. GRAY: I should like at this time to have the reporters sworn. For the information of Dr. Oppenheimer and his counsel, the reporter is Anton Papich, Jr., the transcriber Kenneth V. Bowers.

(The reporter and transcriber were thereupon duly sworn by Dr. Gray.)

DR. GRAY: The hearing will come to order.

This Board, appointed by Mr. K. D. Nichols, General Manager of the Atomic Energy Commission at the request of Dr. J. Robert Oppenheimer, is composed of the following members: Gordon Gray, Chairman, Ward V. Evans and Thomas A. Morgan. All members of the Board are present, and Board counse Roger Robb and C.A.Rolander. Dr..and Mrs. Oppenheimer are present. Present also are Mr. Lloyd K. Garrison, counsel for Dr. Oppenheimer. Would you identify your associates?

MR. GARRISON: Samuel J. Silverman, my partner, and Allen B. Ecker, associate of my firm.

DR. GRAY: An investigation of Dr. J. Robert Oppenheim conducted under the provisions of section 10(b)(5)(B)(i - iii) of the Atomic Energy Act of 1946 has revealed certain information which casts doubt upon the eligibility of Dr. Oppenheimer for clearance for access to restricted data as provided by the Atomic Energy Act of 1946. This information is as follows:

This is a letter addressed to Dr. J. R. Oppenheimer, the Institute for Advanced Study, Princeton, New Jersey, dated December 23, 1953, reading as follows:

"Dear Dr. Oppenheimer:

"Section 10 of the Atomic Energy Act of 1946 places upon the Atomic Energy Commission the responsibility for assuring that individuals are employed by the Commission only when such employment will not endanger the common defense and security. In addition, Executive Order 10450 of April 27, 1953, requires the suspension of employment of any individual where there exists information indicating that his employment may not be clearly consistent with the interests of the national security.

"As a result of additional investigation as to your character, associations and loyalty, and review of your personnel security file in the light of the requirements of the Atomic Energy Act and the requirements of Executive Order 10450, there has developed considerable question whether your continued employment on Atomic Energy Commission work will endanger the common defense and security and whether such continued employment is clearly consistent with the interests of the national security. This letter is to advise you of the steps which you may take to assist in the resolution of this question.

"The substance of the information which raises the

question concerning your eligibility for employment on Atomic Energy Commission work is as follows:"

Let the record show at this point that Mr. Garrison asked to be excused for a few minutes.

"It was reported that in 1940 you were listed as a sponsor of the Friends of the Chinese People, an organization which was characterized in 1944 by the House Committee on Un-American Activities as a Communist front organization. It was further reported that in 1940 your name was included on a letterhead of the American Committee for Democratic and Intellectual Freedom as a member of its National Executive Committee. The American Committee for Democracy and Intellectual Freedom was characterized in 1942 by the House Committee on Un-American Activities as a Communist front which defended Communist teachers, and in 1943 it was characterized as subversive and un-American by a Special Subcommittee of the House Committee on Appropriations. It was further reported that in 1938 you were a member of the Western Council of the Consumers Union. The Consumers Union was cited in 1944 by the House Committee on Un-American Activities as a Communist front headed by the Communist Arthur Kallet. It was further reported that you stated in 1943 that you were not a Communist, but had probably belonged to every Communist front organization on the west coast and had signed many petitions in which Communists were interested.

"It was reported that in 1943 and previously you were intimately associated with Dr. Jean Tatlock, a member of the Communist Party in San Francisco, and that Dr. Tatlock was partially responsible for your association with Communist front groups.

"It was reported that your wife, Katherine Puening Oppenheimer, was formerly the wife of Joseph Dallet, a member of the Communist Party, who was killed in Spain in 1937 fighting for the Spanish Republican Army. It was further reported that during the period of her association with Joseph Dallet, your wife became a member of the Communist Party. The Communist Party has been designated by the Attorney General as a subversive organization which seeks to alter the form of Government of the United States by unconstitutional means, within the purview of Executive Order 9835 and Executive Order 10450.

It was reported that your brother Frank Friedman Oppenheimer became a member of the Communist Party in 1936 and has served as a Party organizer and as Educational Director of the Professional Section of the Communist Party in Los Angeles County. It was further reported that your brother's wife, Jackie Oppenheimer, was a member of the Communist Party in 1938; and that in August, 1944, Jackie Oppenheimer assisted in the organization of the East Bay branch of the California Labor School. It was further

reported that in 1945 Frank and Jackie Oppenheimer were invited to aninformal reception at the Russian Consulate, that this invitation was extended by the American-Russian Institute of San Francisco and was forthe purpose of introducing famous American scientists to Russian scientists who were delegates to the United Nations Conference on International Organization being held at San Francisco at that time, and that Frank Oppenheimer accepted this invitation. It was further reported that Frank Oppenheimer agreed to give a six weeks course on "The Social Implications of Modern Scientific Development" at the California Labor School, beginning May 9, 1946. The American-Russian Institute of San Francisco and the California Labor School have been cited by the Attorney General as Communist organizations within the purview of Executive Order 9835 and Executive Order 10450.

"It was reported that you have associated with members and officials of the Communist Party including Isaac Folkoff, Steve Nelson, Rudy Lambert, Kenneth May, Jack Manley, and Thomas Addis.

"It was reported that you were a subscriber to the Daily People's World, a west coast Communist newspaper, in 1941 and 1942.

"It was reported in 1950 that you stated to an agent of the Federal Bureau of Investigation that you had in the past made contributions to Communist front organizations,

although at the time you did not know of Communist Party control or extent of infiltration of these groups. You further stated to an agent of the Federal Bureau of Investigation that some of these contributions were made through Isaac Folkoff, whom you knew to be a leading Communist Party functionary, because you had been told that this was the most effective and direct way of helping these groups.

"It was reported that you attended a housewarming party at the home of Kenneth and Ruth May on September 20, 1941, for which there was an admission charge for the benefit of The People's World, and that at this party you were in the company of Joseph W. Weinberg and Clarence Hiskey, who were alleged to be members of the Communist Party and to have engaged in espionage on behalf of the Soviet Union. It was further reported that you informed officials of the United States Department of Justice in 1952 that you had no recollection that you had attended such a party, but that since it would have been in character for you to have attended such a party, you would not deny that you were there.

"It was reported that you attended a closed meeting of the professional section of the Communist Party of Alameda County, California, which was held in the latter part of July or early August 1941, at your residence, 10 Kenilworth Court, Berkeley, California, for the purpose ofhearing an explanation of a change in Communist Party Policy. It was

reported that you denied that you attended such a meeting and that such a meeting was held in your home.

"It was reported that you stated to an agent of the Federal Bureau of Investigation in 1950, that you attended a meeting in 1940 or 1941, which may have taken place at the home of Haakon Chevalier, which was addressed by William Schneiderman, whom you knew to be a leading functionary of the Communist Party. In testimony in 1950 before the California State Senate Committee on Un-American Activities, Haakon Chevalier was identified as a member of the Communist Party in the San Francisco area in the early 1940's."

Let the record show that Mr. Garrison has returned to the hearing room.

"It was reported that you have consistently denied that you have ever been a member of the Communist Party. It was further reported that you stated to a representative of the Federal Bureau of Investigation in 1946 that you had a change of mind regarding the policies and politics of the Soviet Union about the time of the signing of the Soviet German Pact in 1939. It was further reported that during 1950 you stated to a representative of the Federal Bureau of Investigation that you had never attended a closed meeting of the Communist Party; and that at the time of the Russo-Finnish War and the subsequent break between Germany and Russia in 1941, you realized the Communist Party infiltration tactics

into the alleged anti-Fascist groups and became fed up with the whole thing and lost what little interest you had. It was further reported, however, that:

"(a) Prior to April, 1942, you had contributed $150 per month to the Communist Party in the San Francisco Area, and that the last such payment was apparently made in April 1942, immediately before your entry into the atomic bomb project.

"(b) During the period 1942-1945 various officials of the Communist Party, including Dr. Hannah Peters, organizer of the Professional Section of the Communist Party, Alameda County, California, Bernadette Doyle, secretary of the Alameda County Communist Party, Steve Nelson, David Adelson, Paul Pinsky, Jack Manley, and Katrina Sandow, are reported to have made statements indicating that you were then a member of the Communist Party; that you could not be active in the Party at that time; that your name should be removed from the Party mailing list and not mentioned in anyway; that you had talked the atomic bomb question over with Party members during this period; and that several years prior to 1945 you had told Steve Nelson that the Army was working on an atomic bomb.

"(c) You stated in August of 1943 that you did not want anybody working for you on the Project who was a member of the Communist Party, since "one always had a

question of divided loyalty" and the discipline of the Communist Party was very severe and not compatible with complet loyalty to the Project. You further stated at that time that you were referring only to present membership in the Communist Party and not to people who had been members of the Party. You stated further that you knew several individuals then at Los Alamos who had been members of the Communist Party. You did not, however, identify such former members of the Communist Party to the appropriate authorities. It was also reported that during the period 1942-1945 you were responsible for the employment on the atom bomb Project of individuals who were members of the Communist Party or closely associated with activities of the Communist Party, including Giovanni Rossi Lomanitz, Joseph W. Weinberg, David Bohm, Max Bernard Friedman, and David Hawkins. In the case of Giovanni Rossi Lomanitz, you urged him to work on the Project, although you stated that you knew he had been very much of a "Red" when he first came to the University of California and that you emphasized to him that he must forego all political activity if he came to the Project. In August, 1943, you protested against the termination of his deferment and requested that he be returned to the Project after his entry into the military service.

"It was reported that you stated to representatives of the Federal Bureau of Investigation on September 5, 1946,

that you had attended a meeting in the East Bay and a meeting in San Francisco at which there were present persons definitely identified with the Communist Party. When asked the purpose of the East Bay meeting and the identity of those in attendance you declined to answer on the ground that this had no bearing on the matter ofinterest being discussed.

"It was reported that you attended a meeting at the home of Frank Oppenheimer on January 1, 1946, with David Adelson and Paul Pinsky, both of whom were members of the Communist Party. It was further reported that you analyzed some material which Pinsky hoped to take up with the Legislative Convention in Sacramento, California.

"It was reported in 1946 that you were listed as Vice Chairman on the letterhead of the Independent Citizens Committee of the Arts, Sciences, and Professions, Inc., which has been cited as a Communist front by the House Committee on Un-American Activities.

"It was reported that prior to March 1, 1943, possibly three months prior, Peter Ivanov, Secretary of the Soviet Consulate, San Francisco, approached George Charles Eltenton for the purpose of obtaining information regarding work being done at the Radiation Laboratory for the use of Soviet scientists; that George Charles Eltenton subsequently requested Haakon Chevalier to approach you concerning this matter; that Haakon Chevalier thereupon approached you,

either directly or through your brother, Frank Friedman Oppenheimer, in connection with this matter; and that Haakon Chevalier finally advised George Charles Eltenton that there was no chance whatsoever of obtaining the information. It was further reported that you did not report this episode to the appropriate authorities until several months after its occurrence; that when you initially discussed this matter with the appropriate authorities on August 26, 1943, you did not identify yourself as the person who had been approached, and you refused to identify Haakon Chevalier as the individual who made the approach on behalf of George Charles Eltenton; and that it was not until several months later, when you were ordered by a superior to do so, that you so identified Haakon Chevalier. It was further reported that upon your return to Berkeley following your separation from the Los Alamos Project, you were visited by the Chevaliers on several occasions; and that your wife was in contact with Haakon and Barbara Chevalier in 1946 and 1947.

"It was reported that in 1945 you expressed the view that "there is a reasonable possibility that it (the hydrogen bomb) can be made," but that the feasibility of the hydrogen bomb did not appear, on theoretical grounds, as certain as the fission bomb appeared certain, on theoretical grounds, when the Los Alamos Laboratory was started; and that in the Autumn of 1949 the General Advisory Committee

expressed the view that "an imaginative and concerted attack on the problem has a better than even chance of producing the weapon within five years". It was further reported that in the Autumn of 1949, and subsequently, you strongly opposed the development of the hydrogen bomb; (1) on moral grounds, (2) by claiming that it was not feasible, (3) by claiming that there were insufficient facilities and scientific personnel to carry on the development, and (4) that it was not politically desirable. It was further reported that even after it was determined, as a matter of national policy, to proceed with development of a hydrogen bomb, you continued to oppose the project and declined to cooperate fully in the project. It was further reported that you departed from your proper role as an advisor to the Commission by causing the distributi separately and in private, to top personnel at Los Alamos of the majority and minority reports of the General Advisory Committee on development of the hydrogen bomb for the purpose of trying to turn such top personnel against the development of the hydrogen bomb. It was further reported that you were instrumental in persuading other outstanding scientists not to work on the hydrogen bomb project, and that the opposition to the hydrogen bomb, of which you are the most experienced, most powerful, and most effective member, has definitely slowed down its development.

"In view of your adcess to highly sensitive

classified information, and in view of these allegations which, until disproved, raise questions as to your veracity, conduct and even your loyalty, the Commission has no other recourse, in discharge of its obligations to protect the common defense and security, but to suspend your clearance until the matter has been resolved. Accordingly, your employment on Atomic Energy Commission work and your eligibilit for access to Restricted Data are hereby suspended, effective immediately, pending final determination of this matter.

"To assist in the resolution of this matter, you have the privilege of appearing before an Atomic Energy Commission Personnel Security Board. To avail yourself of the privileges affordedyou under the Atomic Energy Commission hearing procedures, you must, within thirty days following receipt of this letter, submit to me, in writing, your reply to the information outlined above and request the opportunity of appearing before the Personnel Security Board. Should you signify your desire to appear before the Board, you will be notified of the composition of the Board and may challenge any member of it for cause. Such challenge should be submitted within seventy two hours of the receipt of notice of composition of the Board.

"If no challenge is raised as to the members of the Board, you will be notified of the date and place of hearing at least forty-eight hours in advance of the date

set for hearing. You may be present for the duration of the hearing, may be represented by counsel of your own choosing, and present evidence in your own behalf through witnesses, or by documents, or by both.

"Should you elect to have a hearing of your case by the Personnel Security Board, the findings of the Board, together with its recommendations regarding your eligibility for employment on Atomic Energy Commission work, in the light of Criteria for Determining Eligibility for Atomic Energy Commission Security Clearance and the requirements of Executive Order 10450, will be submitted to me.

"In the event of an adverse decision in your case by the Personnel Security Board, you will have an opportunity to review the record made during your appearance before the Board and to request a review of your case by the Commission's Personnel Security Review Board.

"If a written response is not received from you within thirty days it will be assumed that you do not wish to submit any explanation for further consideration. In that event, or should you not advise me in writing of your desire to appear before the Personnel Security Board, a determination in your case will be made by me on the basis of the existing record.

"I am enclosing herewith, for your information and guidance, copies of the Criteria and Procedures for Determining

Eligibility for Atomic Energy Commission Security Clearance and Executive Order 10450.

"This letter has been marked 'Confidential' to maintain the privacy of this matter between you and the Atomic Energy Commission. You are not precluded from making use of this letter as you may consider appropriate.

"I have instructed Mr. William Mitchell, whose address is 1901 Constitution Avanue, N. W., Washington, D. C. and whose telephone number is Sterling 3-8000, Extension 277, to give you whatever further detailed information you may desire with respect to the procedures to be followed in this matter.

"Very truly yours, K. D. Nichols, General Manager.

"2 Enclosures. 1. Criteria & Procedures.
2. Executive Order 10450."

I think at this time, then, it would be appropriate for the record to reflect Dr. Oppenheimer's reply of March 4, 1954. I shall now read Dr. Oppenheimer's reply.

This is a letter addressed to Major General K. D. Nichols, General Manager, U. S. Atomic Energy Commission, Washington 25, D. C.

"Dear General Nichols:

"This is in answer to your letter of December 23, 1953, in which the question is raised whether my continued employment as a Consultant on Atomic Energy Commission work

"will endanger the common defense and security and whether such continued employment is clearly consistent with the interests ofthe national security.'

"Though of oouuse I would have no desire to retain an advisory position if my advice were not needed, I cannot ignore the question you have raised, nor accept the suggestion that I am unfit for public service.

"The items of so-called 'derogatory information' set forth in your letter cannot be fairly understood except in the context of my life and my work. This answer is in the form of a summary account of relevant aspects of my life in more or less chronological order, in the course of which I shall comment on the specific items in your letter. Through this answer, and through the hearings before the Personnel Security Board, which I hereby request, I hope to provide a fair basis upon which the questions posed by your letter may be resolved.

"The PreWar Period

"I was born in New York in 1904. My father had come to this country at the age of seventeen from Germany. He was a successful businessman and quite active in community affairs. My mother was born in Baltimore and before her marriage was an artist and teacher of art. I attended Ethical Culture School and Harvard College, which I entered in 1922. I completed the work for my degree in the spring of 1925. I then left Harvard

to study at Cambridge University and in Goettingen, where in the spring of 1927 I took my doctor's degree. The following year I was National Research Fellow at Harvard and at the California Institute of Technology. In the following year I was Fellow of the International Education Board at the Universi of Leiden and at the Technical High School in Zurich.

"in the spring of 1929, I returned to the United States. I was homesick for this country, and in fact I did not leave it again for over nineteen years. I had learned a great deal in my student days about the new physics; I wanted to pursue this myself, to explain it and to foster its cultivation. I had had many invitations to university position one or two in Europe, and perhaps ten in the United States. I accepted concurrent appointments as Assistant Professor at the California Institute of Technology in Pasadena and at the University of California in Berkeley. For the coming twelve years, I was to devote my time to these two faculties.

"Starting with a single graduate student in my first year in Berkeley, we gradually began to build up what was to become the largest school in the country of graduate and post-doctoral study in theoretical physics, so that as time went on, we came to have between a dozen and twenty people learning and adding to quantum theory, nuclear physics, relativity and other modern physics. As the number of students increased, so in general did their quality: the men who

worked with me during those years hold chairs in many of the great centers of physics in this country; they have made important contributions to science, and in many cases to the atomic energy project. Many of my students would accompany me to Pasadena in the spring after the Berkeley term was over, so that we might continue to work together.

"My friends, both in Pasadena and in Berkeley, were mostly faculty people, scientists, classicists and artists. I studied and read Sanskrit with Arthur Rider. I read very widely, but mostly classics, novels, plays and poetry; and I read something of other parts of science. I was not interested in and did not read about economics or politics. I was almost wholly divorced from the contemporary scene in this country. I never read a newspaper or a current magazine like Time or Harper's; I had no radio, no telephone; I learned of the stock market crash in the fall of 1929 only long after the event; the first time I ever voted was in the Presidential election of 1936. To many of my friends, my indifference to contemporary affairs seemed bizarre, and they often chided me with being too much of a highbrow. I was interested in man and his experience; I was deeply interested in my science; but I had no understanding of the relations of man to his society.

"I spent some weeks each summer with my brother Frank at our ranch in New Mexico. There was a strong bond of

affection between us. After my mother's death, my father came often, mostly in Berkeley, to visit me; and we had an intimate and close association until his death.

"Beginning in late 1936, my interests began to change. These changes did not alter my earlier friendships, my relations to my students, or my devotion to physics; but the added something new. I can discern in retrospect more then one reason for these changes. I had had a continuing, smoldering fury about the treatment of Jews in Germany. I had relatives there, and was later to help in extricating them and bringing them to this country. I saw what the depression was doing to my students. Often they could get no jobs, or jobs which were wholly inadequate. And through them, I began to understand how deeply political and economic events could affect men's lives. I began to feel the need to participate more fully in the life of the community. But I had no framework of political conviction or experience to give me perspective in these matters.

"In the spring of 1936, I had been introduced by friends to Jean Tatlock, the daughter of a noted professor of English at the University; and in the autumn, I began to court her, and we grew close to each other. We were at least twice close enough to marriage to think of ourselves as engaged. Between 1939 and her death in 1944 I saw her very rarely. She told me about her Communist Party memberships;

they were on again, off again affairs, and never seemed to provide for her what she was seeking. I do not believe that her interests were really political. She loved this country and its people and its life. She was, as it turned out, a friend of many fellow travelers and communists, with a number of whom I was later to become acquainted.

