

Oral History Interview
with
WILLIAM T. GOLDEN

Securities analyst during the 1930s; officer in the United States Navy, 1941-46; Assistant to the Commissioner, Atomic Energy Commission (AEC), 1946-50; consultant to the AEC, 1950-58; Special consultant to the U.S. President to review organization of the Government's military-scientific activities incident to the Korean War, consultant to Director of the Budget, 1950-51; member of military procurement task force of the Commission on Organization of the Executive Branch of Government (Hoover Commission), 1954-55.

Washington, D.C.
June 24, 1989
by Niel M. Johnson, Harry S. Truman Library

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Summary Description:

Topics discussed by Mr. Golden include the policy-making role of scientists in Government; Atomic Energy Act of 1946; the Atomic Energy Commission (AEC); Military Liaison Committee of the AEC; Research and Development Board of the Defense Department; civilian control of atomic energy; Steelman report; efforts to establish the position of science adviser to the President; National Science Foundation; Scientific Research Advisory Board of the Office of Defense Mobilization; decision to develop the hydrogen bomb; the breeder reactor; military procurement task force of the second Hoover Commission; and the President's Committee on Defense Scientific Research.

Names mentioned include Fred Lawton, Charles Stauffacher, Elmer Staats, Lewis Strauss, William Webster, Vannevar Bush, James B. Conant, Mervin Kelly, J. Robert Oppenheimer, Leslie Groves, Edward Teller, Robert Bacher, John Steelman, Donald Dawson, Detlev Bronk, Lucius Clay, Oliver Buckley, Charles E. Wilson, George Merck, Sidney Souers, R. Gordon Arneson, McGeorge Bundy, William Waymack, Sumner Pike, and Alan Waterman.

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William T. Golden

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[1]

JOHNSON: Since we are short on time, Mr. Golden, I'm not going into some of the background that I usually get. That, I think, is in the printed sources. But I will ask what were perhaps the most important influences prior to your service with the government, influences on your values and your career.

GOLDEN: Well, I had always been interested in science and technology from earliest childhood. I became a ham radio operator and got my radio transmitting license when I was just 13 years old. That was the greatest sense of achievement I've ever had. You had to take a test; go down to the Custom House and take a test on code and also on some technical matters. They were mostly simple, but not for a thirteen year old. I got my station, my call letters 2AEN, and I was very proud and pleased. I was always interested in science;

[2]

and I was encouraged by my parents.

JOHNSON: But you went into something else, investment banking?

GOLDEN: Yes, and I decided to go into investment banking. I was also interested in other things. But in college at the University of Pennsylvania, I had to make my mind up and I thought of going to law school, medical school, teaching English, literature, a number of other fields including being a biologist or a physicist. All these fields interested me. I finally decided in the second half of my senior year, I'd go to the Harvard Business School. I won't go into the somewhat romantic ideas I had about what you could do on Wall Street, which I knew nothing about other than having read a few stories. But I had the idea that if you go there and if you have the right turn of mind, you can get into and out of businesses, if your judgment is right. My father had been a small businessman, in the woolen business, and he was necessarily very strongly influenced by seasonal things beyond his control -- the weather, the climate, the general business situation, things like that. I saw that, and I thought well, in Wall Street if you have the

judgment and foresight you can get out of one business and into another business, and you can do it with any amount of money. I had \$400

[3]

saved up from my college allowances, and that was going to be my capital. And it was.

So I was accepted by the Harvard Business School and after one year there, 1930-31, I was fortunate to get a job with in Wall Street in the depths of the Depression. And I was able to make some money. By having some money, you can have the freedom of deciding what you want to do, if you want to do other things. Briefly, that's it. I was very fortunate that worked out that way. I got a good job with honorable and very able people, as a securities analyst. After ten years there I went into the Navy in the summer of 1941 as a civilian expert under contract until I was commissioned as a lieutenant after a waiver was granted for my nearsightedness (2/20 and 4/20) but fully corrected by glasses. The waiver was granted by request of the Chief of the Bureau or Ordnance after several attempts by me to improve my test score by exercises.

I stayed throughout the war as a Lieutenant Commander until the beginning of '46. I was in the Navy Department a large part of those four and a half years, including an aircraft at time, although I was also out at sea, in both oceans, on a wide variety of ships including an aircraft carrier and a submarine. I had an invention which the Navy later used (and after the war patented for me) that was incidental to my

[4]

work, and which also had a technical flavor and mechanical-electrical interest. It was a cyclic rate control device for antiaircraft machine guns. A U.S. patent is an impressive looking document, complete with gold seal and ribbon.