"I should not give the impression that it was wholly because of Jean Tatlock that I made leftwing friends, or felt sympathy for causes which hitherto would have seemed so remote from me, like the loyalist cause in Spain, and the organization of migratory workers. I have mentioned some of the other contributing causes. I liked the new sense of companionship, and at the time felt that I was coming to be part of the life of my time and country.

"In 1937, my father died; a little later, when I came into an inheritance, I made a will leaving this to the University of California for fellowships to graduate students.

"This was the era of what the communists then called the "united front', in which they joined with many non-communist groups in support of humanitarian objectives. Many of these objectives engaged my interest. I contributed to the strike fund of one of the major strikes of Bridges' union; I subscribed to the People's World; I contributed to the various committees and organizations which were intended to help the Spanish loyalist cause. I was invited to help

establish the Teacher's Union, which included faculty and teaching assistants at the University, and school teachers of the East Bay. I was elected recording secretary. My connection with the Teacher's Union continued until some time in 1941, when we disbanded our chapter.

"During these same years, I also began to take part in the management of the Physics Department, the selection of courses, and the awarding of fellowships, and in the general affairs of the Graudate School of the University, mostly through the Graduate Council, of which I was a member for some years.

"I also became involved in other organizations. For perhaps a year, I was a member of the Western Council of the Consumer's Union which was concerned with evaluating information on products of interest on the West Coast. I do not recall Arthur Kallet, the national head of the Consumer's Union; at most I could have met him if he made a visit to the West Coast. I joined the American Committee for Democracy and Intellectual Freedom. I think it then stood as a protest against what had happened to intellectuals and professionals in Germany. I listed, in the Personnel Security Questionnaire that I filled out in 1942 for employment with the Manhattan District, the very few political organizations of which I had ever been a member. I say on that questionnaire that I did not include sponsorships. I have no recollection of the

Friends of the Chinese People, or of what, if any, my connection with this organization was.

"The statement is attributed to me that, while I was not a communist, I "had probably belonged to every communist front organization on the West Coast and had signed many petitions in which communists were interested.' I do not recall this statement, nor to whom I might have made it, nor the circumstances. The quotation is not true. It seems clear to me that if I said anything along the lines quoted, it was a half-jocular over-statement.

"The matter which most engaged my sympathies and interests was the war in Spain. This was not a matter of understanding and informed convictions. I had never been to Spain; I knew a little of its literature; I knew nothing of its history or politics or contemporary problems. But like a great many other Americans I was emotionally committed to the loyalist cause. I contributed to various organizations for Spanish relief. I went to, and helped with, many parties, bazaars, and the like. Even when the war in Spain was manifestly lost, these activities continued. The end of the war and the defeat of the loyalists caused me great sorrow.

"It was probably through Spanish relief efforts that I met Dr. Thomas Addis, and Rudy Lambert. As to the latter, our association never became close. As to the former, he was a distinguished medical scientist who became a friend.

Addis asked me, perhaps in the winter of 1937-38, to contribute through him to the Spanish cause. He made it clear that this money, unlike that which went to the relief organizations, would go straight to the fighting effort, and that it would go through communist channels. I did so contribute; usually when he communicated with me, explaining the nature of the need, I gave him sums in cash, probably never much less than a hundred dollars, and occasionally perhaps somewhat more than that, several times during the winter. I made no such contributions during the spring terms when I was in Pasadena or during the summers in New Mexico. Later -- but I do not remember the date -- Addis introduced me to Isaac Folkoff, who was, as Addis indicated, in some way connected with the Communist Party, and told me that Folkoff would from then on get in touch with me when there was need for money. This he did, in much the same way that Addis had done before. As before, these contributions were for specific purposes, principally the Spanish War and Spanish relief. Sometimes I was asked for money for other purposes, the organization of migratory labor in the California valleys, for instance. I doubt that it occurred to me that the contributions might be directed to other purposes than those I had intended, or that such other purposes might be evil. I did not then regard communists as dangerous; and some of their declared objectives seemed to me desirable.

"In time these contributions came to an end. I went to a big Spanish relef party the night before Pearl Harbor; and the next day, as we heard the news of the outbreak of war, I decided that I had had about enough of the Spanish cause, and that there were other and more pressing crises in the world. My contributions would not have continued much longer.

My brother Frank married in 1936. Our relations thereafter were inevitably less intimate than before. He told me at the time -- probably in 1937 -- that he and his wife Jackie had joined the Communist Party. Over the years we saw one another as occasions arose. We still spent summer holidays together. In 1939 or 1940 Frank and Jackie moved to Stanford; in the autumn of 1941 they came to Berkeley, and Frank worked for the Radiation Laboratory. At that time he made it clear to me that he was no longer a member of the Communist Party.

"As to the alleged activities of Jackie and Frank in 1944, 1945 and 1946: I was not in Berkeley in 1944 and 1945; I was away most of the first half of 1946; I do not know whether these activities occurred or not, and if I had any knowledge of them at the time it would have been very sketchy. After Christmas of 1945 my family and I visited my brother's family for a few days during the holidays, and I remember that we were there New Year's eve and New Year's day in 1946.

On New Year's day people were constantly dropping in. Pinsky and Adelson, who were at most casual acquaintances of mine, may have been among them, but I cannot remember their being there, nor indeed do I remember any of the others who dropped in that day or what was discussed.

"It was in the summer of 1939 in Pasadena that I first met my wife. She was married to Dr. Harrison, who was a friend and associate of the Tolmans, Lauritsens and other of the California Institute of Technology faculty. I learned of her earlier marriage to Joe Dallet, and of his death fighting in Spain. He had been a Communist Party official, and for a year or two during their brief marriage my wife was a Communist Party member. When I met her I found in her a deep loyalty to her former husband, a complete disengagement from any political activity, and a certain disappointment and contempt that the Communist Party was not in fact what she had once thought it was.

"My own views were also evolving. Although Sidney and Beatrice Webb's book on Russia, which I had read in 1936, and the talk that I heard at that time had predisposed me to make much of the economic progress and general level of welfare in Russia, and little of its political tyranny, my views on this were to change. I read about the purge trials, though not in full detail, and could never find a view of them which was not damning to the Soviet system. In 1938

I met three physicists who had actually lived in Russia in the 30's. All were eminent scientists, Placzek, Weisskopf and Schein; and the first two have become close friends. What they reported seemed to me so solid, so unfanatical, so true, that it made a great impression; and it presented Russia, even when seen from their limited experience, as a land of purge and terror, of ludicrously bad management and of a long-suffering people. I need to make clear that this changing opinion of Russia, which was to be reinforced by the Nazi-Soviet pact, and the behavior of the Soviet Union in Poland and in Finland, did not mean a sharp break for me with those who held to different views. At that time I did not fully understand -- as in time I came to understand -- how completely the Communist Party in this country was under the control of Russia. During and after the battle of France, however, and during the battle of England the next autumn, I found myself increasingly out of sympathy with the policy of disengagement and neutrality that the communist press advocated.

"After our marriage in 1940, my wife and I for about two years had much the same circle of friends as I had had before -- mostly physicists and University people. Among them the Chevaliers, in particular, showed us many acts of kindness. We were occasionally invited to more or less obviously leftwing affairs, Spanish relief parties that still

continued; and on two occasions, once in San Francisco and once in Berkeley, we attended social gatherings of apparently well to do people, at which Schneiderman, an official of the Communist Party in California, attempted, not with success as far as we were concerned, to explain what the communist line was all about. I was asked about the Berkeley meeting in an interview in 1946 with agents of the F.B.B. I did not then recall this meeting, and in particular did not in any way connect it with Chevalier, about whom the agents were questioning me; hence it seemed wholly irrelevant to the matter under discussion. Later my wife reminded me that the Berkeley meeting had occurred at the house of the Chevaliers; and when I was asked about it by the F.B.I. in 1950, I told them so.

"We saw a little of Kenneth May; we both liked him. It would have been not unnatural for us to go to a housewarming for May and his wife; neither my wife nor I remember such a party. Weinberg was known to me as a graduate student; Hiskey I did not know. Steve Nelson came a few times with his family to visit; he had befriended my wife in Paris, at the time of her husband's death in Spain in 1937. Neither of us has seen him since 1941 or 1942.

"Because of these associations that I have described, and the contributions mentioned earlier, I might well have appeared at the time as quite close to the Communist Party --

perhaps even to some people as belonging to it. As I have said, some of its declared objectives seemed to me desirable. But I never was a member of the Communist Party. I never accepted communist dogma or theory; in fact, it never made sense to me. I had no clearly formulated political views. I hated tyranny and repression and every form of dictatorial control of thought. In most cases I did not in those days know who was and who was not a member of the Communist Party. No one ever asked me to join the Communist Party.

"Your letters sets forth statements made in 1942-45 by persons said to be Communist Party officials to the effect that I was a concealed member of the Communist Party. I have no knowledge as to what these people might have said. What I do know is that I was never a member of the party, concealed or open. Even the names of some of the people mentioned are strange to me, such as Jack Manley and Katrina Sandow. I doubt that I met Bernadette Doyle, although I recognize her name. Pinsky and Adelson I met at most casually, as previously mentioned.

"By the time that we moved to Los Alamos in early 1943, both as a result of my changed views and of the great pressure of war work, my participation in leftwing organizations and my associations with leftwing circles had ceased and were never to be re-established.

"In August 1941, I bought Eagle Hill at Berkeley for my wife, which was the first home we had of our own. We settled down to live in it with our new baby. We had a good many friends, but little leisure. My wife was working in biolog at the University. Many of the men I had known went off to work on radar and other aspects of military research. I was not without envy of them; but it was not until my first connection with the rudimentary atomic energy enterprise that I began to see any way in which I could be of direct use."

Let the record show that Mr. Oppenheimer has asked to be excused briefly.

"The War Years.

"Ever since the discovery of nuclear fission, the possibility of powerful explosives based on it had been very much in my mind, as it had in that of many other physicists. We had some understanding of what this might do for us in the war, and how much it might change the course of history. In the autumn of 1941, a special commitee was set up by the National Academy of Sciences under the chairmanship of Arthur Compton to review the prospects and feasibility of the different uses of atomic energy for military purposes. I attended a meeting of this committee; this was my first official connection with the atomic energy program.

"After the Academy meetng, I spent some time in preliminary calculations about the construction and

performance of atomic bombs, and became increasingly excited at the prospects. At the same time I still had a quite heavy burden of academic work with courses and graduate students. I also began to consult, more or less regularly, with the staff of the Radiation Laboratory in Berkeley on their program for the electro-magnetic separation of uranium isotopes. I was never a member or employee of the laboratory; but I attended many of its staff and policy meetings. With the help of two of my graduate students, I developed an invention which was embodied in the production plants at Oak Ridge. I attended the conference in Chicago at which the Metallurgical Laboratory (to produce plutonium) was establishec and its initial program projected.

"In the spring of 1942, Compton called me to Chicago to discuss the state ofwork on the bomb itself. During this meeting Compton asked me to take the responsibilit for this work, which at that time consisted of numerous scattered experimental projects. Although I had no administrative experience and was not an experimental physicist, I felt sufficiently informed and challenged by the problem to be glad to accept. At this time I became an employee of the Metallurgical Laboratory.

"After this conference I called together a theoretical study group in Berkeley, in which Bethe, Konopinsk Serber, Teller, Van Fleck and I participated. We had an

adventurous time. We spent much of the summer of 1942 in Berkeley in a joint study that for the first time really came to grips with the physical problems of atomic bombs, atomic explosions, and the possibility of using fission explosions to initiate thermo-nuclear reactions. I called this possibility to the attention of Dr. Bush during the late summer; the technical views on this subject were to develop and change from then until the present day.

"After these studies there was little doubt that a potentially world-shattering undertaking lay ahead. We began to see the great explosion at Alamogordo and the greater explosions at Eniwetok with a surer foreknowledge. We also began to see how rough, difficult, challenging and unpredictable this job might turn out to be.

"When I entered the employ of the Metallurgical Laboratory I filled out my first Personnel Security Questionnaire."

Let the record show that Dr. Oppenheimer has returned to the hearing room."

"Later in the summer, I had word from Compton that there was a question of my clearance on the ground that I had belonged to leftwing groups; but it was indicated that this would not prove a bar to my further work on the program.

"In later summer, after a review of the experimental work, I became convinced, as did others, that a major change

was called for in the work on the bomb itself. We needed a central laboratory devoted wholly to this purpose, where people could talk freely with each other, where theoretical ideas and experimental findings could affecteach other, where the waste and frustration and error of the many compartmentalized experimental studies could be eliminated, where we could begin to come to grips with chemical, metallurgical, engineering and ordnance problems that had so far received no consideration. We therefore sought to establish this laboratory for a direct attack on all the problems inherent in the most rapid possible development and production of atomic bombs.

"In the autumn of 1942 General Groves assumed charge of the Manhattan Engineer District. I discussed with him the need for an atomic bomb laboratory. There had been some thought of making this laboratory a part of Oak Ridge. For a time there was support for making it a military establishment in which key personnel would be commissioned as officers; and in preparation for this course I once went to the Presidio to take the initial steps toward obtaining a commission. After a good deal of discussion with the personne who would be needed at Los Alamos and with General Groves and his advisers, it was decided that the Laboratory should, at least initially, be a civilian establishment in a military post. While this consideration was going on, I had showed

General Groves Los Alamos; and he almost immediately took steps to acquire the site.

"In early 1943, I received a letter signed by General Groves and Dr. Conant, appointing me director of the laboaratory, and outlining their conception of how it was to be organized and administered. The necessary construction and assembling of the needed facilities were begun. All of us worked in close collaboration with the engineers of the Manhattan District.

The site of Los Alamos was selected, in part at least because it enabled those responsible to balance the obvious need for security with the equally important need of free communication among those engaged in the work. Security, it was hoped, would be achieved by removing the laboratory to a remote area, fenced and patrolled, where communication with the outside was extremely limited. Telephone calls were monitored, mail was censored, and personnel who left the area -- something permitted only for the clearest of causes-- knew that their movements might be under surveillance. On the other hand, for those within the community, fullest exposition and discussion among those competent to use the information was encouraged.

"The last months of 1942 and early 1943 had hardly hours enough to get Los Alamos established. The real problem had to do with getting to Los Alamos the men who would make

a success of the undertaking. For this we needed to understand as clearly as we then could what our technical program would be, what men we would need, what facilities, what organization, what plan.

The program of recruitment was massive. Even though we then underestimated the ultimate size of the laboratory, which was to have almost 4,000 members by the spring of 1945, and even though we did not at that time see clearly some of the difficulties which were to bedevil and threaten the enterprise, we knew that it was a big, complex and diverse job. Even the initial plan of the laboratory called for a start with more than one hundred highly qualified and trained scientists, to say nothing of the technicians, staff and mechanics who would be required for their support, and of the equipment that we would have to beg and borrow since there would be no time to build it from scratch. We had to recruit ata time when the country was fully engaged in war and almost every competent scientist was already involved in the military effort.

"The primary burden of this fell on me. To recruit staff I traveled all over the country talking with people who had been working on one or another aspect of the atomic energy enterprise, and people in radar work, for example, and underwater sound, telling them about the job, the place that we were going to, and enlisting their enthusiasm.

"In order to bring responsible scientists to Los Alamos, I had to rely on their sense of the interest, urgency and reasibility of the Los Alamos mission. I had to tell them enough of what the job was, and give strong enough assurance that it might be successfully accomplished in time to affect the outcome of the war, to make it clear that they were justified in their leaving other work to come to this job.

"The prospect of coming to Los Alamos aroused great misgivings. It was to be a military post; men were asked to sign up more or less for the duration; restrictions on travel and on the freedom of families to move about were to be severe; and no one could be sure of the extent to which the necessary technical freedom of action could actually be maintained by the laboratory. The notion of disappearing into the New Mexico desert for an indeterminate period and under quasi-military auspices disturbed a good many scientists and the families of many more. But there was another side to it. Almost everyone realized that this was a great undertaking. Almost everyone knew that if it were completed successfully and rapid enough, it might determine the outcome of the war. Almost everyone knew that it was an unparalleed opportunity to bring to bear the basic knowledge and art of science for the benefit of his country. Almost everyone knew that this job, if it were achieved, would be a part of history. This sense of excitement, of devotion and of

patriotism in the end prevailed. Most of those with whom I talked came to Los Alamos. Once they came, confidence in the enterprise grew as men learned more of the technical status of the work; and though the laboratory was to double and redouble its size many times before the end, once it had started it was on the road to success.

"We had information in those days of German activity in the field of nuclear fission. We were aware of what it mightmean if they beat us to the draw in the development of atomic bombs. The consensus of all our opinions, and every directive that I had, stressed the extreme urgency of our work, as well as the need for guarding all knowledge of it from our enemies. Past communist connections or sympathies did not necessarily disqualify a man from employment, if we had confidence in his integrity and dependability as a man.

"There are two items of derogatory information on which I need to comment at this point. The first is that it was reported that I had talked the atomic bomb question over with Communist Party members during this period (1942-45). The second is that I was responsible for the employment of the atomic bomb project of individuals who were members o the Communist party or closely associated with activities of the Communist Party.

"As to the first, my only discussions of matters connected with the atomic bomb were for official work or for

recruiting the staff of the enterprise. So far as I knew none of these discussions were with Communist Party members. I never discussed anything of my secret work or anything about the atomic bomb with Steve Nelson.

"As to the statement that I secured the employment of doubtful persons on the project: Of those mentioned, Lomanitz, Friedman and Weinberg were never employed at Los Alamos. I believe that I had nothing to do with the employment of Friedman and Weinberg by the Radiation Laboratory; I had no responsibility for the hiring of anyone there. During the time that I continued to serve as a consultant with the Radiation Laboratory and to advise and direct the work of some of the graduate students, I assigned David Bohm and Chaim Richman to a problem of basic science which might prove useful in analyzing experiements in connection with fast neutrons. That work has long been published. Another graduate student was Rossi Lomanitz. I remember vaguely a conversation with him in which he expressed reluctance to take part in defense research, and I encouraged him to do what other scientists were doing for their country. Thereafter he did work at the Radiation Laboratory. I remember no details of our talk. If I asked him to work on the project, I would have assumed that he would be checked by the security officers as a matter of course. Later, in 1943, when Lomanitz was inducted into the Army, he wrote me asking

me to help his return to the project. I forwarded a copy of this letter to the Manhattan District Security officers, and let the matter rest there. Still later, at Lomanitz' request, I wrote to his commanding officer that he was qualified for advanced technical work in the Army.

"I asked for the transfer of David Bohm to Los Alamos; but this request, like all others, was subject to the assumption that the usual security requirements would apply; and when I was told that there was objection on security grounds to this transfer, I was much surprised, but of course agreed. David Hawkins was known to the Personnel Director at the laboratory, and I had met and liked him and found him intelligent; I supported the suggestion of the Personnel Director that he come to Los Alamos. I understand that he had had leftwing associations; but it was not until in March of 1951, at the time of his testimony, that I knew about his membership in the Communist Party.

"In 1943 when I was alleged to have stated that "I knew several individuals then at Los Alamos who had been members of the Communist Party," I knew of only one; she was my wife, of whose disassociation from the party, and of whose integrity and loyalty to the United States I had no question. Later, in 1944 or 1945, my brother Frank, who had been cleared for work in Berkeley and at Oak Ridge, came to Los Alamos from Oak Ridge with official approval.

"I knew of no attempt to obtain secret information at Los Alamos. Prior to my going there my friend Haakon Chevalier with his wife visited us on Eagle Hill, probably in early 1943. During the visit, he came into the kitchen and told me that George Ententon had spoken to him of the possibility of transmitting technical information to Soviet scientists. I made some strong remark to the effect that this sounded terribly wrong to me. The discussion ended there. Nothing in our long standing friendship would have led me to believe that Chevalier was actually seeking information; and I was certain that he had no idea of the work on which I was engaged.

"It has long been clear to me that I should have reported the incident at once. The events that led me to report it -- which I doubt ever would have become known without my report -- were unconnected with it. During the summer of 1943, Colonel Lansdale, the Intelligence Office of the Manhattan District, came to Los Alamos and told me that he was worried about the security situation in Berkeley because of the activities of the Federation of Architects, Engineers, Chemists and Technicians. This recalled to my mind that Eltenton was a member and probably a promoter of the FAECT. Shortly thereafter, I was in Berkeley and I told the security officer that Eltenton would bear watching. When asked why, I said that Eltenton had attempted, through

intermediaries, to approach people on the project, though I mentioned neither myself nor Chevalier. Later, when General Groves urged me to give the details, I told him of my conversation with Chevalier. I still think of Chevalier as a friend.

"The story of Los Alamos is long and complex. Part of it is public history. For me it was a time so filled with work, with the need for decision and action and consultation, that there was room for little else. I lived with my family in the community which was Los Alamos. It was a remarkable community, inspired by a high sense of mission, of duty and of destiny, coherent, dedicated and remarkably selfless. There was plenty in the life of Los Alamos to cause irritation; the security restrictions, many of my own devising, the inadequacies and inevitable fumblings of a military post unlike any that had ever existed before, shortages, inequities, and in the laboratory itself the shifting emphasis on different aspects of the technical work as the program moved forward; but I have never known a group more understanding and more devoted to a common purpose, more willing to lay aside personal convenience and prestige, more understanding of the role that they were playing in their country's history. Time and again we had in the technical work almost paralyzing crises. Time and again the laboratory drew itself together and faced the new problems and got on with the work.

We worked by night and by day; and in the end the many jobs were done.

"These years of hard and loyal work of the scientists culminated in the test on July 16, 1945. It was a success. I believe that in the eyes of the War Department, and other knowledgeable people, it was as early a success as they had thought possible, given all the circumstances, and rather a greater one. There were many indications from the Secretary of War and General Groves, and many others, that official opinion was one of satisfaction with what had been accomplished. At the time, it was hard for us in Los Alamos not to share that satisfaction, and hard for me not to accept the conclusion that I had managed the enterprise well and played a key part in its success. But it needs to be stated that many others contributed the decisive ideas and carried out the work which led to this success and that my role was that of understanding, encouraging, suggesting and deciding. It was the very opposite of a one man show.

"Even before the July 16th test and the use of the bombs in Japan, the members of the laboratory began to have a new sense of the possible import of what was going on. In the early days, when success was less certain and timing unsure, and the war with Germany and Japan in a desperate phase, it was enough for us to think that we had a job to do. Now, with Germany defeated, the war in the Pacific approaching

a crisis, and the success of our undertaking almost assured, there was a sense both of hope and of anxiety as to what this spectacular development might portend for the future. This came to us a little earlier than to the public generally because we saw the technical development at close range and in secret; but its quality was very much the same as the public response after Hiroshima and Nagasaki.

"Thus it was natural that in the spring of 1945 I welcomed the opportunity when I was asked by Secretary Stimson to serve, along with Compton, Lawrence and Fermi, on an advisory panel to his Interim Committee on Atomic Energy. We met with that committee on the 1st of June 1945; and even during the week when Hiroshima and Nagasaki were being bombed, we met at Los Alamos to sketch out a prospectus of what the technical future in atomic energy might look like: atomic war heads for guided missiles, improvements in bomb designs, the thermonuclear program, power, propulsion, and the new tools available from atomic technology for research in science, medicine, and technology. This work absorbed much of my time, during September and October; and in connection with it I was asked to consult with the War and State Departments on atomic energy legislation, and in a preliminary way on the international control of atomic energy.

"I resigned as Director of Los Alamos on October 16, 1945 after having secured the consent of Commander Bradbury

and General Groves that Bradbury should act as my successor.

"There were then on the books at the laboratory, embodied in memoranda and reports and summarized by me in letters to General Groves, developments in atomic weapons, which could well have occupied years for their fulfillment, and which have in fact provided some, though by no means all, of the themes for Los Alamos work since that time. It was not entirely clear whether the future of atomic weapons work in this country should be continued at or confined to Los Alamos or started elsewhere at a more accessible and more practical site, or indeed what effect international agreements might have on the program. But in the meantime Los Alamos had to be kept going until there was created an authority competent to decide the question of its future. This was to take almost a year.

The Post War Period.

"In November 1945, I resumed my teaching at the California Institute of Technology, with an intention and hope never realized, that this should be a full time undertaking. The consultation about postwar matter which had already begun continued, and I was asked over and over both by the Executive and the Congress for advice on atomic energy. I had a feeling of deep responsibility, interest and concern for many of the problems with which the development of atomic energy confronted our country.

"This development was to be a major factor in the history of the evolving and mounting conflict between the free world and the Soviet Union. When I and other scientists were called on for advice, our principal duty was to make our technical experience and judgment available. We were called to do this in a context and against a background of the official views of the government on the military and political situation of our country. Immediately after the war I was deeply involved in the effort to devise effective means for the international control of atomic weapons, means which might, in the words of those days, tend toward the elimination of war itself. As the prospects of success receded, and as evidence of Soviet hostility and growing military power accumulated, we had more and more to devote ourselves to finding ways of adapting our atomic potential to offset the Soviet threat. In the period marked by the first Soviet atomic explosion, the war in Korea and the Chinese communist intervention there, we were principally preoccupied, though we never forgot long term problems, with immediate measures which could rapidly build up the strength of the United States under the threat of an imminent general war. As our own atomic potential increased and developed, we were aware of the dangers inherent in comparable developments by the enemy; and preventive and defensive measures were very much on our minds. Throughout this time the role of atomic

weapons was to be central.

From the close of the war, when I returned to the West Coast until finally in the spring of 1947 when I went to Princeton as the Director of The Institute for Advanced Study, I was able to spend very little time at home and in teaching in California. In October 1945, at the request of Secretary of War Patterson, I had testified before the House Committee on Military Affairs in support of the May-Johnson Bill, which I endorsed as an interim means of bringing about without delay the much needed transition from the wartime administration of the Manhattan District to postwar management of the atomic energy enterprise. In December, 1945, and later, I appeared at Senator McMahon's request in sessions of his Special Committee on Atomic Energy, which was considering legislation on the same subject. Under the chairmanship of Dr. Richard Tolman, I served on a committee set up by General Groves to consider classification policy on matters of atomic energy. For two months, early in 1946, I worked steadily as a member of a panel, the Board of Consultants to the Secretary of State's Committee on Atomic Energy, which, with the Secretary of State's Committee, prepared the so-called Acheson-Lilienthal report. After the publication of this report, I spoke publicly in support of it. A little later, when Mr. Baruch was appointed to represent the United States in the United Nations Atomic Energy Committee, I became one

of the scientific consultants to Mr. Baruch, and his staff in preparation for and in the conduct of our efforts to gain support for the United States' plan. I continued as consultant to General Osborn when he took over the effort.

"At the end of 1946 I was appointed by the President as a member of the General Advisory Committee to the Atomic Energy Commission. At its first meeting I was elected Chairman and was reelected until the expiration of my term in 1952. This was my principal assignment during these years as far as the atomic energy program was concerned, and my principal preoccupation apart from academic work.

"A little later I was appointed to the Committee on Atomic Energy of the Research and Development Board, which was to advise the Military Establishment about the technical aspects of the atomic energy program; I served on it for seven years; and twice was designated chairman of special panels set up by the Committee.

"Meanwhile I had become widely regarded as a principal author or inventor of the atomic bomb, more widely, I well knew, than the facts warranted. In a modest way I had become a kind of public personage. I was deluged as I have been ever since with requests to lecture, and to take part in numerous scientific activities and public affairs. Most of these I did not accept. Some, important for the promotion of science or learning or of public policies that corresponded

to my convictions, I did accept: the Council of the National Academy of Sciences,,the Committee on the Present Danger; the Board of Overseers of Harvard College, and a good number of others.

"A quite different and I believe unique occurrence is cited as an item of derogatory information -- that in 1946 I was "listed as Vice Chairman on the letterhead of the Independent Citizens Committee of the Arts, Sciences, and Professions, Inc. . . . cited as a Communist front by the House Committee on Un-American Activities." The fact is that in 1946, when I was at work on the international control of atomic energy, I was notified that I had been nominated and then elected as Vice Chairman of this organization. When I began to see that its literature included slogans such as "Withdraw United States troops from China" and that it was endorsing the criticism enunciated by the then Secretary Wallane of the United States policy on atomic energy, I advised the organization in a letter of October 11, 1946, that I was not in accord with its policy, that I regarded the recommendations of Mr. Wallace as not likely to advance the cause of finding a satisfactory solution for the control of atomic energy, and that I wished to resign. When an effort was made to dissuade me from this course I again wrote on December 2, 1946, insisting upon resignation.

Later in the postwar period an incident occurred

which seems to be the basis of one of the items of derogatory information. In May, 1950, Paul Crouch, a former communist official, and Mrs. Crouch, testified before the California State Committee on Un-American Activities that in July 1941 they had attended a Communist Party meeting at a house in Berkeley, of which I was then the tenant. On the basis of pictures and movies of me which they saw some eight years later, they said they recognized me as having been present. When the F.B.I. first talked to me about this alleged incident, I was quite certain that no such meeting as Crouch described had occurred. So was my wife, when I discussed it with her. Later, when I saw the testimony, I became even more certain. Crouch had described the gathering as a closed meeting of the Communist Party. I was never a member of the party. Crouch said that no introductions had been made. I would not recall ever having had a group of people at my home that had not been introduced. In May of 1952, I again discussed this alleged meeting with the U. S. attorney in the Weinberg case (an indictment against Joseph Weinberg for perjury for having among other things denied membership in the Communist Party). I again said that I could not have been present at a closed meeting of the Communist Party because I was not a member of the party; that I had searched my memory and that the only thing that conceivably could be relevant was the vaguest impressions that

someone on the campus might at some time have asked permission to use our home for a gathering of young people; that, however, I could recall no such gathering, nor any meeting even remotely resembling the one described by Crouch; that I thought it probable that at the time of the meeting, which by then had been fixed by Crouch as approximately July 23rd, my wife and I were away from Berkeley. Shortly thereafter, with the aid of counsel, we were able to establish that my wife and I left Berkeley within a few days after July 4, 1941, and did not return until toward the end of the first week in August.

"I need to turn now to an account of some of the measures which, as Chairman of the General Advisory Committee, and in other capacities, I advocated in the years since the war to increase the power of the United States and its allies to resist and defeat aggression.

"The initial members of the General Advisory Committee were Conant, then President of Harvard, DuBridge, President of the California Institute of Technology, Fermi of the University of Chicago, Rabi of Columbia University, Rowe, Vice President of the United Fruit Company, Seaborg of the University of California, Cyril Smith of the University of Chicago, and Worthington of the DuPont Company. In 1948 Buckley, President of the Bell Telephone Laboratories, replaced Worthington; in the summer of 1950, Fermi, Rowe and

Seaborg were replaced by Libby of the University of Chicago, Murphree, President of Standard Oil Development Company, and Whitman of the Massachusetts Institute of Technology. Later Smith resigned and was succeeded by vo Neumann of The Institute for Advanced Study.

"In these years from early 1947 to mid-1952 the Committee met some thirty times and transmitted perhaps as many reports to the Commission. Formulation of policy and the management of the vast atomic energy enterprises were responsibilities vested in the Commission itself. The General Advisory Committee had the role, which was fixed for it by statute, to advise the Commission. In that capacity we gave the Commission our views on questions which the Commission put before us, brought to the Commission's attention on our initiative technical matters of importance, and encouraged and supported the work of the several major installations of the Commission.

"At one of our first meetings in 1947 we settled down to the job of forming our own views of the priorities. And while we agreed that the development of atomic power and the support and maintenance of a strong basic scientific activity in the fields relevant to it were important, we assigned top priority to the problem of atomic weapons. At that time we advised the Commission that one of its first jobs would be to convert Los Alamos into an active center

for the development and improvement of atomic weapons. In 1945-46 during the period immediately following the war, the purposes of Los Alamos were multiple. It was the only laboratory in the United States that worked on atomic weapons. Los Alamos also had wide interests in scientific matters only indirectly related to the weapons program. We suggested that the Commission recognize as the laboratory's central and primary program the improvement and diversification of atomic weapons, and that this undertaking have a priority second to none. We suggested further that the Commission adopt administrative measures to make work at Los Alamos attractive, to assist the laboratory in recruiting, to help build up a strong theoretical division for guidance in atomic weapons design, and to take advantage of the availability of the talented and brilliant consultants who had been members of the laboratory during the war. In close consultation with the Director of the Los Alamos Laboratory, we encouraged and supported courses of development which would markedly increase the value of our stockpile in terms of the destructive power of our weapons, which would make the best use of existing stockpiles and those anticipated, which would provide weapons suitable for modern combat conditions and for varied forms of delivery and which in their cumulative effect would provide us with the great arsenal we now have.

We encouraged and supported the building up of

the laboratory at Sandia whose principal purpose is the integration of the atomic warhead with the weapons system in which it is to be used. In agreement with the Los Alamos staff we took from the very first the view that no radical improvement in weapons development would be feasible without a program of weapons testing. We strongly supported such a program, helped Los Alamos to obtain authorization for conducting the tests it wished, and encouraged the establishment of a permanent weapons testing station and the adoption of a continental test station to facilitate this work. As time went on and the development of atomic weapons progressed, we stressed the importance of integrating out atomic warheads and the development of the carriers, aircraft, missiles, etc., which could make them of maximum effectiveness.

"We observed that there were opportunities which needed to be explored for significantly increasing our arsenal of weapons both in numbers and in capabilities by means of production plant expansion and by ambitious programs to enlarge the sources of raw materials. It was not our function to formulate military requirements. We did regard it as our function to indicate that neither the magnitude of existing plant nor the mode of operation of existing plant which the Commission inherited, nor the limitation of raw materials to relatively well known and high grade sources of ore, need limit the atomic weapons program.

The four major expansion programs which were authorized during the six years 1946 to 1952 reflect the decision of the Commission, the Military Establishment, the Joint Congressional Committee and other agencies of the government to go far beyond the production program that was inherited in 1946. And the powerful arsenal of atomic weapons and the variety of their forms adaptable to a diversity of military uses which is today a major source of our military strength in turn reflect the results of these decisions. The record of minutes, reports and other activities of the General Advisory Committee will show that that body within the limits of its role as an advisory group played a significant, consistent and unanimous part in encouraging and supporting and sometimes initiating the measures which are responsible for these results.

"As a committee and individually, our advice was sought on other matters as well. As early as October 1945 I had testified before a Senate Committee on the Kilgore-Magnuson Bill -- the initial measure for a National Science Foundation; like most scientists I was concerned that steps be taken for recreating in the United States a healthy scientific community after the disruption of the war years. In the General Advisory Committee we encouraged the Commission to do everything that it properly could to support atomic science, both in its own laboratories and in the

University centers to which we felt we must look for the training of scientists for advances of a basic character. Throughout the postwar period my colleagues and I stressed the importance of continuing support and promotion of basic science so that there might be a healthy balance between the effort invested in military research and applied science, and that invested in pure scientific training and research which is indispensable to all else. We supported the Commission's decision to make available for distribution in appropriate form and with appropriate safeguards the tracer materials, isotopes and radioactive substances which have played so constructive a part in medicine, in biological research, in techology, in pure science and in agriculture.

"We took an affirmative view on the development of reacters for submarines and naval propulsion not only for their direct military value but also because this seemed a favorable and forward-looking step in the important program of reactor development. We were, for the most part, skeptical about the initially very ambitious plans for the propulsion of aircraft, though we advocated the studies which in time brought this program to a more feasible course. We frequently pointed out to the Commission the technical benefits which would accrue to the United States by closer collaboration withthe atomic energy enterprise in Canada and the United Kingdom.

"During all the years that I served on the General Advisory Committee, however, its major preoccupation was with the production and perfection of atomic weapons. On the various recommendations which I have described, there were never, so far as I can remember, any significant divergences of opinion among the members of the committee. These recommendations, of course, constitute a very small sample of the committee's work, but a typical one.

"In view of the controversies that have developed I have left the subject of the "Super" and thermonuclear weapons for separate discussion -- although our Committee regarded this as a phase of the entire problem of weapons.

"The Super itself had a long history of consideration, beginning, as I have said, with our initial studies in 1942 before Los Alamos was established. It continued to be the subject of study and research at Los Alamos throughout the war. After the war, Los Alamos intself was inevitably handicapped pending the enactment of necessary legislation for the atomic energy enterprise. With the McMahon Act, the appointment of the Atomic Energy Commission and the General Advisory Committee, we in the Committee had occasion at our early meetings in 1947 as well as in 1948 to discuss the subject. In that period the General Advisory Committee pointed out the still extremely unclear status of the problem from the technical standpoint, and urged encouragement

of Los Alamos' efforts which were then directed toward modest exploration of the Super and of thermonuclear systems. No serious controversy arose about the Supmr until the Soviet explosion of an atomic bomb in the autumn of 1949.

"Shortly after that event, in October 1949, the Atomic Energy Commission called a special session of the General Advisory Committee and asked us to consider and advise two related questions: first, whether in view of the Soviet success the Commission's program was adequate, and if not, in what way it should be altered or increased; second, whether a "crash" program for the development of the Super should be a part of any new program. The Committee considered both questions, consulting various officials from the civil and military branches of the Executive Departments who would have been concerned, and reached conclusions which were communicated in a report to the Atomic Energy Commission in Octbber 1949.

"This report, in response to the first question that had been put to us, recommended a great number of measures that the Commission should take the increase in many ways onr overall potential in weapons.

"As to the Super itself, the General Advisory Committee stated its unanimous opposition to the initiation by the United States of a crash program of the kind we had been asked to advise on. The report of that meeting, and the

Secretary's notes, reflect the reasons which moved us to this conclusion. The annexes, in particular, which dealt more with political and policy considerations -- the report proper was essentially technical in character -- indicated differences in the views of members of the Committee. There were two annexes, one signed by Rabi and Fermi, the other by Conant, DuBridge, Smith, Rowe, Buckley and myself. (The ninth member of the committee, Seaborg, was abroad at the time.

"It would have been surprising if eight men considering a problem of extreme difficulty had each had precisely the same reasons for the conclusion in which we joined. But I think I am correct in asserting that the unanimous opposition we expressed to the crash program was based on the conviction, to which technical considerationsas well as others contributed, that because of our overall situation at that time such a program might weaken rather than strengthen the position of the United States.

"After the report was submitted to the Commission, it fell to me as Chairman of the Committee to explain our position on several occasions, once at a meeting of the Joint Congressional Committee on Atomic Energy. All this, however, took place prior to the decision by the President to proceed with the thermonuclear program.

"This is the full story of my "opposition to the hydrogen bomb." It can be read in the records of the General

transcript of my testimony before the Joint Congressional Committee. It is a story which ended once and for all when in January 1950 the President announced his decision to proceed with the program. I never urged anyone not to work on the hydrogen bomb project. I never made or caused any distribution of the GAC reports wxcept to the Commission itself As always, it was the Commission's responsibility to determine further distribution.

"In summary, in October, 1949, I and the other members of the General Advisory Committee were asked questions by the Commission to which we had a duty to respond, and to whi we did respond with our best judgment in the light of evidence then available to us.

"When the President's decision was announced in January, 1950, our Committee was again in session and we immediately turned to the technical problems facing the Commission in carrying out the President's directive. We sougl to give our advice then andin ensuing meetings as to the most promising means of solving these problems. We never again raised the question of the wisdom of the policy which had now been settled, but concerned ourselves rather with trying to implement it. During this period our recommendations for increasing production facilities included one for a dual purpose plant which could be adapted to make materials either for fission bombs or materials useful in a thermonuclear

program. In its performance characteristics, the Savannah River project, subsequently adopted by the Commission, was foreshadowed by this recommendation.