I got to know something about the Government by being in the Washington scene. I had never been interested in that before, so the exposure did a lot for me.

JOHNSON: You were stationed here in Washington, D.C.?

GOLDEN: I was stationed in Washington, in the Bureau of Ordnance. I had additional duty in the Chief of Naval Operations' office, on war plans, and then in the Secretary of the Navy's office for additional duty. Then, I was at sea from time to time on everything from aircraft carriers to submarines -- but not very long on any of them. And I got to know people who had been in the Government. Some of the people I met in the Navy Department then had previously been in the Bureau of the Budget, and other places in the Federal Government.

JOHNSON: Fred Lawton for instance? You got acquainted with Fred Lawton?

GOLDEN: Fred Lawton, yes. He was Director of the Bureau of the Budget around 1950. I met him primarily through

[5]

Stauffacher and through Elmer Staats. It was Charlie Stauffacher who was Assistant Director, and Elmer Staats, who I met through Charlie. I met Charlie in the Navy. And that's how these things kind of worked.

Well, I had decided in the Navy that after the war I was going to look toward spending maybe half my time on not-for-profit activities that would be interesting and useful. That had been my idea originally when I decided to work in Wall Street and it was working out and I was really going to stick to it. I didn't want to try to be the richest man on Wall Street, or the richest man in the graveyard as the saying goes. So I told lots of people I'm prepared to work on things, without getting paid, that will be interesting and useful.

To shorten this a bit, the most attractive thing that came along quite soon after the war was the Atomic Energy Commission. Lewis Strauss, whom I had met and got to know well in the Navy, and who had been in Wall Street (but I had not known him there), was asked to become a member of the Commission. He knew of my interest in science and technology and in doing some interesting and useful work. He asked if I would come down and help him get it organized. He was one of five commissioners. He asked if I would come down for maybe three months, back to Washington.

[6]

Well, I couldn't think of anything more exciting than the AEC. I was there from the first day, along with the five commissioners and three other staff members.

JOHNSON: The AEC, established by the Atomic Energy Act.

GOLDEN: The McMahon Act that was passed that summer of 1946. It was during that summer, while my wife and I were driving around seeing the USA, we decided that when I got out of the Navy the first thing we were going to do was see the country, before I got into any ruts, new or old. You name the place, and we've probably been there. It was not long, but it was a wonderful experience. We spent seven months driving around the country (and parts of Mexico and Canada). My wife kept a daily travel diary and I had it typewritten and bound: "A Happy Journey."

JOHNSON: What was your position, your role then, in the AEC?

GOLDEN: I was Assistant to Commissioner Lewis L. Strauss.

JOHNSON: All right.

GOLDEN: I was always an independent in politics by the way. He was a very "Republican" Republican as you may know, and Harry Truman was a very "Democratic" Democrat, and

[7]

I have been a very "Independent" Independent always.

JOHNSON: And, of course, you were supporting the civilian control of atomic energy, of atomic weapons.

GOLDEN: Right.

JOHNSON: Did you believe at that time that scientists should be given policy-making roles in the Government?

GOLDEN: Well, I think that I didn't think of it that way, but I think I would have thought so to a degree, yes, as one component of the orchestra. A lot of scientists are very bright people but not very practical. I think now, and would have thought then, they should be a component of the policy-making and I think very much so now.

JOHNSON: Was it necessary to clarify the role of the scientist and the military in the control of atomic energy before steps could be taken to establish a scientific adviser to the President?

GOLDEN: I don't think that had any direct relevance. I should say it had a relevance, but I don't think that was an important element in deciding that there ought to be a science adviser to the President, regardless of how the division of authority would go with military and civilian people.

[8]

JOHNSON: But you were thinking of a civilian scientist, as adviser, not a military scientist?

GOLDEN: Yes, very definitely. But the most important functions were things relating to military matters at that time. That's not so anymore. Now it's important but there are so many others. But then the focus of interest was on military aspects, military applications, and relevance, but not only that.

JOHNSON: Did you feel that the Atomic Energy Act of 1946 nationalized, or socialized, the production and use of atomic energy, and perhaps did this help prepare the way for an advisory committee to the President on science in general?

GOLDEN: Well, I think one thing it did, was to prepare my mind and acquaint me with a very wide range of the scientific community. I got to know them. So I think the answer is "yes," the existence of the AEC and the involvement of scientists in public affairs certainly did help. I hadn't thought of it, but yes, I would say it helped prepare the climate.