"While the history of the GAC opposition to a crash program for the Super ended with the announcement of the President's decision, the need for evaluation and advice continued. There were immense technical complications both before and after the President's decision. It was of course a primary duty of the committee, as well as other review committees on which I served, to report new developments which we judged promising, and to report when a given weapon or family of weapons appeared impractical, unfeasible or impossible. It would have been my duty so to report had I been alone in my views. As a matter of fact, our views on such matters were almost always unanimous. It was furthermore a proper function for me to speak my best judgment in discussion with those responsibly engaged in the undertaking.

Throughout the whole development of thermonuclear weapons, many occasions occurred where it was necessary for us to form and to express judgments of feasibility. This was true before the President's decision, and it was true after the President's decision. In our report of October 1949, we expressed the view, as your letter states, that "an imaginative and concerted attack on the problem has a better than even chance of producing the weapon within five years."

Later calculations and measurements made at Los Alamos led us to a far more pessimistic view. Still later brilliant inventions led to the possibility of lines of development of very great promise. At each stage the General Advisory Committee, and I as its Chairman and as a member of other bodies, reported as faithfully as we could our evaluation of what was likely to fail and what was likely to work.

In the Spring of 1951 work had reached a stage at which far reaching decisions were called for with regard to the Commission's whole thermonuclear program. In consultation with the Commission, I called a meeting in Princeton in the late spring of that year, which was attended by all members of the Commission and several members of its staff, by members of the General Advisory Committee, by Dr. Bradbury and staff of the Los Alamos Laboratory, by Bethe, Teller, Bacher, Fermi, von Neumann, Wheeler and others responsibly connected with the program. The outcome of the meeting, which lasted for two or three days, was an agreed program and a fixing of priorities and effort both for Los Alamos and for other aspects of the Commission's work. This program has been an outstanding success.

"In addition to my continuing work on the General Advisory Committee there were other assignments that I was asked to undertake. Late in 1950 or early in 1951 the President appointed me to the Science Advisory Committee to

advise the Office of Defense Mobilization and the President in 1952 the Secretary of State appointed me to a panel to advise on armaments and their regulation; and I served as consultant on continental defense, civil defense, and the use of atomic weapons in support of ground combat. Many of these duties led to reports in the drafting of which I participated, or for which I took responsibility. These supplement the record of the General Advisory Committee as an account of the counsel that I have given our government during the last eight years.

In this letter, I have written only of those limited parts of my history which appear relevant to the issue now before the Atomic Energy Commission. In order to preserve as much as possible the perspective of the story, I have dealt very briefly with many matters. I have had to deal briefly or not at all with instances in which my actions or views were adverse to Soviet or communist interest, and of actions that testify to my devotion to freedom, or that have contributed to the vitality, influence and power of the United States.

"In preparing this letter, I have reviewed two decade of my life. I have recalled instances where I acted unwisely. What I have hoped was, not that I could wholly avoid error, but that I might learn from it. What I have learned has, I think, made me more fit to serve my country.

"Very truly yours, J. Robert Oppenheimer,

Princeton, New Jersey, March 4th, 1954."

DR. GRAY: This Board is convened to enable Dr. Oppenheimer to present any information he considers appropriate having a bearing on the documents just read and the information contained in them, this information being, of course, the same as that disclosed to Dr. Oppenheimer in Mr. K. D. Nichols' letter of December 23, 1953 to Dr. Oppenheimer and Dr. Oppenheimer's reply of March 4, 1954, and to provide a record as a basis for a recommendation to the General Manager of theAtomic Energy Commission as to Dr. Oppenheimer's eligibility for access to restricted data.

At this point, I should like to remind everyone concerned that this proceeding is an inquiry and not in the nature of a trial. We shall approach our duties in that atmosphere and in that spirit.

Dr. Oppenheimer, have you been given an opportunity to exercise the right to challenge any or all of the members of this Board?

DR. OPPENHEIMER: I have, indeed.

DR. GRAY: I should point out to you, sir, that if at any time during the course of this hearing it appears that grounds for challenge for cause arise, you will exercise your right to challenge for cause and the validity of the challenge will be determined in closed session by the members of the Board.

The proceedings and stenographic record of this

2 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 iniative in public release of any information relating to the proceeding before this Board.

Now, at this time, Dr. Oppenheimer, you will be given the opportunity to present any material relevant to the issues before the Board. At this point I think we shall find it necessary to exclude all witnesses except the one whose testimony is being given to the Board under the provisions of the procedures which we must follow in this inquiry.

I shall read from the Security Clearance Procedures of the United States Atomic Energy Commission, dated 12 September, 1950, under Section 4.15, subsection (b):

"The proceedings shall be open only to duly authorized representatives of the Staff of the Atomic Energy Commission, the individual, his counsel, and such persons as may be officially authorized by the Board."

The Chairman would make the observation that Counsel for the Board has suggested that in the spirit of these regulations we should have present only the witness who is testifying or who is appearing.

MR. GARRISON: Mr. Chairman, may I just say that I have a few preliminary remarks as counsel to make before Dr.

3 Oppenheimer testifies, and it may well be that they will perhaps bring us to a suitable point of adjourning forthis morning, so that Dr. Oppenheimer's testimony might begin this afternoon.

However, if you would prefer that Mrs. Oppenheimer not be present while I make these preliminary remarks which have to do largely with procedural aspects of what we propose to do, it would be quite satisfactory, of course, to me.

DR. GRAY: Let us then proceed on that basis. So, Mrs. Oppenheimer, you are not at this moment excused.

I should like to ask Dr. Oppenheimer whether he wishes to testify under oath in this proceeding?

DR. OPPENHEIMER: Surely.

DR. GRAY: You are not required to do so.

DR. OPPENHEIMER: I think it best.

DR. GRAY: I should remind you, then, of the provisions ofSection 1621 of Title 18 of the United States Code, known as the perjury statute, which makes it a crime punishable by a fine of up to $2,000 and/or imprisonment of up to five years for any person stating under oath any material matter which he does not believe to be true.

It is also an offense under Section 1001 of Title 18 of the United States Code, punishable by a fine of not more than $10,000 or imprisonment for not more than five years, or both, for any person to make any false, fictious,

4 or fraudulent statement or representation in any matter within the jurisdiction of any agency of the United States.

I think that before you proceed, Mr. Garrison, that it would be well to administer the oath to Dr. Oppenheimer.

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

DR. OPPENHEIMER: I do.

DR. GRAY: May I also point out that in the event that it is necessary for anyone to disclose restricted data during his statements before this Board should advise the Chairman before such disclosure in order that persons unauthorized to have access to restricted data may be excused from the hearing.

Now, Dr. Oppenheimer, you may proceed, and I gather from what Mr. Garrison said, that he will at this point make a statement to the Board.

MR. GARRISON: Mr. Chairman, Members of the Board, I would like to say at the outset that far from having thought of challenging any member of the Board, we appreciate very much the willingness of men of your standing and responsibilities to accept this exacting and onerous job in the interests of thecountry. I express my appreciation to you.

We cannot help but be conscious of the fact that

for the past week the members of the Board have been examining a file containing various items about Dr. Oppenheimer to which we have had, and to which we shall have no access at all. I have been told that this is a large file, and I suppose a great deal of time has been spent on it. I am sure that it goes without saying that we are confident that the minds of the members of the Board are open to receive the testimony that we shall submit.

If, as a result of going through the file, there are troublesome questions which have arisen, any items of derogatory information not mentioned in the Commission's letter of December 23, I know we can count on you to bring those to our attention so that we may have an adequate opportunity to reply to them.

I would take note at this point of section 4.15(j) of the Rules and Regulations of the Commission, and of the second sentence, which reads, "If prior to or during the proceeding, in the opinion of the Board, the allegations in the notification letter are not sufficient to cover all matters into which inquiry should be directed, the Board shall suggest to the manager concerned that in order to give full notice to the individual, notification letter should be amended."

If there are questions that you have in mind about these possible other items in the file that you would like to

have cleared up, and shall go through the formality of the amendment of the letter, we will not press. But it would be helpful to us if we could at least be notified of any such items in a manner that would give us adequate time to study them and to prepare appropriate response.

DR. GRAY: I think you need have no concern on that score, Mr. Garrison.

MR. GARRISON: I am sure not. I would like at this point to read into the record a letter from Dr. Oppenheim to Chairman Strauss of the Atomic Energy Commission, dated December 22, 1953. I would be glad to give copies to the members of the Board.

I shall explain the purpose in a moment of reading this letter to you.

This letter is addressed to Admiral Lewis L. Strauss, Chairman of the Atomic Energy Commission, Washington, D. C., and is dated December 22, 1953, and reads as follows:

"Dear Lewis:

"Yesterday, when you asked to see me, you told me for the first time that my clearance by the Atomic Energy Commission was about to be suspended. You put to me as a possibly desirable alternative that I request termination of my contract as a consultant to the Commission, and thereby avoid an explicit consideration of the charges on which the Commission's action would otherwise be based. I was told that if I did not do this within a day, I would receive a letter

notifying me of the suspension of my clearance and of the charges against me, and I was shown a draft of that letter.

"I have thought most earnestly of the alternative suggested. Under the circumstances this course of action would mean that I accept and concur in the view that I am not fit to serve this government, that I have now served for some twelve years. This I cannot do. If I were thus unworthy I could hardly have served our country as I have tried, or been the Director of our Institute in Princeton, or have spoken, as on more than one occasion I have found myself speaking, in the name of our science and our country.

"Since our meeting yesterday, you and General Nichols told me that the charges in the letter were familiar charges, and since the time was short, I paged through the letter quite briefly. I shall now read it in detail and make appropriate response.

"Faithfully yours, Robert Oppenheimer."

I have presented that, Mr. Chairman, simply to show that there has been no disposition on Mr. Oppenheimer's part to hold onto a job for the sake of a job. It goes without saying that if the Commission did not wish to use his services as a consultant that was all right with him. The point of this letter is that he felt that he could not in honor and integrity of his person simply resign and leave these questions unadjudicated. Fully realizing the terrible burden of

going forward with this matter, and the natural risks in any proceeding of this character, including what may go on outside of these walls, nevertheless went forward.

He speaks in this letter of charges. I am glad that the Chairman pointed out that word is not the appropriate word to be used here. We recognize that fact and have noted, indeed, earlier from a letter from General Nichols to me, dated January 27, 1954, in which I in a letter to him inadvertently used the word "charges", he said, "Please be advised that we do not consider that letter,"the one of December 23, 1953, the principal letter which you read into the record,"as being a statement of charges, but rather a statement of substantial derogatory information bearing upon his eligibility for AEC security clearance."

Gentlemen, for the last several months I have been immersed in talking with all of the people I could find who had worked with Dr. Oppenheimer over the years about their recollections of his activities and their impressions of him as a man and as a citizen, and I have immersed myself in his writings and in all of the details of the case.

I would just like to say that I have been struck by the instantaneous and warm and universal support which everybody that I talked with who has worked with him has given. It will be reflected in the testimony which we will bring here before you. I shall speak a little later about

the scope of the testimony and the number of witnesses, but it has really quite impressed me.

I have also found among these gentlemen a great sense of anxiety about this case of what it may portend to the science program of the country if clearance in the end could be denied to a man who has tried to serve his country as Dr. Oppenheimer has served it; not so much a sense of what might happen to the scientists now in the government service thsmevles, although this certainly has come to them as a great shock, but rather what it may do to the young scientists to whom the government must turn in the future for aid and assistance in seeking to recruit personnel to the government.

I mention this not becasse it has any precise bearing on the action and the findings in this case, but it is a part of the warp and woof then of the feelings with which the witnesses here will address you.

The case as it has looked to me stands out in sharp feature rather simply this way, that these derogatory items in the file mostly have to do with activities of Dr. Oppenheimer that go back to 12 to 15 years ago. A few have to do with 9 to 12 years. Since the war -- since 1945 -- apart from the Crouch incident, which itself has to do with an alleged occurrence in 1941, there is in this letter of December 23 -- I think I am correct in saying -- not a single item of derogatory information except the Independent Citizens

Committee of the Arts, Sciences, from which he in fact resigned, the proffer of vice chairmanship, when he saw what it was up to and except for the hydrogen matter, which stands all by itself.

Far from being to his discredit, far from casting doubt on his desire to serve his country as best he sees how to do it, I think our witnesses will persuade you beyond any doubt that his conduct in the hydrogen bomb matter was beyond any reproach; that it was an exercise of the most honest judgment done in the best interests of the country, and that his whole record since the war is rather astonishingly filled with a continuous series of efforts to strengthen the defenses of the United States in a world threatened by totalitarian aggression.

I was surprised to find that about half of his working time since 1945 has been devoted to service on government boards and committees, from 1945 on, as a volunteer citizen, placing his talents at the service of the country. The richness and the variety of the services that he rendered in those capacities will be vividly brought out in the testimony.

I would like to say that everything he has done since the war, the hydrogen bomb and all the rest, has been done in a blaze of light. There has been not one thing that has not been done in the full daylight of the work of the government and subjected to the most searching criticism of the

ablest men in science and government, all doing each in their own way what they could do to serve the country.

I believe this record will be one which will persuade this Board that to exclude Dr. Oppenheimer from the capacity that he continue to serve the government as he has in the past would be contrary to the best interests of all of us.

Now a word about the procedure. We hope to present this case to you in terms of unrestricted data. It would be an unclassified case. We would like to present it in that direct lay fashion. I am not a scientist and except for Dr. Evans the members of the Board are not. We thought it would be best if we could avoid having to get involved in technical evidence of a very complex and difficult nature which would involve a great deal of time, and which would perhaps tend to lead us into the wrong path of exposing that the issue here is whether at a particular juncture Dr. Oppenheimer's scientific judgment was right or wrong. I am sure we all agree that the question here is not whether the advice that he gave at a particular time was from a scientific point of view one with which this Board might differ in the light of history. The real question is was his judgment an honest judgment; did he do the best he could for his government.

I was a little fearful if we got into the whole realm of science that we would perhaps lose sight of that

simple fact.

We want to on the other hand tell you as completely as we can -- and I think it can be done within the limits of classification, the proper limits we can talk about here -- exactly how the things were done which he did, and the procedures that were adopted and the way the tasks were gone about, the atmosphere in which they were conducted.

I would like to start, when we get into Dr. Oppenheimer's testimony, with a somewhat fuller account from him of his record of public service, beginning with the war years, and coming down to date.

I would like to start with that because the answer to which he gave is less complete with respect to that portion of his life. With respect to the derogatory items of the early years, we have said about all that we can say except as you of course may wish to question him further, as I have no doubt you will, with regard to them. But I would like to have Dr. Oppenheimer tell you more than he has been able to do in the encompass of the answer about the way in which he has sought to serve the country since the war.

Our witnesses will mostly be bringing testimony about that service.

When we get through with that, there are a few supplementary things to be said about these earlier derogatory items, and some documentary evidence that we want to introduce.

The witnesses that we would like to call, after you yourself have finished questioning Dr. Oppenheimer, and when he is through -- and I should say we will welcome questions as we go along, whatever you may wish to ask,as Dr. Oppenheimer testifies, and I hope you will, because I think it will make it easier for all of us if you would do that instead of leaving it all for the end -- whenever we are through and the Board is through with questioning Dr. Oppenheimer, then we would be prepared to invite a considerable number of witnesses to testify. There are as of this moment 27 witnesses whom we expect to call. There may be several more. There will also be three or four or five -- I don't know exactly how many -- written documents from some witnesses who are simply unable to get here at all.

If the Board would like, I should be glad to give you a list of the proposed witnesses, so that you may have it before you, and also a skeleton of the proposed times.

DR. GRAY: I would think that would be very helpful, Mr. Garrison, if you would. It just possibly might have some bearing on the questions that might be put to Dr. Oppenheimer.

MR. GARRISON: We will, I think, bring that in after lunch. It is not quite ready for presentation.

DR. GRAY: Very well.

MR. GARRISON: I would like at this time to give

you, and I hope you might perhaps keep this handy, an exhibit called, "Biographical Data on Dr. J. Robert Oppenheimer."

DR. GRAY: Are you offering that now, Mr. Garrison?

MR. GARRISON: Yes.

DR. GRAY: Would you mark the Oppenheimer Exhibit No. 1, and we will received it for the record.

(THE DOCUMENT WAS MARKED OPPENHEIMER EXHIBIT NO. 1 AND RECEIVED FOR THE RECORD.)

MR. GARRISON: I would say to the Board that if you will turn to the first page I, this is a concise summary of the major steps in Dr. Oppenheimer's career. I t will be a handy guide for use when witnesses are testifying to particular committees or whatnot to see just at what stage they call.

Turning to the next page, II, you will see listed the various government committees on which Dr. Oppenheimer has served, with the dates of service and the people who served with him on these various undertakings. This is since Los Alamos.

DR. GRAY: May I interrupt? Just in the interest of keeping the record precisely clear, I wonder if that last statement is quite correct, because I believe you terminated your association with Los Alamos in the fall of 1945, and some of these committees overlap.

MR. GARRISON: You are correct.

DR. GRAY: I am not making it any more a particular point other than --

MR. GARRISON: I appreciate the correction. I should make a further correction, that this is a partial list of the committees on which he served. They are the principal ones, the ones about which we shall mainly be talking here at the hearings.

Beginning with III and running all the way over is a detailed biography in which, to the best of our ability, we have put down year by year every association of which we have any record of his having joined or been a member of, every publication of his, every position that he has held on committees, either private or public, lectures that he has given, addresses. This is the outward and visible account of his activities, in short, as best we could compile them. If there are inaccuracies, they are entirely inadvertent.

One of the things that struck me as I went over this biography, which I asked to have prepared, was the quite evident fact that during the prewar years in which most of these derogatory items arise,his energies were quite strongly devoted to almost entirely really his scientific work and scientific undertakings. They reveal really very little in the way of political interest or associations on his part just on the face of the record.

In the postwar period this becomes again apparent, and I would direct your attention to the fact that in this postwar period, I donot think there is a single association of his that can possibly be questioned as derogatory by the Commissbn or by this Board, or anything, indeed, other than a rich record of association and devotion to his science and his service to the government, and his membership in various scientific and civic organizations of the highest standing.

There is, of course, also that notation about the Independent Citizens Committee of Arts and Sciences, but that, as he stated in his answer, he withdrew from because of its policy in replying to their proffer of an officer's position in the association and indicated his complete lack of sympathy with the kind of policies which it stood for.

I am going to, in the course of the testimony, introduce in evidence at pertinent places extracts from some of Dr. Oppenheimer's writings and addresses from the period 1945 to date. To attempt to introduce them all here would be beyond the obvious scope of this inquiry. But I want to assure the Board that you will find a very consistent and very striking thread of continuous thought on Dr. Oppenheimer's part with respect to the strengthening of the defenses of this country, with respect to what has to be done to counter the Russian threat from abroad, with respect to building the strongest and soundest democratic America

that man can do, consistent, unvarying and very impressive.

This whole postwar period, everything in it, is really utterly inconsistent with any notion that this man could have been anything but a devoted suporter of the American system that we love.

I think that is all that I have to say of a preliminary character, Mr. Chairman. I appreciate your leeting me say it. I think perhaps this would be an appropriate point to adjourn, and shall we come back at 2:30? Is that your schedule?

DR. GRAY: Perhaps it should be a recess, I do not know, Mr. Garrison, and not an adjournment. I am sure we want to try to meet the convenience of everyone concerned, and at the same time not to waste hours or minutes which could be useful in getting ahead with the inquiry.

Speaking for the Board, I am sure we could be ready at 2 o'clock but I do not want to press you and Dr. Oppenheimer.

MR. SILVERMAN: I was suggesting that we could use a little extra time.

DR. GRAY: Would you suggest 2:30?

MR. GARRISON: Let us say 2:15.

DR. GRAY: All right.

MR. GARRISON: Before the recess, I want to read this into the record. This is a letter to me from Mr. William Mitchell, General Counsel, dated January 15, 1954, that the

that the Commission will be prepared to stipulate as follows for purposes of the hearing:

"On August 6, 1947, the Commission recorded clearance of Dr. J. Robert Oppenheimer, which it noted had been authorized in February 1947."

What that has reference to, as we will show more fully in the testimony, was the fact that in 1947 Dr. Oppenheimer's personnel file was sent tothe Commission by Mr. J. Edgar Hoover, with the request that it be reviewed. This was at the time of the early days of the establishment of the Commission, and Dr. Oppenheimer had been appointed to the General Advisory Committee, and had been elected its chairman. The Commission considered this entire file, which I believe the evidence will show contained substantially all that you have before you in the letter of December 23, except the Crouch incident, which occurred afterwards, though it related to something in 1941, and except for the hydrogen bomb matters, and the Commission unanimously after discussing Dr. Oppenheimer's qualifications with many of the leading people who had had to do with him in the past and with officials of the government reached the view that there was no question as to his clearance. That we will show later by testimony. I merely mention now that will be before you. I don't mean to import what I said into the stipulation which goes in front of what I have just said.

DR. GRAY: We will now recess until 2:15.

(Thereupon at 12:20 p.m., a recess was taken until 2:15 p.m., the same day.)

AFTERNOON SESSION 2:15 P.M.

DR. GRAY: We will begin the proceedings at this point again. Let the record show that Mrs. Oppenheimer is not present this afternoon.

Whereupon,

J. ROBERT OPPENHEIMER

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

DIRECT EXAMINATION

BY MR. GARRISON:

Q Will you tell the Board what your present position is, Dr. Oppenheimer, at Princeton?

A My job is Director of The Institute of Advanced Study. For the most part this is not relevant to the hearing, but I will outline briefly some of the circumstances.

The Institute is not part of Princeton University. It is a separate institute, very highbrow. It has about 130 members who are post-doctoral. Some of them are youngsters just out of graduate school; some are men of 50 and 60.

We try, though only in part, to patronize scholarship and science; science in the old sense of the word, meaning both the natural science and the humanities. I think the parts that are relevant to the welfare of the United States are fairly limited. We have a very good training ground for various students in pure mathematics, applied mathematics and

theoretical physics. Many of them who are at the Institute are supported by the United States Government, many more go into work for a limited time or for a longer time on behalf of our government.

We have a number of governmental undertakings. I think one of the more interesting is that we seem for the first time to be able to predict cyclogenesis, the generation of storms. This is of practical value, and the government has instituted a program based on the research started at the institute.

We have one other function which I believe to be important at this time. We are as much as we can be with our limited resources an open house to scholars throughout the free world, from Europe, from Japan, from India, mostly, of course, from Europe. I think more than half of our people are from outside the United States. I think we go a long way toward persuading a very small fraction of the people abroad that the United States is a humane and civilized place, and programs about the Institute have been carried by the Voice of America, and in State Department bulletins, and I beleve that the ill-fated glossy magazine that we put out in the Soviet Union, called "America", published an account of our work.

In any case, there are many people in the government who are proud of what we are doing, and I am proud of it.

Q Do you have occasion to use classified material at the Institute?

A The Institute has never accepted -- I don't know how the Board of Trustees would respond -- a classified contract. It has never been asked to accept one. There is work going on at the Institute which is very close to classified work, but by the time it is fed in to us, it is unintelligible, and therefore declassified.

Individual members of the Institute, of course, have done classified work. I am an obvious example. George F. Kennan is an example. von Neumann is an example. I won't reel off the list of names. This is an affair between the individual and the government. The Institute interposes no objection.

Every year I get a letter from Los Alamos, "Do you object if we renew the contracts of these people", and I say that it is up to them and up to you.

Q What security methods have you used at the Institute in connection with your own classified materials in the past?

A They have been very elaborate. When I came to Princeton, The Atomic Energy Commission established a top secret facility. I need not describe the rigamarole that goes into this, the warning systems and all the rest. There is a vault there. It has been moved recently, but it still is

at the Institute. I have never known the combination. The combination has been rigidly guarded. I believe our record is that we have never even displaced a document. I hope this continues to be true. That facility is still in existence for the benefit of other people who wish to use it.

Q When did you come to the Institute at Princeton?

A I came in the late summer, I think, of 1947. I had been a professor at California Institute of Technology and at the University of California at Berkeley. In late 1946 perhaps or early 1947, the present Chairman of the Atomic Energy Commission was Chairman of the Nominating Committee to seek a new director to succeed Dr. Aydelott at the Institute and he offered me the job stating that the trustees and the faculty desired this.

I did not accept at once. I like California very much, and my job there, but I had, as will appear, not spent very much time in California. Also, the opportunity to be in a small center of scholarship across the board was very attractive to me. Before I accepted the job, and a number of conversations took place, I told Mr. Strauss there was derogatory information about me. In the course of the confirmation hearings on Mr. Lilienthal especially, and the rest of the Commissioners, I believe Mr. Hoover sent my file to the Commission, and Mr. Strauss told me that he had examined it rather carefully. I asked him whether this seemed to him

in any way an argument against my accepting this job, and he said no, on the contrary-- anyway, no -- in April I heard over the radio I had accepted, and decided that was a good idea. I have been there since.

Q You said you had not spent much time in California. That I take it was because of your engagement in public service in a rather continuous way?

A Yes.

Q That leads, I think, naturally into a discussion of your record of public service, and I would like to begin, Dr. Oppenheimer, with the war years, and have you tell the Board how you happened to get involved in atomic bomb work.

A In the autumn of 1941 I was asked by Arthur Compton to attend a session of the Special Committee of the National Academy of Sciences, which had been set up to study the military uses of fission, the uranium project. I think that committee had other meetings. I attended a two day meeting. At that time -- I need not go into details -- I took an active part in the discussion.

Q What was your position?

A I was professor of physics at the University of California. I took an active part in the discussion primarily, I think, to be sure that the open questions were recognized as open and some sketch of a program understood. I believe everyone there was quite clear that we had to go ahead with this.

The next step was double. On the one hand, Earnest Lawrence, who was Director of the Radiation Laboratory at Berkeley, had on the trip to this meeting become more and more enthusiastic about the prospects for an electromagnetic separation of uranium isotopes, and we talked about that the whole way. When he got back, he started getting other people thinking about it, and I became a sort of advisor or consultant without appointment to that undertaking. I don't remember just when, but some time in the course of the next few months I had an idea which turned out to have been useful. It was not decisive, but it perhaps doubled or tripled the capacity, or halved orthirded the price of the plant they were building.

I met with them quite often at their Steering Committee and Coordinating Committee meetings, but never as an employee. I was still teaching and in fact teaching more than usual, because other people had gone off to work on radar and we were very badly understaffed.

Other things that I began to think more intensively and on my own about how to make atomic bombs and made some calculations on efficiency, design, probably amounts of material and so on, so that I got into it,,and knew something about it. The result was that when I was called probably in the first days of 1942 -- anyway after Pearl Harbor -- to Chicago, I was able to give a little information about this aspect of

the problem. The main thing the Chiago people were up to was building reactors to make plutonium, or trying to see if they bould build reactors to make plutonium. But in the original assignment of the responsibility, work on fast fission, which is what they used as a shorthand for the bomb, was also part of their job.

The man in charge of it was Gregory Breit. He had the wonderful code name of Coordinator of Rapid Ruptnne. He wrote me some time in the spring, suggesting that we might have a conference in Minneapolis, that he was interested in work I had been dding, and perhaps might even want to come longer to Minneapolis. This never matured. I think Breit quit in June, I believe it was.

I went on to Chicago at Arthur Compton's request. I made arrangements to have Bethe and Teller and a few other people meet and also the heads of the sub-undertakings that were trying to make measurements relevant to the design of bombs and specifications of bombs, and we had conferences for some days.

Fairly early in the game, Arthur Compton said would I take charge of this part of the work and I agreed to do so.

We also agreed that at that moment the job fell into two parts. One was the job of analysis and thinking, of theory, and we would set that up as a summer study in

Berkeley. The other was to try to get some sense into the distorted and fragmentary work that was going on in a number of laboratories. There was a third part which was to get some new projects started.

Q You spoke about the fragments and the scattering. I take it that was one of the factors that led you to think in terms of what ultimately was to become Los Alamos?

A It did not go quite so fast. We spent this summer in study, and I traveled around and saw most of the laboratories. I had very good help from John Manley, who is not Jack Manley. We had a very heavy study, and began to see what was involved, not all of it, I would not say all of it until much later, and also spent a fantastically large fraction of our time on the thermonuclear program. That is the first time we really got into it.

What we then saw of the thermonuclear program was not very relevant to what you are reading in the papers today. But it excited us, and it seemed to make even more necessary that we understand what this was all about.

After our conferences were over, I went and reported to Compton who was off on a summer holiday about this aspect of it, as well as others. I then came on, I think, at his request, and saw Dr. Bush and told him about it. We also at about this time prepared a report on our views for transmission to the British.

There was a fairly complete interchange. We did not write about the thermonuclear program, but we wrote about some of the subtleties of the atomic bomb program.

Then we began to notice how very much needed doing and how much the little laboratories were suffering from their isolation.

There was supposed to be security; anyway, there was good compartmentalization and the result was that people would not know what was going on anywhere else. Work was duplicated, and there was almost no sense of hope or direction in it.

By the fall of 1942, not only the theoretical people but anyone who knew the experimental situation realized that this had to be pulled together. It was not the first job. The first job was to make the stuff. But in hope that would come out all right, we had to have a place where we could learn what to do with it. Thiswas not trivial. We therefore started chattering about should we have a laboratory in Chicago, should we have one at Oak Ridge. The prevailing notion was that there would be more or less a conventional laboratory until such time as we were really ready to get into almost ordnance experiments, and then we would go uout and get a proving ground somewhere, which would be rather remote and a very few people involved.

This did not seem sound. It seemed to me and

knowledgeable people, it was one package, ordnance, chemistry, physics theory, effects, all had to be understood together or the job would not get done. These were the considerations that led me to say to General Groves, who had by then been brought into the project as its head, that I thought a bomb laboratory was a good idea. That I thought it needed to have two characteristics; one, that it be free internally to talk about problems from one part of the job to the other, and that its external security be very, very good indeed, that it be isolated, if necessary guarded, and all the rest of it.

General Groves was very much interested from the beginning. I think I had a message from him to come on down to discuss the matter, and I remember that he and Colonel Nichols, and Colonel Marshall, and I got into some very limited place on the Twentieth Century Limited and talked about plans for such a post.

The original plans were much too small. They had in mind that it might be a useful thing if the key personnel of the laboratory were commissioned. I at that time very foolishly I think had no objection to it. I would have been glad to be an officer. I thought maybe the others would. But it was not very long before I talked to people who had to come to Los Alamos, especially those who had experience in radar and in military research, and they explained that

it was hopeless to superimpose on a natural technical organization of a laboratory the irrelevant and sort of additional organization of the military establishment.

We had a long hassle about that. I think everyone agreed. In a letter which reached me early in 1943, signed by Groves and Conant, it was agreed that initially the workers in the laboratory would be civilians. It was contemplated that later at the more critical phases the key people would be commissioned. That plan was dropped, I think essentially because the numbers got so big and there was no need for it, and it became impractical.

About this time, in the autumn of 1943, Groves sent an engineer around to look for a place. He was around in the southwest where I knew the country and in New Mexico, and I showed him and showed General Groves the City of Los Alamos. This appealed to General Groves very much, and he moved with unbelievable dispatch to acquire it, and we started construction.

It may be of interest to you that one of the first buildings built, and one of the first projects that we started was a measurement of the properties of tritium, which is a conceivably important part of the thermonuclear program, and one of the first buildings built at Los Alamos had as its purpose the handling of materials that we thought might be of interest in thermonuclear work.

We put up a laboratory and a lot of houses which were hopelessly inadequate to our future needs, but at least did get us started. The real problem, of course, had nothing to do with that. It had to do with persuading people to come there. I think it true that at that time among scientists engaged in immediate military problems, radar, underwater sound, ordnance and so on, the name of the uranium project was not good, because work had been going on for a number of years without very much sense of direction. There was great fear that this was a boondoggle, which would in fact have nothing to do with the war we were fighting. Very, very few people turned us down coming to Los Alamos, but this was work for everyone. I think it was perhaps most work for me. I got a large group from Princeton, many people from the chemistry group in Berkeley, where we recruited the key chemical personnel. A group from Stanford. I won't bore you with the details of this. But it took from perhaps October or November 1942 until March of 1943 to get the rudiments of a laboratory. We stole a cyclotron from Harvard, some accelerators from Wisconsin. Everybody arrived with trunkloads of junk and equipment, and in this way we were able to be doing experiments -- well, I got to Los Alamos toward the end of March, the equipment started coming a few days later, and by June we were finding out things that nobody knew before. That we thought was a fairly good record

of speed.

We had a general notion at that time that all the work of the laboratory would be open to all the scientific members of the laboratory. This is a matter which General Groves, I think, concurred in, but which he never entirely liked. In other words, within the laboratory the competent people were supposed to know what the story was. It turned out over and over again this was a wise policy. Good ideas came from places that you would not have expected. Enthusiasm and understanding could be generated because people knew what it was all about.

On the other hand, we communicated very sparingly and through quite restricted external channels with other parts of the Manhattan District, the places that were making the materials, and the other laboratories, and I should say not at all or almost not at all with any other military research establishments, except those from whom we needed gear. We had some really fantastic security provisions. They were not in the end effective as we know. Families were supposed to come with their husbands if they wanted to, but they were not allowed to leave. We did have to let a couple of people leave the project, but the onus of doing this was very great and the pressure against it very great. We had all our phone calls monitored. It was illegal to mail a letter except in the authorized drops and ingoing and outgoing mail was

censored.

Our names were not known and our drivers licenses were all made out under fictitious or artificial names. The laboratory was guraded within the post and the post was gumrded. We went to precautions which did not do the trick, but which looked very formidible at the time.

I had partly the job of devising these idiotic things and partly the job of making them welcome. I engaged in several speeches why these precautions were necessary and desirable. I think I took most people along pretty well so that there was not too much kicking about the security regulations. I think we may have a letter which President Roosevelt wrote to me for the laboratory, I should think, and which gives some people two aspects of it. It was a sort of official statement that the security provisions, however irksome, were justified, and the other was that we better get on the job. We had enemies who might be up to it, and we better beat them to the draw. Shall we simply submit this?

MR. GARRISON: I would like to read this letter into the record at this point, if I may. This is a letter from President Roosevelt to Dr. Oppenheimer, under date of June 29, 1943:

"My dear Dr. Oppenheimer:

"I have recently reviewed with Dr. Bush the highly important and secret program of research, development and

manufacture with which you are familiar. I was very glad to hear of the excellent work which is being done in a number of places in this country under the immediate supervision of General L. R. Groves and the general direction of the Committee of which Dr. Bush is Chairman. The successful solution of the problem is of the utmost importance to the national safety, and I am confident that the work will be completed in as short a time as possible as the result of the wholehearted cooperation of all concerned.

"I am writing to you as the leader of one group which is to play a vital role in the months ahead. I know that you and your colleagues are working on a hazardous matter under unusual circumstances. The fact that the outcome of your labors is of such great significance to the nation requires that this program be even more drastically guarded than other highly secret war developments. I have therefore given directions that every precaution be taken to insure the security of your project and feel sure that those in charge will see that these orders are carried out. You are fully aware of the reasons why your own endeavors and those of your associates must be circumscribed by very special restrictions. Nevertheless, I wish you would express to the scientists assembled with you my deep appreciation of their willingness to undertake the tasks which lie before them in spite of the dangers and the personal sacrifices. I am sure we can

we can rely on their continued wholehearted and unselfish labors. Whatever the enemy may be planning, American science will be equal to the challenge. With this thought in mind, I send this note of confidence and appreciation.

"Though there are other important groups at work, I am writing only to you as the leader of the one which is operating under very special conditions, and to General Groves. While this letter is secret, the contents of it may be disclosed to your associates under a pledge of secrecy.

"Very sincerely yours, Franklin D. Roosevelt."

BY MR. GARRISON:

Q This, I gathered, was in connection with your own efforts to impress upon the group the parmount needs of security and the importance of the work they were doing.

A The importance I think there was very little doubt about. Everybody who was there who was a scientist knew it was important. We had a great deal of trouble with people who were not given information, with technicians, machinists, and so on, who found the conditions of life very disagreeable and no countervailing advantage of being associated with something they understood. But the scientists knew it was important.

Q You were under a great deal of time pressure, were you? Was there a sense of urgency in the air?

A My directive, I haven't got it, it is probably at

Los Alamos as part of the record, was to lose no day in preparing an atomic bomb. The definition of an atomic bomb was that it should be at least equal to 1000 tons of TNT in explosive force. This sense of pressure started at the beginning and never let up. I will come in a moment to how it was at the end.

If you want to ask anything about it, please do not hesitate to interrupt me.

DR. GRAY: Yes. I think, Dr. Oppenheimer, we would prefer for you to go ahead. I do not want to say that no member of the Board or counsel will not interrupt, and I think we are free to do so, but I think we would like you to proceed and if there is anything, we will inquire.

THE WITNESS: Fine. We started out the job there with two sets of meetings. One was a large meeting --

DR. GRAY: When was this?

THE WITNESS: This would have been April 1943. A large meeting that I called all the people there in and a number of others whom I hoped to lure there, and many of whom were in fact later to come, to discuss the technical program. The other was a review committee that Groves appointed, more or less to find out what we were up to and to see that we were doing and what we were not doing. One of the things that the review committee recommended was that we immediately get into the ordnance problems.

This is something we felt very strongly. We immediately got into large scale chemical and metallurgical problems.

Another thing they recommended was that I not try to do everything myself, but get a personnel director, and some aides so that the place would run a little bit better.

We were building a town at the same time that we were building the laboratory. The program of the laboratory emerged from the technical meetings, and so did many of the people whowere later to come there and play prominent parts. Some of them are probably going to appear before you. Bethe an enormous, robust and talented theoretical physicist; Aemiral Parsons, who was the head of the Ordnance Division and is now gone. Fermi, who came in rather late and became an associate director and who among other things was in charge of those activities of the laboratory which were directly not relevant to the atomic bomb but looked further ahead. Bacher, who was in charge of one aspect of the physics of the bomb, and who will appear later. Cyril Smith, ~~Saerias,~~ ZACHARIAS Hartley Rowe, who after he got back from General Eisenhower in Normandy landings advised us on engineering problems and helped set up the Sandia laboratory which has played such a large part since that time.

BY MR. GARRISON:

Q That is Mr. Rowe of the United Fruit Company?

A Yes. Norman Ramsey, who was Parson's deputy. I mentioned some of the names of people who will appear here probably.

My job, I don't think too much should be made of it. It was the job of being sure that people understood and that the decisions were properly made, and there were many not easy decisions. We did this through a system of groups, divisions and coordinating councils and a steering committee which finally made the determination of laboratory policy. Sometimes on trivial things like on did we need another housing development, sometimes on very serious things which if made wrong would in fact have prevented our doing the job. We had quite complex relations in which Admiral Parsons was very helpful with the military services who in the end had to deliver this thing, and had to train for delivering it, and had to be sure that they knew all about it. We had to agree with them about the hardware, and be sure that the hardware we were developing would be useable by airmen when they were actually involved in it.

We had the problem of relations with the British. Bacher and I were called on to discuss with Chadwick and Peierls ~~Pyrles~~, the state of the British program and where a British mission was established at Los Alamos under the leadership of Chadwick, who is very, very famous and very forthright British scientist, a mission of some 20 people, extremely good.

We had the problem of relations with the laboratories and plants that were providing us with military, the question of the specifications of the material and who was to do what. We had the normal administrative problems of a job that was quite unfamiliar, not as dangerous as President'Roosevelt's letter indicated, but still capable of great danger as accidents occurring shortly after the war showed. It was very new and terribly exciting.

We had the job of keeping this rapid expansion and with the very end large group of brilliant individualistic and talented people in harmony and pulling on the same team. We had people there who were refugees from Germany and Italy. We had Englishmen, we had lots of Americans. It was in a funny way an international effort.