JOHNSON: The Military Liaison Committee, that was the contact between the AEC and the Defense Department was it not?

GOLDEN: Right.

[9]

JOHNSON: Did you have much interaction with the Military Liaison Committee?

GOLDEN: Yes, I did. As assistant to Lewis Strauss, I had a very broad charter. I was the only assistant; the other commissioners didn't have assistants, and I think I got along well with all of the commissioners, I think with mutual respect and friendship.

JOHNSON: The military was not lobbying for military control, none of the military people that you knew?

GOLDEN: No. The McMahon Act established civilian control. No, I never saw any signs of lobbying to undo what the Act had accomplished. But the Liaison Committee was very important, just as it sounds.

JOHNSON: In your interviews in 1950 the majority of your respondents seemed to feel the RDB, the Research and Development Board, in the Defense Department was not living up to expectations. But most of RDB's critics did not seem to have suggestions that would remedy the problem. Do you recall what might have accounted for this skepticism about the Research and Development Board? I think some of this does come out in the papers, but if you want to elaborate on it a little.

GOLDEN: Well, I think to give you a really thoughtful response to that would take some time. You stimulate

[10]

me to think back on those days when Bill Webster was the director, and I knew him well. I knew the cast of characters very well. You know, it's much easier to complain than it is to subscribe, so you've said that in other words, and that was true. I would just say that. There was room for improvement but it was a very useful entity and people wanted it to be more useful. Well, yes, it should have been.

JOHNSON: Sure. Apparently, there was some personality conflict between Vannevar Bush and the President, President Truman.

GOLDEN: Well, they were both very strong-willed people. I knew Van Bush; I got to know him quite well. He was very pleasant and helpful to me. I was quite a young man at the time (born in 1909), and he was helpful. He and Lewis Strauss were not the warmest of friends; they were polite and so on, but Van Bush was a very independent character. So was Harry Truman; and Lewis Strauss was, in a different way. So I think there were some, yes, personality tensions there.

JOHNSON: But did that retard progress toward important goals as far as you could see?

GOLDEN: I'm not mindful of that.

JOHNSON: Now Dr. [James B.] Conant, he appeared to favor a

[11]

very strong military role, if not control, in the AEC and he thought the "new OSRD" (Office of Scientific Research and Development) should be in the Department of Defense. Was that not an unusual position for a top-flight civilian scientist to take?

GOLDEN: I would really have to refresh my memory on Jim Conant's positions on these matters. In general, I think what you have said is consistent with my memory. As you stated it, if it's not overstated, and I'm not sure, he was unusual in that; yes, very much so.

JOHNSON: I notice you apparently were especially "sold" on Mervin Kelly, Assistant Director of Bell Laboratories?

GOLDEN: Yes.

JOHNSON: He was in industrial research, and not a university prof or a college president. Kelly recommended [J. Robert] Oppenheimer as an adviser. Was Oppenheimer under a cloud yet in December of 1950?

JOHNSON: No, but yes. Oppenheimer had always been suspect by General Groves, who had kept very close tabs on him and no doubt had him tapped and whatnot. But Groves also used Oppenheimer very effectively. They were a marvelous team, but General Groves was very suspicious, concerned, or mindful of early associations with Communists that Robert Oppenheimer had. There was

[12]

that, but at any rate, no charges had been brought against Oppenheimer at the time.

Let me say right here that I think that was a tragic episode for the United States that those kind of charges were brought against Oppenheimer who was a very talented, albeit somewhat inscrutable, man; who also had a kind of arrogance, which led him to think that he was above the rules in some ways. It got him into trouble, and he lied, and the like. But I think that the bringing of the charges and the revocation of his security clearance were unnecessary. His clearance could have been just left to run off. I think it was a tragic misfortune for the United States, for American science; it divided the camp. There was a clash of personalities underlying all of this. That's an aside.

JOHNSON: Did you interview Edward Teller at the time?

GOLDEN: Well, yes, I knew Teller. I've known him. I don't recall whether I had an interview with him in that collection or not. I don't think so; but I had known him before.*

JOHNSON: Was he ever considered for that role of science adviser?

*Edward Teller is not listed among those individuals interviewed by William T. Golden in 1950-51.

[13]

GOLDEN: I don't think so, not to my knowledge. No, I don't think so. Certainly not by me. It was a very informal situation there in the White House staff and the Bureau of the Budget, the way these things came about. I made my recommendations and Truman took them, presumably took them with him for the weekend; came back on Monday, "approved HST." When Harry Truman had confidence in somebody, like he did, of course, especially in Dean Acheson, he delegated. He had confidence and he went along.