I need to say that it was not an international effort including Iron Curtain countries. I guess in those days there was only one iron curtain country. In a visit during the summer of 1943, Colonel Lansdale, head of Manhattan District Security, in a talk, I think, to the key personnel of the laboratory, made it very clear how great weight the government attached to maintaining this operation secure against Russian espionage or Russian intelligence.

Q As the work progressed, you began to get goals and deadlines, I suppose, against which to produce the bomb, if you could?

A The deadline never changed. It was as soon as possible. This depends on when we were ready, when the stuff was ready, and how much stuff we needed.

Q Wasn't there a particular effort to get it done before the Potsdam Conference?

A Yes, that was of course quite late. After the collapse of Germany, we understood that it was important to get this ready for the war in Japan. We were told that it would be very important -- I was told I guess by Mr. Stimson -- that it would be very important to know the state of affairs before the meeting at Potsdam at which the future conduct of the war in the Far East would be discussed.

Q Discussed with the Russians?

A I don't want to overstate that. It was my understanding, and on the morning of July 16, I think Dr. Bush told me, that it was the intention of the United States statesmen who went to Potsdam to say something about this to the Russians. Inever knew how much. Mr. Stimson explained later that he had planned to say a good deal more than what was said, but when they saw what the Russians looked like and how it felt, he didn't know whether it was a good idea. The historical record as it is published indicates that the President said no more than we had a new weapon which we planned to use in Japan, and it was very powerful. I believe we were under incredible pressure to get it done before the

Potsdam meeting and Groves and I bickered for a couple of days. But in actual time it has been done enough times. There havebeen enough lurid news stories about that first test so that I need not repeat what it was like. In other context it should be said that it was as successful as we had any reason to hope, and I believe we got the job done as fast as we could. That is what we were told to do.

MR. GARRISON: At this point I would like to read into the record a letter from General Groves --

MR. ROBB: May I inquire, Mr. Garrison, these are copies, but you have the originals available?

MR. GARRISON: We have the originals available and we would be very glad to show them to you.

MR. ROBB: Thank you.

MR. GARRISON: This is the letter of July 19, 1945, from General Groves and Dr. Oppenheimer reading as follows:

"Since I returned to Washington I have done little else but think about and talk about the truly magnificent result of the test conducted at Trinity last Monday morning."

Trinity was the code name for the place.

THE WITNESS: Yes.

MR. GARRISON: "As time goes on and the test begins to take on its true perspective, I appreciate more and more the outstanding performance of you and your people in making the test so successful.

"General Farrell and I have discussed the project in all its many phases and have reviewed it from every possible angle. We both feel that the job is a high water mark of scientific and engineering performance. Your leadership and skill and the loyal and able performance of all your subordinates made it possible.

"An immediate report was cabled to the Secretary of War on Monday on the great performance.--"

That would be to Potsdam, I take it?

THE WITNESS: Yes.

MR. GARRISON: --" He promptly cable d back heartiest congratulations to all concerned. This morning a fuller written report was sent to him by special courier and he should have our impressions of the test by the name you get this letter. I know that The President, The Secretaries of State and War and General Marshall who are so importantly engaged at Potsdam now will be as tremendously impressed as we were by the results of the test.

"I hope you will show or read the suitable parts of this letter to the men who did so much to make the job go so well and that you will extend to them my grateful thanks for a job well done.

"Again, with deepest thanks and every good wish for the continued success of our great project from both General Farrell and myself, I am, Sincerely yours, L. R. Groves, Major

General, USA."

THE WITNESS: Now, there are a few points I might make about this period. After the test but before the use of the bombs in Japan, I had a meeting with General Groves in Chicago to get some last minute arrangements fixed for the combat use of the weapon. I asked him at that time, how do you feel about this Super -- the Super was our code name for what we then thought of the hydrogen bomb, and we don't know any more than we did when he came up, there was a little work but very inconclusive. As a matter of fact, the decisive measurements on the behavior of tritium were on my desk when I got home --

DR. EVANS: What, sir?

THE WITNESS: The decisive measurements on the tritium -- these are declassified now, as you know -- were on my desk when I got back from Trinity. General Groves was unclear whether his mandate and therefore mine extended to fiddling with this next project. I so reported to the people in the laboratory, who were thinking about it.

The second point I would not think to mention except that Mr. Garrison has asked me and that is whether there was any change in tempo after the war against Germany ended. There was, but it was upward. It was upward simply because we were still more frantic to have the job done and wanted to have it done so that if needed, it would be available.

In any case, we wanted to have it done before the war was over and nothing much could be done. I don't think there was any time where we worked harder at the speedup than in the period after the German surrender and the actual combat use of the bomb.

The third thing is that I did suggest to General Groves some changes in bomb design which would make more efficient use of the material, and they have long since been done, of course. He turned them down as jeopardizing the promptness of the availability of bombs. He and I may not entirely agree about how long a delay would have been involved, but the very fact that any delay was involved was unacceptable.

Finally, there was, of course, a great deal of discussion -- and I will return to the formal aspects of that -- about the desirability of using the bombs in Japan. I think the hotbed of this discussion was in Chicago rather than in Los Alamos. At Los Alamos I heard very little talk about it. We always assumed if they were needed, they would be used. But there were places where people said for the future of the world it would be better not to use them.

This problem was referred to me in a capacity differer than director of Los Alamos. We did everything we could to get them out there and as fast and smooth as possible.

There was, however, at Los Alamos a change in the feel of people. I am talking vaguely because this is a

community now of 7 or 8 thousand people, of whom maybe 1,000 or more are scientists and very close to each other, talking all the time. This was partly a war measure, but it was also something that was here to stay. There was a great sense of uncertainty and anxiety about what should be done about it.

The generation of that kind of public -- of a concern very similar to the public concern -- that followed Hiroshima and one natural outgrowth of which was our abortive effort to establish quite a new relation among nations in the control of atomic energy, that was not something that had its roots very far back, it started toward the end when the war was about over.

Hiroshima was, of course, very successful, partly for reasons unanticipated by us. We had been over the targets with a committee that was sent out to consult us and to consider them and the targets that were bombed were among the list that seemed bright to us.

The Secretary of War deleted one target, and I have always been glad he did. That was the unbombed and culture capital of Japan, Kyoto. He struck that off. The two that were hit were among the targets selected. We sent a mission on out from Los Alamos to assemble, test the bombs on Tinian, and to fly with the B-29's that went out over the targets, and also to go in as soon as they could get clearance

from General MacArthur.

That mission was under General Farrell who might appear, I am not sure he can, to see what mess we made of those two towns.

When the war was over we came east,, Dr. Bacher, Dr. Rabi and I together. There was a rumor of some wonderful method of getting energy for nothing that the General Electric research people had discovered. Groves thought I ought to have a look at it. It turned out to be nonsense. In the course of this visit I talked with General Groves. There were at least two points that I ought to report.

One was that I told him that as I had earlier suggested in outlining what the future work of the laboratory would be, I thought I should not continue as director. I was the director of an emergency. This was going to be something different, and I would not be the right person to preside over the change or the new effort.

In addition, there was not much left in me at the moment. We talked about my successor. This was not a trivial problem. It took a while. I talked to Commander Bradbury, I talked to General Groves. Everyone was pleased with that and I think it was a very fine selection. I was therefore free to resign and did mid-October, October 16th or something like that.

The other thing is that General Groves told me very

briefly that he had been told by Governor Byrnes, Justice Byrnes, I guess, who was then I think representing the President on the Secretary of War's Interim Committee, that with things as they were, the work at Los Alamos ought to continue, but this did not apply to the "Super" or didn't think this applied to the Super.

I don't know whether I left out some things that would be illuminating. This is not a very vital part of our story from the point of view of the case, and I would like to get on.

MR. GARRISON: I happen to have here, Mr. Chairman, the original of the United States of America Medal for Merit Awarded to Dr. Oppenheimer, and I would just like to read it. It would only take a second. The citation is signed by President Truman to Dr. Oppenheimer "for exceptionally meritorious conduct in the performance of outstanding service to the War Department, in brilliant accomplishments involving great responsibility and scientific distinction in connection with the development of the greatest military weapon of all time, the atomic bomb. As director of the Atomic Bomb Project Laboratory in New Mexico, his initiative and resourcefulness, and his unswerving devotion to duty have contributed immeasurably to the successful attainment of the objective. Dr. Oppenheimer's accomplishments reflect great credit upon himself and upon the military service." Signed,

"Harry Truman."

I am sorry I didnt have a copy for you to follow.

MR. ROBB: That is already in the file.

MR. GARRISON: This is January 12, 1946.

DR. GRAY: You wish to read that in the record?

MR. GARRISON: Yes. I think that is enough for the war period. I think we will now swing into the postwar problems that arose immediately out of the war, and the way in which they involved Dr. Oppenheimer in the service of the country.

BY MR. GARRISON:

Q You went back to Berkeley, of course, or you went back to Pasadena after you left Los Alamos.

A We are not quite so far.

Q What did you want to say previous?

A In May I was asked to serve on the Interim Committee which Mr. Stimson set up.

Q This prevented your leaving.

A Yes, this was before I left Los Alamos. Lawrence, Fermi and Arthur Compton were the other members of this panel. We met with the Interim Committee I think on the 1st of June -- I am not certain -- of 1945 for a very prolonged discussion which was attended by all members of the committee, all members of the panel and for most of the time General Marshall.

Apart from trying to make as vivid as we could the

novelty, the variety and the dynamic quality of this field, which we thought very important to get across, that this was not a finished job, and there was a heck of a lot we didn't know, much of the discussion resolved around the question raised by Secretary Stimson as to whether there was any hope at all of using this development to get less barbarous relations with the Russians.

The other two assignments which the panel had, one was quite slight. We were asked to comment on whether the bomb should be used. I think the reason we were asked for that comment was because a petition had been sent in from a very distinguished and thoughtful group of scientists, "No, it should not be used." It would be better for everything that they should not. We didn't know beans about the military situation in Japan. We didn't know whether they could be caused to surrender by other means or whether the invasion was really inevitable. But in back of our minds was the notion that the invasion was inevitable, because we had been told that. I have not been able to review this document, but what it said I think is characteristic of how technical people should answer questions.

We said that we didn't think that being scientists especially qualified us as to how to answer this question of how the bombs should be used or not; opinion was divided among us as it would be among other people if they knew about

it. We thought the two overriding considerations were the saving of lives in the war, and the effect of our actions on the stability, on our strength and the stability of the post war world. We did say that we did not think exploding one of these things as a firecracker over a desert was likely to be very impressive. This was before we had actually done that. The destruction on the desert is zero, as I think Mr. Gray may be able to remember. He had seen all these tests.

The other assignment brought me and the other members of the panel to Washington. They asked us to produce a prospectus about what needed to be done in atomic energy. We wrote a great big book. We called in all sorts of people -- Allison -- well, there is a list somewhere about -- I won't try to remember -- Allison, Rabi, Lawrence, Thomas -- and tried to give as good an account of where the problem stood as we could.

This included the military applications. There was a special chapter on the thermonuclear problem written by Fermi, on the delivery problem, making weapons that were less clumsy than the ones we had, on the use of atomic energy for power, and its use for propulsion, its use for instruments of scientific investigation, neutrons and radioactive tracers. Anyway, it was a fairly big fat book. I suppose it is from that that the remark is quoted on the feasibility of the Super that is ascribed to m e in 1945. In any case that would

have been my summary view of it at that time.

In connection with writing this report, I became involved in other activities here in Washington. The War Department was anxious to get legislation passed so that the atomic energy enterprise was not part of its budget and responsibility. General Marshall talked to me about it, and Mr. Harrison, who was Mr. Stimson's aide, talked to me about it and others as well. The matter seemed to be a bit stuck because on the one hand it was difficult to present legislation on the domestic control of atomic energy without saying whether you were going to do anything toward seeking an international control of some kind.

On the other hand, the State Department was not quite clear what it wanted to say about this for very understandable reasons. Therefore, I was asked to consult with Mr. Acheson and eventually with Mr. Byrnes and the purposes of my visits were double. One was to explain how important it was for the survival of any atomic energy enterprise at all that there be some legislation and soon. That the people who were working on the job had some assurance of where they were going. And the second was to urge that in so far as it could be with safety done, we explore the possibility of international control.

I did that as I say with Mr. Acheson and Mr. Byrnes. Then I went back to Los Alamos. We turned in our report from the

Interim Committee. I was called back to testify on a matter not directly connected with the atom, and that was a pair of bills to set up a National Science Foundation by the Joint Committee called the Kilgore-Magnuson Committee. I did so testify and they asked me what the relation between the atomic energy undertaking and the National Science Foundation should be, and I think this is the first time I had public occasion to talk about the importance of unplanned and unprogrammed scientific work, the enormous importance of training scientists, the importance of freedom in scientific world as opposed to the need for programmatic and concentrated work on practical problems.

The next day I went up before Representative May's committee which was considering the May-Johnson Bill. The May-Johnson bill was the outgrowth of the effort to get legislation adopted. The President had stated that he would seek international control, first talk with our allies, the British and Canada, and other nations, and he was considering a measure which would at least put our domestic house in order. This bill had been introduced in the House and Senate simultaneously. Hearings were being held on it in the House. Most scientists and I think all the liberal press was very mad at this bill. It wounded repressive. It had severe penalties for revealing information. It gave the Commission that was to handle the atom rather wide and rather undefined

powers. I had a lot of confidence in the people who had drafted it, and the people who would be administering it, and I testified in favor of it as an interim measure, because I thought the sooner this got into organized hands, the better chance that places like Oak Ridge and Los Alamos would be taken good care of, and after a year there would be plenty of chance to amend the legislation with whatever one had learned in between.

The newspaper PM had on the basis of my testimony the day before made one of their cartoons "Hats Off",on the basis of my testimony on this bill put in another cartoon "Hats On". They didn't like it.

After that I went with Patterson -- I think before this Stimson had left Washington. I saw him on the last day he was in office here, and he had indicated to me on that day that he thought it right and necessary to see if we could work out an international agreement on the regulation of the atom -- I went with Patterson to talk to President Truman about it. He told me that he had invited King ~~at~~ AND Atlee to come and they would shortly be getting into it. By this time I moved to Pasadena.

I took up a job there as professor of physics. I did actually give a course, but it is obscure to me how I gave it now. The intention was to make that quite a full time job, and settle in Pasadena at least for that year. I still

had the appointments at the University of California at Berkeley and the California Institute of Technology at Pasadena. I was called away from Pasadena to come back to Washington and testify before McMahon's committee. I was sort of reluctant to do it on the ground that I hoped to stay put. But I came back. He kept me over for several days to give both public testimony and secret testimony.

While that was going on, I was brought into conferences in the State Department --

BY MR. GARRISON:

Q That committee of McMahon's was for what purpose?

A The Special Senate Committee he was trying to study the atom and draft legislation which was better than the May-Johnson bill, the committee that led to the McMahon Act under which we are operating even today. I was called into the State Department in the preliminary discussions of what the mission that was going over to Moscow might talk to the Russians about. The United States, England and Canada had issued a very resounding declaration about the need for international control of atomic energy consistent with safeguards, and the question was what do we do next.

We discussed this at some length. I got the impression that we didn't have a very well thought through notion of what international control was, or what we would say to the Russians, and I think it ended by our simply asking

them to subscribe to the three power declaration.

It is, I think, partly because of that that my interest in and to some extent my knowledge about the problem became known to people in the Department, and the result was that I was called back shortly after the opening of the next year for very serious work on the problem of international control.

I ought to mention one thing that occurred in Pasaden at that time. General Groves had this immense mass of technical information developed during the war. All of it was secret. Some was about lubricants, some about valves and some about bombs. He wanted to get started on the job of sorting it out. What should be made public, what should by all means not be made public, and what should be worried about.

He appointed Dr. Richard Tolman in Pasadena as the chairman of the committee, and I was a member of it. You have a list of the other members. I think Lawrence and Urey were on it, to begin this process of sorting it out. We divided things into three classes; those which were manifestly useful for science and the arts, and seemed to have no security value of any kind; those that were obviously connected with the military aspects of atomic energy and which should not be declassified unless there were international safeguards, an intermediate class of tough problems where we thought it would be dependent on the political assessment

of the state of the enemy -- it was not enemy in those days -- of Soviet efforts and the prospects of conflict in a short time.

Our general philosophy was that if we are going to have a long, long period when we are not going to use these things and don't need these things, the more that is open, the better American technology and science will prosper. If the time is kind of short, then the advantages of our secretly developed information will be considerable.

DR. GRAY: You say Dr. Tolman was chairman of this committee?

THE WITNESS: That is right.

DR. GRAY: What was this committee called?

THE WITNESS: I have it down as Declassification Committee, but I am not sure. May I at this point interpolate that the biographical material that you were given late this morning was compiled by a very intelligent secretary. I did check with her on one or two things I remember. The records are good only since we came to the Institute. I wouldn't have you think that they are admirable records of the years during the war, because there just are no such things. It is the best we could do for your convenience.

BY MR. GARRISON:

Q Then this takes us into the beginning of the plans for international control of atomic energy.

A Yes.

Q And the preliminary discussions within the government about that?

A I have talked about some of the preliminary discussions. I believe the background for the Lilienthal panel was the following. The Russians didn't want to talk about the atom at Moscow, but they did agree to this three power declaration, and they threw the thing into the United Nations. There there was another resounding declaration and two Senators, Vandenberg and Connally, were disturbed that this might leak secrets, that we might not be adequately protected.

The Secretary of State said no, there will be safeguards. When he got home he set up a committee under the chairmanship of Mr. Acheson, with General Groves, Dr. Bush, Dr. Conant and Jack McCloy on it, and they were supposed to devise the safeguards. They started thinking about the safeguards and in Mr. Acheson's words, they soon found they were trying to devise a cowcatcher without ever having seen a locomotive, because nobody knew what was meant by international control. What sort of things would be, who would do what and what would the rules be. They appointed a panel of which Lilienthal was chairman, the membership you have in full there, Mr. Barnard was on it, and Mr. ~~Weiney~~ WINNE was on it, and we were supposed to make a sketch of international control

which would be useful in coping with the atom and which would, if possible, be a step in carrying out that avowed intent of our action, namely, so to alter the relations between nations that war itself would be a lot less likely.

This was a pretty ambitious thing with all that in mind. It did not work, but people were talking that way in those days, and I must say that I was one of those who talked that way very freely.

Q Did you about this time prepare a memorandum to Mr. Lilienthal containing your ideas?

A The way it worked is that we met and in the first few weeks, a week or two, my job was that of teacher. I would get back at the blackboard and say you can make energy this way in a periodic table, and that way and that way. This is the way bombs are made and reactors are made. I gave in other words a course. I gave parts of this course also to Mr. Acheson and Mr. McCloy at night informally. When we listened to parts of it that I didn't know anything about, where the raw materials were, and what kind of headache that was. Then everybody was kind of depressed the way people are about the atom, and we decided to take a recess.

Mr. Lilienthal asked everybody to write him a note if they had any ideas as to what might work and asked me in particular to write a primer on the subject so that people could have the facts at their disposal. I stayed in Washington

and did both of these. I think the note is the thing to which you refer.

Q Yes. I show you this document entitled, "Memorandum of February 2, 1946". It should be entitled, "Extract from memorandum of February 2, 1946, from Dr. J. Robert Oppenheimer to David E. Lilienthal, Chairman of the Board of Consultants to the Secretary of State's Committee on Atomic Energy." This extract has been copied, has it not, from a carbon copy in your files from a memorandum which you gave to Mr. Lilienthal at the time?

A So you tell me. There is no reason why the whole memorandum should not be available, but it is rather long.

MR. GARRISON: I might say to the Board that we will from time to time as we go along be offering you extracts from writings and articles and addresses of Dr. Oppenheimer. The full text of each ofthose will be available to the Board, and the only reason for taking excerpts from them is to save time, and because they have a certain relevance to Dr. Oppenheimer's views at the time with respect to our foreig relations. This is an example of what we shall be doing. I would just like to read this, because it is quite a significant document.

"It is probable that the main desire of our Government is the achievement of safety and protection against the threat of atomic warfare. Even if it were possible to achieve

this without considering such positive features as the extension of knowledge and its application to constructive purposes, it might be argued that such a sourse should not be followed. It is my belief that quite apart from its desirability, the provision for constructive development of the field of atomic energy will turn out to be essential for the operation of any system of safeguards. . . In particular, it has become clear to us that not only politically, but scientifically and technically as well, the field of atomic energy has witnessed very rapid change and very rapid progress. I believe that this will be the case in the future, too, and that no organization and no proposal can be effective which does not have a flexibility adequate to these changes. I further believe that any proposed organization must itself reflect the changing character of the problem and the constructive purposes which are a complement to control. . .

"Almost everyone has, at one stage or another in his acquaintance with this problem, considered prohibiting further work on atomic energy, and devising a system of inspection adequate to insure that this prohibition is carried out. It is not only that this proposal would make impossible the application of existing knowledge to constructive ends; it would be so contrary to the human patterns of exploration and exploitation that no agreement entered into by heads of state could command the interest or the oooperation of the

people of the world. An apparently less radical solution would be the separation of the functions and development and of control according to which the only responsibility of an international authority would be the inspection of work carried out under a purely national or private initiative, and the possible prohibition of some of this work. The negative approach to the problem of control would leave the inspecting agency with inadequate insight, both into the technical state of the subject, and into its motivation and the organic characteristics of its growth. . . .

"Against this background of the difficulties of control as an isolated and negative function, I have thought it essential at least to consider combining the functions of development and of control in a single agency. It is fairly certain that there are now, and will increasingly be, activities having to do with atomic energy which are not vital to control and which, for human, or organizational, or politica reasons should not be included among the functions of the controlling authority; but there are certainly several such functions which, as matters now appear, should be so included among them: the development of raw materials, the exploration of atomic weapons, and the application, in its more dangerous forms, of atomic energy to power and technology. . ."

MR. ROBB: Do you have the original of that, Mr. Gamrison, so that we can see the end of these sentences?

THE WITNESS: We have only my own carbon of it, but we have it complete.

MR. ROBB: That is what I mean.

THE WITNESS: I am not ashamed of any aspect of the memorandum.

MR. ROBB: I was not suggesting that you are, Doctor.

THE WITNESS: I didn't want to burden you with it.

DR. GRAY: May I ask a question there. Is your request there for the purposes of making the entire memorandum part of the record?

MR. ROBB: Oh, no.

MR. GARRISON: Quite probably we should have had it ready, and we will have it ready in a moment.

THE WITNESS: Shall we save time by going on and we will have it as soon as it is available.

MR. ROBB: Yes.

BY MR. GARRISON:

Q Would you care to make any comment between the relationship of the ideas you expressed in this memorandum and the central philosophy of the Acheson-Lilienthal Report as it finally emerged?

A The comment seems to come inappropriately from me. I think they are identical. I think this is the heart of United States policy. I will say more. I think that any attempt at that time to establish control along these lines

would, if accepted by the Soviets, have so altered their whole system and so altered their whole relations with the western world that the threat which has been building up year after year since could not have existed. I think that no one at that time could with much confidence believe that they would accept these proposals. I think it was important to put them forward, and it was also important not to express too much doubt that they might be accepted.

In the UN we hammered away at this line, but there are some intervening complications.

Q The central idea of this scheme, I take it, was that there should be not merely inspection of atomic energy production and atomic energy armaments, but actual ownership and control of that whole process by an international agency, so that purely national development of these atomic energy programs would be ruled out, and that would have entailed in Russia as in other countries the actual ownership of productive facilities in that land, as in others, by an international agency, is that correctly stated?

A That is correctly stated. I think it is part of the story. It would have meant that the Russian Government gave up control over things going on involving their citizens on their territory. It would have permitted free intercourse between Russian nationals and people of the rest of the world. It would have meant that there could be no iron curtain. How

radical it was I may indicate by a comment that came much later. General Ridgway was on the Military Staff Committee at the UN at the time when I was on Mr. Brooks' staff, and our people had looked at this proposal and said if it were to go through, they would recommend that all secret military establishments be abolished. This was quite a slug.

Q Then work went forward on the report?

A We worked very hard on it. I think I should say this, I have been on many committees. The last thing I want to persuade you is that I was the big cheese on these committees. I did have this idea. It does derive from me. But in other ways, the other members of the committee had similar ideas.

For instance, Dr. ~~Wine~~ WINNE and Dr. Thomas said when they heard about the raw material situation, we ought to get rid of the scramble for uranium. If we don't work together on this we will never catch up with the control problem. So each relying on his experience came to somewhat similar conclusions.

I think the implication that I am responsible and alone responsible for the report is wrong. I am responsible for writing a great deal of it; not all of it, but perhaps a half of it. It was, I think, persuasive document which both here and abroad spoke well of the generosity and prudence and sense of America.

MR. GARRISON: I have here, Mr. Chairman, a copy of the Acheson-Lilienthal Report, entitled "Report on the International Control of Atomic Energy" in case any members of the Board would like to look at it now or later. I would like at this time to just read into the record three very short extracts from it.

DR. GRAY: What is the date of that report?

MR. GARRISON: March 16, 1946. It was prepared for the Secretary of State's Committee on Atomic Energy by a board of consultants, Chester I. Barnard, Dr. J. R. Oppenheimer, Dr. Charles A. Thomas, Harry A. ~~Werney~~ WINNE, David Lilienthal, Chairman. I can put the page references into these excerpts.

DR. GRAY: I don't think that is necessary.

MR. GARRISON: "International control implies an acceptance from the outset of the fact that our monopoly cannot last." (p. 53).

"It is essential that a workable system of safeguards remove from individual nations or their citizens the legal right to engage in certain well defined activities in respect to atomic energy which we believe will be generally agreed to be intrinsically dangerous because they are or could be made steps in the production of atomic bombs." (P. 22)

"It therefore becomes absolutely essential that any international agency seeking to safeguard the security of the world against warlike uses of atomic energy should be in the

very forefront of technical competence in this field. If the international agency is simply a police activity for only negative and repressive functions, inevitably and within a very short period of time the enforcement agency will not know enough to be able to recognize new elements of danger, new possibilities of evasion, or the beginning of a course of development having dangerous and warlike ends in view...." (p. 23.)

I think those three paragraphs are significant of the central thought of the report. I am sure if the Board will at its leisure re-read again the memorandum to Mr. Lilienthal that Dr. Oppenheimer wrote on February 2, 1946, you will see that the same thought appears in that memorandum as appears in the final report.

DR. GRAY: For the purposes of the record, these are not paragraphs which appear consecutively in this document. I don't know. I am asking for information. Are they separated? Is my question clear?

MR. GARRISON: Yes, it is, indeed.

MR. ECKER: I believe they do not appear consecutively where the quotes are closed.

MR. GARRISON: Suppose we at the end of the hour put the page references in. They should be in.

DR. GRAY: I think that is satisfactory.

BY MR. GARRISON:

Q Do you want to go now to your testimony before the McMahon Committee?

A I will go quickly. When the report was done, we had several conferences with Acheson's committee. In fact, the last and rather delicate chapter of the report which I largely wrote we did not originally have in. But the committee thought that some description of how you might get from where we were then to where we thought we would like to be was called for. This had the disadvantage that it tended to disclose some aspects of our negotiating position and made the publication of the report perhaps less wise than it would otherwise have been.

I went home and I was very soon called back for two reasons. The report was out and the newspapers greatly distorted and exaggerated the virtues of denaturing. We had said you could fix up fissionable material so it was not immediately useable in bombs. This was the headline. Probably when we wrote it we invited that distortion. In any case it occurred.

I came back partly to attend the meeting to get an agreed statement out of a lot of technical people as to what the truth was and partly to testify before McMahon's Committee. I remember Senator Vandenberg saying "I like this. This is the kind of test we should be putting to the Russians." I think it was largely in that spirit that we went on with it.

Baruch had been appointed to represent the United States in these negotiations and this was announced, I think, just about the time the report was done. I went back to California again, but before long I came back to talk with Mr. Baruch and Hancock and Eberstadt and tell them a little bit about how we had gone about it.

I then gave some lectures at Cornell on a rather broad subject, but one of the lectures was about the international control of atomic energy. It was reprinted rather widely, and was an advocacy of the position that we had adopted. I gave another talk the next day in Pittsburgh which was another job of advocacy of this set of proposals. It was reprinted in the New York Times. Mr. Baruch told me that I had schoped his speech that he was going to make at the opening of the UN. That was not true. But it did have in it one element which was missing from the Lilienthal report and that was the remark that this business we were talking about was incompatible with a veto. You could not run a job like this and have Yugoslavia or Crete decide that they didn't like what was going on and stop it. This was the veto on operations; it was not the veto on sanctions, because nothing we discussed had to do with sanctions. That was the second of Mr. Baruch's points.

We met in Blair Lee House the next day and had a long discussion with Mr. Baruch and his staff. He asked me

what we had done wrong in the report. I remember mentioning a few points, among them the failure to make clear the relation of what we proposed to the veto, and the invitation that we gave to the press and the public to exaggerate the value of the denaturing.

Very shortly thereafter I agreed to serve as one of the consultants to Mr. Baruch in preparation for and in the conduct of the UN negotiations. The senior consultant was Dr. Richard Tolman, whom I mentioned before. I think Dr. Robert Bacher and I were the most active next to Dr. Tolman, but Compton and Thomas and one or two other people were also involved.

We spent through the summer with him and with his staff, and tried to help. The main job we did was to get an agreed paper out of the International Commission that international control was technically feasible. This was something you could do. The Russian delegate, I think it was Gromyko, balks at signing this, but finally the Russians agreed that international control was technically feasible. I think it is the last time we have agreed with them on anything in the UN, and certainly anything having to do with the atom.

Q They agreed that it was technically feasible, but the report did not say it was politically feasible.

A They attacked the proposal. They attacked both the aspects which were prominent in the Acheson and Lilienthal

thing, and that which Mr. Baruch added having to do with sanctions. I think they mostly attacked the main point, that this would have been a terrible invasion of their privacy, and they were not going to have it. This attack continued for years.

DR. GRAY: May I interrupt you there, Dr. Oppenheimer I want to know whether you want a break. You have been talking rather constantly.

MR. GARRISON: I think he will be getting a break because I will be reading a few documents into the record, but I think the Board would like a break.

DR. GRAY: I would like to see the point at which we will stop the hearing this afternoon.

(Discussion off the record.)

DR. GRAY: Suppose we take a recess for five minutes.

(Brief recess.)

L DR. GRAY: I think we might as well proceed, Dr. Oppenheimer.

THE WITNESS: After the summer of work with Mr. Baruch, it became difficult even for a dedicated optimist to think that anything would come of the negotiations in the sense of a real agreement. It was hard to believe that before it started, and the nature of the Soviet conduct, not only the kind of objections they made, but the nature of their dealings was extremely revealing to anyone who saw it for the first time.

In fact, it is worth recollecting that the Acheson-Lilienthal Board was working in early 1946 at precisely the time when Stalin made the speech about their encirclement and their need to keep their gaurd up and to re-arm.

I revert to the fact that it was healthy for us to attempt this, but thatit should not be read into that time that we were going around in a mood of high optimism. I have seldom been as gloomy in my life; that even includes today.

Nevertheless there was a job to do and I continued to do it. The job was establishing to our friends in the U.N., to the governments and fo far as possible to the officials and the people of our friendly nations, that what we had put up made sense and was not a bluff and was not propoganda and that it had merit.

I don't know how important that job was but I

2 stayed with the Baruch enterprise until he resigned, and then I was asked to serve as advisor to General Osborn, who took over in the spring of 1947. Osborn asked me to come up and spend some time with him talking it over. On the way I stopped at the State Department and Mr. Acheson showed me the President's speech on the Truman Doctrine. He wanted me to be quite clear that we were entering an adversary relationship with the Soviet, and whatever we did in the atomic talk we should bear that in mind.

I worked with Mr. Osborn intensively at first. I testified before the U. N. AEC, or one of its committees, on how you would go about on the international cooperative beneficial uses of atomic energy.

I continued to consult Mr. Osborn in company with Dr. Conant and General Farrell and General Groves, and maybe General Nichols, as long as the problem of atomic control was still a matter of debate in the United Nations until it was engulfed in the wider but also hopeless job of disarmament.

I would like at this time to say only two things. One is that the negative view of the possibility of any agreed solution with the Russians which came on us all then, as it has not gotten any different but gotten deeper, and I would like to refer to that again in connection with the work we did in 1952 for the State Department on the regulation of armaments, where the context was somewhat different.

3 The second is to say that incidental good did come of this effort. I think that in so far as people paid attention to it, the United States' proposals were recognized as indeed sensible, and we got lots of credit for them.

I ran into therepresentatives of the French and English, and some other countries, too -- however, primarily the French and English -- and though always keeping my own government informed as usual, I was, I think, able to do some useful jobs on the side. I talked to the French officials as well as the French scientists about the desirability of their building up a real scientific life in France, and about the undesirability of their getting into any rivalry with us on the atomic business.

I said I thought we would be able to help and have more fellowships and laboratories, and we would get into lots of trouble if they were getting into sensitive areas from the point of view of security. Ithink I always reported and checked with the officials of AEC or the State Department when any such conversations occurred.

With the United Kingdom it was quite a different thing. There we had had an intimate partnership, as you read in the newspapers and know anyway, in the last few years and during the war. There were some excluded areas, but all the things I was concerned with the British knew about and contributed to.

4 I visited Europe in the summer of 1948. In the winterof 1949 we undertook to see what could be done to restore this partnership. You will hear testimony about this from other people. The problem kept arising because of raw materials allocation, because of the dissatisfaction of the British, and because of the double problem that it was nonsense to have their best people duplicating what we were doing, and that there was thought to be and perhaps was a security problem in working with them.

We had a meeting in Princeton for two or three days that I think was chaired by Mr. William Webster. The Commission was represented by the General Manager and General Counsel. The Military Establishment was represented by General Nichols and General Norstad, the State Department by Mr. Kennan and Mr. Butler, and the interest at laiety by Dr. Conant and myself.

This was the beginning of an attempt which was abortive but which got quite far along to re-uniting the relations between United States, England and Canada in the atomic energy business. It was abortive -- I had better not say why because I was not in the politics of its abortion. But I have always regretted that failureand I am not sorry for the efforts I made.

Mr. Morgan. When was that?

THE WITNESS: The meeting was in 1949. I read

5 when I was out West in 1949 of the evening when the President called in the senators to Blair House when he was leaving, and when they came out of the door the reporters talked to them and were told that the senators heard something so dreadful that they could not speak about it. What they heard was about the war time collaboration and that the Brit ish knew a lot about atomic bombs and could probably make them if they tried, and that they were on the point of trying on their own. This is hearsay testimony, or testimony as to what I read in the papers.

As I say, our relations with the scientists of other countries and some effort to improve what we have learned to call the basis, the cordiality and strenght of our alliances, these things did come out of these U. N. meetings. But it was pretty thin fruits compared to the vision of world government and permanent peace which some people had at the time.

I think now there is stuff to read.

By Mr. Garrison:

Q. Dr. Oppenheimer, I have here a document, called "Atomic Energy as a contempary problem" by Dr. J. Robert Oppenheimer, presented at the National War College in Washington, September 17, 1947. This is a stenographic transcript of the remarks made by you on that occasion.

This came from your files, I take it?

6 A That is right.

MR. GARRISON: I would be glad to hand it to counsel as I read an excerpt from it

THE WITNESS: This may not be published without the permission of the War College. It has no restricted data, but it cannot be published without the permission of the War College.

(Discussion off the record.)

DR. GRAY: Will you proceed.

MR. GARRISON: These excerpts are from pages 6 to 8 of that transcript.

MR. ROBB: I have it.

MR. GARRISON: "At the same time, I think no one can take with any seriousness the hope or expectation that the Soviet Union will accede -- or that it will come closer to acceding to what is now the majority plan."

Thatis the United States plan.

"That is not too hard to understand. The cornerstone of our proposal is an institution which requires candid ness and great openness in regard to technical realities and policy. It involves the working cooperation between peoples, irrespective of nationality. It involves a maximum effort to abolish national rivalries in the field of atomic energy, and in all dangerous areas of atomic energy it involves a total and genuine international action. It is clear

7 that, even for the United States, proposals of this kind involve a very real renunciation ..."

MR. ROBB: Wasn't there an omission at that point?

MR. GARRISON: There are three dots which I have indicated here, and if there is anything significan in the omission --

MR. ROBB: No; I have not said there is.

MR. GARRISON: I have indicated the omissions by dots.

MR. ROBB: I think for the record it should be indicated.

MR. GARRISON: Yes; the reporter will so indicate.

"But if for the United States and the Western European powers some sacrifices are required by these proposals, the sacrifices, the renunciation, required of Russia are of another order of magnitude; and that is because the proposed pattern of control stands in a very gross conflict to the present patterns of state power in Russia, and because the ideological underpinning of that power, namely the belief in theinevitability of conflict between Russia and the capitalist world, or the allegedly capitalist world, this underpinning, which is most difficult I suppose for a government to renounce, would be repudiated by a cooperation as intense or as intimate as is required by our proposals for the control of atomic energy. Thus what we have asked of the Russians is

8 a very far reaching renunciation and reversal of the basis of their state power, and of their state power itself. It does not seem to me likely that we have found inducements, or cajolery, or threats which together are adequate to make them take this great plunge. That does not mean, I suppose, that this will never happen, but it will almost certainly not happen as a result of the discussions in the United Nations.

"The whole notion of international control presupposes a certain confidence, a confidence which may not be inconsistent with carrying a gun when you sit down to play poker, but at least is consistent with siting down to play poker. In the year and a half since the effort on these problems started we have found ourselves forced by the Soviet moves, and by the changing political situation throughout the world, over and over again to take steps which were in essence a repudiation of that confidence; and the Soviet has taken ever more grave steps in repudiation of that confidence... I therefore think that to believe seriously today (1947)that in six months, a year, or a year and a half, we will have something resemblin an ADA (Atomic Development Authority), the cooperative development of atomic energy, involves a kind of schizophrenia which can only lead to very bad political confusion. I even think the worry that one often hears discussed in unofficial, and sometimes official, circles --

9 'What would happen if the Russians suddenly reversed their stand, embraced our proposals, and started to work to put them in effect?' -- that is an empty worry because it is in the nature of the proposals we have made -- a portection afforded by our plans for the United States -- that they cannot be implemented in very bad faith, that they presuppose a very large measure of peaceful intention, of cooperation, of confidence and candor before they can get started. I am therefore not very much alarmed that Mr.Gromyko will some day say to Mr. Osborn, 'We finally have understood your proposals and we think they are wonderful. We accept them in full.' I do not think this will happen."

The next excerpt is from an article in Foreign Affairs for January, 1948, entitled "International Control of Atomic Energy", by J. Robert Oppenheimer. These are pages 12, 13 and 14 in that article.

Mr. Robb, do you have page 12 there?

MR. ROBB: Yes.

MR. GARRISON: This, you will see, is several months after the War College speech which we have just been through.

"Two aspects of this development need to be specially mentioned. One has to do with what may be called the aim of the United States policy -- the sketch of our picture of the world as we would like to see it in so far as atomic

10 energy was concerned. Here, the principles of internationalization, openness, candor and the complete absence of secrecy, and the emphasis on cooperative, constructive development, the absence of international rivalry, the absence of legal right for national governments to intervene -- these are the pillars on which our policy was built... The second aspect of our policy which needs to be mentioned is that while these proposals were being developed, and their soundness explored and understood, the very bases for international cooperation between the United States and the Soviet Union were being eradicated by a revelation of their deep conflicts of interest, the deep and apparently mutual repugnance of their ways of life, and the apparent conviction on the part of the Soviet Union of the inevitability of conflict -- and not in ideas alone, but in force. For these reasons, the United States has coupled its far reaching proposals for the future of atomic energy with rather guarded reference to the safeguards required, lest in our transition to the happy state of international control we find ourselves at a marked relative disadvantage.. Natural and inevitable as these desires are, they nevertheless stand in bleak contradiction to our central proposals for the renunciation of sovereignty, secrecy and rivalry in the field of atomic energy. Here again, it is no doubt idle to ask how this country would have responded had the Soviet Union approached the problem

11 of atomic energy control in a true spirit of cooperation. Such a situation presupposes those profound changes in all of Soviet policy, which in their reactions upon us would have altered the nature of our political purposes, and opened new avenues for establishing international control....

"Questions will naturally arise as to whether limited but nevertheless worthy objectives cannot be achieved in this field. Thus, there is the question of whether agreements to outlaw atomic weapons more like the conventional agreements, supplemented by a more modest apparatus for instpection,may not give us some degree of security. Possibly when the lines of political hostility were not as sharply drawn as they are now between the Soviet Union and the United States, we might have tried to find an affirmative answer to this question. Were we not dealing with a rival whose normal practices, even in matters having nothing to do with atomic energy, involve secrecy and police control which is the very opposite of the openness that we have advocated -- and under suitable assurances offered to adopt -- we might belive that less radical steps of internationalization could be adequate... My own view is that only a profound change in the whole orientation of Soviet policy, and a corresponding reorientation of our own, even in matters far from atomic energy, would give substance to the initial high hopes."

By Mr. Garrison:

Q Dr. Oppenheimer, here is a letter to you from Mr.

12 Chester Wood, the Secretary of theNew York State Bar Association, enclsoing a transcript of the remarks that you addressed to a meeting of the Judicial Section of that Association, this being February, 1948 -- the precise day is not clear. This wan taken from your files, was it not?

A It was certainly taken from my files. That is all I can say.

Q Then you identify the document, I assume, do you not?

A If I am to make a serious identification, I should see it.

Q Yes. (handing)

MR. GARRISON: The excerpts which I have taken from that are at pages 7 to 10, inclusive.

THE WITNESS: I do identify it.

MR. GARRISON: Now I would like to read from this address:

"The proposals which the United States made and which are manifestly not going to be accepted were perhaps somewhat more radical even than the people of this country believed, perhaps even than some of the officers of this Government believed. The idea was, not that one would fasten a scheme of control onto an otherwise unaltered pattern of the relations between sovereign states. The relation was rather that here appeared to be an opportunity, very pressing

13 in its urgency and very rich in its technical patterns, for getting started, for making a very profound alteration in the relations between states, and one which might conceivably be sufficiently attractive to the Government of the Soviet Union to cause them to reverse what has been their long-standing plicy of extreme secrecy, considerable terror and very great latent hostility to the non-Soviet world.

"The changes that wereimplied or that would have been implied by the acceptance of our proposals, by the elaboration and implementation of our proposals, would have altered the face of the world. They would have done so in ways that no one is wise enough to predict, but that surely would have led to a much greater openness, to a much greater candidness, to much more working cooperation between the peoples of various nations.... When you think, for instance, that so obvious a notion as the economic cooperation of the countries of Western Europe is still very far from a reality, you begin to realize that the formal agreement of the delegates was only the beginning of the problem. But one point overshadows this, and that is, however great the enunciation of what is for us a powerful action, however great the enunciation might appear to the British, who are concerned, as rapidly as possible to reach the exploitation of atomic energy as a form of power, the sacrifices which the acceptance of these proposals would have meant to the Government of the

14 Soviet Union went very much further than that, because it implied a repudiation of the philosophy by which that Government has come into being, has been living...."

DR. GRAY: Do you suppose that word "enunciation" was improperly transcribed from your remarks?

THE WITNESS: Yes. It was certainly "renunciation".

MR. ECKER: It is a verbatim copy of the stenographer's transcript.

MR. GARRISON: I am sure you are right, Mr. Chairman.

By Mr. Garrison:

Q Dr. Oppenheimer, I show you a manuscript entitled, "Address by J. Robert Oppenheimer before the Rochester Institute of International Affairs, December 11, 1948", at Rochester, New York, devoted to the prospects for world peace, and ask you if counsel selected that from your files?

A He did.

Q Will you hand it to counsel.

A Yes. (handing)

MR. GARRISON: Mr. Chairman, I have a very short extract from that at page 3:

"Certainly there was little to inspire, and nothing to justify, a troubled conscience in the proposals that our government made to the United Nations, as to the form which the international control of atomic energy should take. These proposals, and some detailed means for implementing

15 them, were explored and criticized, elaborated, and recommended for adoption by fourteen of the seventeen member nations who served on the United Nations Atomic Energy Commission. They were rejected as wholly unacceptable, even as a basis for further discussion, by the three Soviet States, whose contributions to policy and to debate have throughout constituted for us a debasingly low standard of comparison."

MR. GARRISON: I have here a reprint from the record of the Association of the Bar of the City of New York, Volume 6, No. 3, for March, 1951, containing an address by Dr. Oppenheimer, entitled "Contempary problems of Atomic Energy".

The excerpts which I am about to read to the Board appear at page 109 of this reprint from the record.

"Our proposals for the International Control of Atomic Energy, which were largely based on the technical realities of the field, were presented on our behalf to the United Nations by Mr. Baruch, and were widely accepted by the non-Communist nations. The implementation of these proposals would have required a profound alteration in some, at least. of those features of the Soviet system which are responsible for the great troubles we are in today. The failure to persuade the Soviet Government to alter its practices was anticipated by many. Yet we should not forget that this is an objective not only of the past but of the future as well.

16 "Let me mention one or two points. One, to my mind the principal one, was that it was clear that no secure system could be developed for protecting people against the abuse of atomic weapons, unless the world were open to access, unless it was possible to find out the relevant facts everywhere in the world which had to do with the security of the rest of the world. This notion of openness, of an open world, is, of course, relevant to other aspects of the Soviet system. It is doubtful whether, without the newly terrible, yet archaic, apparatus of the Iron Curtain, a government like theSoviet Government could exist. It is doubtful whether the abuses of that government could persist."

MR. GARRISON: I have just one more short excerpt to read. This is from another article in Foreign Affairs of which we have a copy here for July, 1953. This is quite recent. The excerpts are from pages 525 to 526 of that article.

"Earlier, shortly after the war's end, the Government of the United States had put forward some modest suggestions, responsive to these views, for dealing with the atom in a friendly, open, cooperative way. We need not argue as to whether these proposals were stillborn. They have been very dead a long, long time, to the surprise of only a few. Openness, friendliness and copperation did not seem to be what the Soviet Government most prized on this earth.

17 "It should not be beyond human ingenuity for us to devise less friendly proposals. We need not here detail the many reason why they have not been put forward, why it has appeared irrelevant and grotesque to do so. These reasons range from the special difficulties of all negotiation with the Soviet Union, through the peculiar obstacles presented by the programmatic hostility and the institutionalized secretiveness of Communist countries, to what may be regarded as the more normal and familiar difficulties of devising instruments for the regulation of armaments in a world without prospect of political settlement.

"Instead we came to grips, or began to come to grips, with the massive evidences of Soviet hostility and the growing evidences of Soviet power, and with the many almost inevitable, yet often tragic, elements of weakness, disharmony and disunity in what we have leared to call the Free World."

THE WITNESS: I think we are through with this. I will leave it to counsel to say what it means, but I think that in every case I tried to explain that we could not take this path to people who insisted on thinking that we might, and yet not to talk publicly of the fact that we were giving up a position until the Government of the United States had in fact given it up.

There was a bit of discrepancy between our offical

18 position and reality and the opinion, let us say. of my colleagues in science. I tried to explain to them that the jig was up, because that was relevant to getting back to work. At the same time I could not come out and say, "This is a hopless thing," because I had some official connection with the Government until the Government had itself said so. I think these dates will bear that out more or less.

Now we are through with this phase and entering on a new one. In late 1946, I was appointed by the President as a member of the General Advisory Committee to the Atomic Energy Commission. That is a long big job and I will talk about it. Shortly thereafter I was given a concurrent appointment which I held perhaps even a little longer. That was as a member of the Committee on Atomic Energy of the Joint Research andDevelopment Board in the military establishment. This later became the Research and Development Board and the chairmen varied. The initial arrangements were made by Dr. Bush who was head of this outfit.

Dr. Bush appointed Conant as chairman, the members of the statutory military liaison committee as members, and as civilian members me and Crawford Greenewalt. There was some overlapping of membership between the Advisory Committee and thiscommittee, and total overlapping of membership between the military liaison committee and this committee.

What we did on this committee I don't propose to go

19 into in such detail, and I will try to finish with that this afternoon.

The initial job was to try to give direct technical information to the military on the military liaison committee. General Groves knew quite a lot about the atom and so did Admiral Parsons. The other members of the committee in those days were not very fresh to it. There was at that time not very much machinery for gathering information.

I think, as Dr. Bush explained it, it seemed like a good idea if the same technical considerations which were being made available to the Commission were being made available directly to the military. It was a liaison function. We had very little, if any, power, but we had the ability to talk about common problems.

The importance of this function declined very much because the military developed admirable ways of getting their own intelligence and their own knowledge and became as expert as anyone. But it did provide a continuing channel of discussion. Every once in a while we would stir something up in this committee which was usefull.

I have in mind two examples. One, toward the end of 1950 and the beginning of 1951, on the operational readiness for tactical use of atomic weapons. I won't spell out the details but the question of getting from the hardware which the Commission provided and the hardware which the military

20 services had to the point where you could really make effective use right away.

This was the time, I may remind you, when the feeling that war might break out, however erroneous -- widespread war -- was very, very general, and there was a war going on in Korea and it was not going too well.

Another example -- our role was certainly not major in it -- comes to mind, and that was trying to be sure that there was an interim capability in the field of hydrogen weapons, of thermonuclear weapons, at a time after the 1952 tests and before the current series of tests when it seemed important that having announced this thing throughout the world, we have something to back it ~~it.~~ up.

There were two panels on this Board of which I acted as chairman. One was in the summer of 1948, and I think the members of it are listed on your paper, which was a general sorting operation. By then an enourmous number of potentially useful applications of atomic energy to military things came up, some of them crazy, some of them sensible, some of them immediate and some of them very remote.

We sat down, the three generals, the admiral and I, and called in other people whose help would be useful and wrote our best opinion as to the relative time sales and absolute time scales of submarine propulsion and nuclear aircraft propulsion; how it was going with the deliverbility of

of tacticalweapons, what needed to be done here, what needed to be done there.

The description of the report, the contents of which I cannot tell you, is not going to be very interesting. I think it was a decent job.

The report that we wrote in late 1950 and early 1951 -- and I may remind you of who was on that committee. I was again the chairman.

Q You are reading from what?

A The third page of your notes. Bacher, Alvarez, Lawrence, Kelly, Parsons, Wilson, McCormack. There we took a somewhat deeper bite, because this was the time of the Chinese intervention and a time when as you may remember of daily alerts about the possibility of attack on the continental United States, a time of very great anxiety. We addressed ourselves to the question with what we have and can have soon, how rapidly we can get a really effective use of the atomic capability that we have developed. What can we do fast about this. You will hear testimony about this possibly from the other witnesses.

It is also a time at which technical prospects on the thermonuclear program were quite bleak. We so reported. I think it is interesting that there was no difference of opinion among us as to what we had solved.

This committee has continued until the Research and

Development Board was abolished. I think these are the few points that I wanted to cover.

Now we have the GAC appointment and I suppose there it would be best to start up fresh in the morning. There is something to read. It is something that I came upon in the files during the period of getting them straight. It is a letter I wrote to Admiral McMorris of the General Board of the Navy, and it represents the view of our military problem which, at that time, and I believe before and after, was the view that I took into the General Advisory Committee and kept through it. It is not a committee statement. It is not a report of the GAC. It is my own thoughts. It may give some background for what we started out to do and what we did do in the descriptions we gave on the General Advisory Committee.

Q These excerpts there come from this carbon from your files, is that correct?

A That is correct.

Q They begin on page 1.

DR. GRAY: What isthe date of this?

MR. GARRISON: April 14, 1948.

"Whatever our hopes for the future, we must surely be prepared, both in planning and in the development of weapons, and in so far as possible in our 'force in being', for more than one kind of conflict. That is, we must be prepared to

meet the enemy in certain crucial, strategic areas in which conflict is likely, and to defeat him in those areas. We must also be prepared, if need be, to engage in total war, to carry the war to the enmmy and attempt to destroy him. One reason why we must keep both of these objectives in mind (and they call for quite definite plans and quite different emphasis as to equipment, troops and weapons) is that it may not be in our hands to decide. With this reservation, it seems appropriate to suggest that there may be two phases to the problem.

"At the present time (1948), to the best of my knowledge, the Soviet Union is not in a position to effectively attack the United States itself. Opinions differ and evidence is scanty as to how long such a state of affairs may last. One important factor may be the time necessary for the Soviet Union to carry out the program of atomic energy to obtain a significant atomic armament. With all recognition of the need for eaution in such predictions, I tend to believe that for a long time to come the Soviet Union will not have achieved this objective, nor even the more minor, but also dangerous possibility of conducting radiological warfare."

THE WITNESS: This was a bad guess.

MR. GARRISON: "In so far as the United States need not for some time to come fear a serious and direct attack on this country, it would seem to me likely that our primary objective would be to prevent the success of Soviet arms and

Soviet policies, to carry out a policy of attrition, and not to engage in a total war aimed at destroying entirely the sources of Soviet power. There are many arguments for this and I have little to add to the obvious ones. Yet, the general political consideration that the consequences, even in victory, of a total war carried out against the Soviet Union would be inimical to the preservation of our way of life, is most persuasive to me.

"On the other hand, as time approaches, if it ever should, where as a result of political or military success in Europe or Asia, a s a result of advancing technological development and improved industrial output, the Soviet Union becomes a direct threat to the United States, we shall no longer have this option. We should no longer have this option if the maintenance of a strategic area such as Western Europe or Japan could not be achieved without a direct attack on the sources of Soviet power.

"From this it seems to me that two conclusions would seem to follow: (1) that we must be prepared, in planning, in logistics, and in development, for more than one kind of war; and (2) that the very greatest attention must be given to obtaining reliable information about the state of affairs within the Soviet Union bearing on its military potential.

"One final comment: There is to my mind little

doubt that were we today, with the kind of provocation which the Soviet Union almost daily affords, to attack the centers of Soviet population and industry with atomic weapons, we should be forfeiting the sympathy of many potential allies on whose cooperation the success of our arms and the fundamental creation of a stable peace may very well depend. These same people would no doubt be almost equally disturbed were we to renounce, irrespective of the development of Soviet power, recourse to such armament."

Are there any comments you would like to make on the views expressed there?

THE WITNESS: I need to say two things. First, that this was apparently an answer to some inquiry. I don't know what the inquiry was. Second, that I was completely wrong in thinking that we could be relaxed about the Soviet atomic threat. I think I was in very general company. I think we all very soon rectified these views as the evidence came in. But this was a year and a half before the first Soviet explosion and the time when my view was, I think, quite the same as the general intelligence view.

BY MR. GARRISON:

Q This opening paragraph, if I may go back to it for a moment, sounds to me rather like what Admiral Radford said the other day about the new look. "We must be prepared to meet the enemy in certain crucial, strategic areas in which

conflict is likely, and to defeat him in those areas. We must also be prepared, if need be, to engage in total war, to carry the war to the enemy and to attempt to destroy him." This has emphasis on flexibility, which I think is also apparent in that testimony by Admiral Radford.

DR. GRAY: May I ask, did you read the beginning of this letter?

THE WITNESS: No. I would like to have the beginning read, because the beginning states that I don't know anything about this subject.

MR. ROBB: It occurred to me, Mr. Chairman, that the beginning and the end should be read to give the entire picture.

THE WITNESS: I don't know what the beginning says.

MR. ROBB: You are quite right, it says you don't know anything.

THE WITNESS: Shall I do that:

"Thank you for your letter of March 31st. In this you enclose the agenda for the study of the General Board, Serial 315. You request specifically such comments as I can make on Items 110, 118 and 120.

"Though I am aware of the great importance which attaches to this study, and the need for serious thought and effort on the part of many if the study is to be successful, I nevertheless must protest my almost total lack of

qualification for speaking to the question which you have put. Such comments as I can make should be given no great weight; they rest on little experience and little knowledge.

"All three of the items referred to me have to do with the plans of the United States for waging war, and with the kind of war we should fight. Implicit in some questions and explicit in others, is the issue of weapons of mass destruction; should we use these, should we plan to use these, shouldwe postpone the use of these. Implicit in the question is also the issue of a limited versus a total conflict: should the objective be destruction of the enemy, or his defeat in a specific area. Let me attempt to give my views on these matters."

Then it goes into what Mr. Garrison read.

The end is: "In conclusion, let me again remind you that these are in the nature of personal views, and that I can attach little weight to them, If, in matters which fall more closely within my field of competence, I can be of use to you, I shall of course be glad to do so."

DR. GRAY: That is addressed to whom?

THE WITNESS: Admiral McMorris, head of the General Board of the Navy. I am in a complete fog as to what it was all about, except in so far as this answer --

DR. GRAY: Was this signed as Chairman of some panel?

THE WITNESS: No, this was an individual opinion.

DR. GRAY: Thank you.

MR. GARRISON: It is simply introduced at this time to show his general approach to the whole policy of armament of this country.

THE WITNESS: There is one small item before we get into the General Advisory Committee, andthat is the following: There was set up under the contract with all three services, Army, Navy and Air Forces, I think the operating contractor was the Army, a study at California Institute of Technology, Dr. DuBridge was in charge of it, under the name of Project Vista, and its function was generally speaking to talk about ground oombat and the support of ground combat. What that finally came down to was the study of the defense of Europe and what it came down to was the study of what you do to defend Europe at any time, as soon as possible, if necessary.

The men involved in this project worked very hard on it, and they kept asking me to come out and talk about the use of atomic weapons in this picture. I thought they knew as much as I did. Dr. Bacher was there, Dr. Lawrence was there, Dr. Christie was and Dr. DuBridge was there. But they finally prevailed upon me, and I went out in the autumn of 1951, and we worked together on this problem.

Dr. Lauretsen and Dr. DuBridge went over with Mr. Whitman from the Office of the Secretary of Defense to visit General Eisenhower, Gruenther, Norstadt and Hanley in Europe.

What we attempted to do was to be sure it was clear to them how varied and useful atomic weapons could be in ways that are probably now quite obvious to you and ways which were not completely obvious then. General Eisenhower made one or two suggestions about things that he thought it would be handy to have. The principal messages that we brought back to this country were a plea for more information as well as more hardware and to make atomic weapons available and for restriction of the limitations on discussions of military problems with Allied Commanders. These were the things that made it hard to get on with these. I don't want to go into the technical aspects of it, though the antiair use of atomic weapons, their use to put out enemy airfields, both those that are near enough for combat planes and the deep lines strategic ones is an obvious example. This was the complement to the panel report I spoke of earlier on getting the atom to work on the battlefield as well as in the heartland. I think this may be a place to stop.

DR. GRAY: Before we stop, I wonder if you can, Mr. Garrison, give an indication of the witnesses.

MR. GARRISON: Ithought we might discuss that informally off the record. I can bring this chart and show you about how it looks now.

DR. GRAY: We will go off the record for a moment.

(Discussion off the record.)

DR. GRAY: Are we prepared to say we will meet again tomorrow morning at 9:30?

MR. GARRISON: We will undertake to be prompt.

MR. ROBB: May I say, Mr. Chairman, as far as I am concerned, and Mr. Rolander, I cannot speak for the Board, if it will accelerate matters and assist counsel to get some witnesses here, I would be very happy to come here earlier in the morning. I do not want to make that proposition too firm.

DR. GRAY: Let the Chairman speak for himself only and not for the other members of the Board. If by meeting at 9 o'clock we could move along without inconvenience and so forth, I believe the Board would be willing to meet at that time.

MR. EVANS: You can say it for me, because time is important to me.

MR. MORGAN: Yes.

DR. GRAY: So would you bear that in mind, Mr. Garrison. Any telescoping we can do without inconvenience or harm we would be interested in doing.

(Thereupon at 5:13 p.m., a recess was taken until Tuesday, April 13, 1954, at 9:30 a.m.)