JOHNSON: And Teller, of course, became sort of the antagonist to Oppenheimer and that...

GOLDEN: That came a little later.

JOHNSON: Yes, after the Truman period.

GOLDEN: The conflict of personality was underlying. That was not an issue at that time of my job for President Truman. I got along well with both of them.

JOHNSON: One question that seemed not to come up in your interviews was that of possible conflicts of interest if a corporate executive were to serve as science adviser. Would it not have raised questions if an executive of Standard Oil, DuPont, Monsanto, or such were to be selected for that position? Was there concern at all?

[14]

GOLDEN: I don't think so. The climate was different in those days. You know, it certainly was not of any significant concern, and in my opinion it shouldn't have been if you are dealing with honorable people. It wasn't at that time. But it's different now.

JOHNSON: Getting back to Oppenheimer, according to your memoranda on December 20, 1950 he took a position against a science adviser to the President; later he changed to a kind of a neutral position, and then he had "no confidence that any one man could have a comprehensive knowledge of even the major directions of military research and development." But he came around to accepting the idea of adviser as sort of a "Minister without Portfolio."

GOLDEN: Yes.

JOHNSON: Oppenheimer was not in favor of a "new OSRD," which would have been an idea carried over from World War II.

GOLDEN: That's right. That was one of the questions that was put to me: should we reestablish the OSRD? I said, "No, you should not reestablish it."

JOHNSON: Robert Bacher -- I was going to ask if he was a protege of Oppenheimer's, but it was Robert Serber I was thinking of. What do you recall of Robert Bacher?

[15]

GOLDEN: Robert Bacher was one of the members of the first commission, one of the first five commissioners, a physicist. He and Oppenheimer had worked together at Los Alamos. They

knew each other, and I guess quite well. Bacher was a very thoughtful man, and one of the most effective members of that commission. I realized it as soon as I saw him; he had my great respect and admiration for an open-minded thinking machine.

JOHNSON: Well, apparently Bacher, Oppenheimer, and Charles Lauritsen were all very much in favor of a strong grants program, in the NSF.

GOLDEN: Yes.

JOHNSON: To train new young scientists and post doctorals, too. But Oppenheimer said that basic research could not really be categorized by areas "covered and uncovered."

I think Oppenheimer did see the problems of trying to categorize basic science at least for purposes maybe of an advisory committee, or adviser to the President. He apparently just felt that would be such a difficult job that he wasn't sure anybody could really fill the bill.

GOLDEN: I think that's right, and it was a difficult job. My feeling was that it's better to do as well as one

[16]

can than to have nothing, or to have some elaborate kind of committee structure. But he came around to be fully supportive, Robert Oppenheimer did, as you will find in those memoranda.

JOHNSON: I notice that John Steelman preferred to have somebody with industrial background. I wonder if you have any idea why that might be...

GOLDEN: Well, John Steelman had done a report before my time there, but not so far before, and the Steelman report, in several volumes, really didn't enter into my work at all. I met him once, and that was all. He had withdrawn and he was not involved. As far as I know, he was not consulted. He may have been, but not to my knowledge. It was a very informal group there, of Lawton and especially Stauffacher and Staats. There were few of the President's staff people; there was [Donald] Dawson, the personnel director. It was just a little group. It was a wonderfully enlightening experience for me, and quite easy walking around those halls.

JOHNSON: The Steelman report was part of the background briefing or background information that you used -- or did it offer much help to you at all?

GOLDEN: Not very much, no. It was a useful document, but

[17]

it did not enter into my thought processes particularly at all.

JOHNSON: I noticed Conant continued to favor a committee over an individual adviser, and he believed the National Academy of Sciences should be made more useful, including recommending names for adviser or the committee, and that the RDB must be strengthened. You

told Conant that he and Oppenheimer "were the principal, in fact, virtually the only non-agreers to the idea of a scientific adviser to the President."

GOLDEN: Yes. But then Oppenheimer came round to be quite supportive. I don't remember about Jim Conant. Really he was not in opposition. You're ahead of me because you've looked at the record more recently.

JOHNSON: The Killian Review Group Conferences at the RDB in mid-December 1950 apparently resulted in a recommendation agreeing with your own, that the position of science adviser to the President should be established. You submitted your recommendation for such a post to the President on December 29, 1950 -- although I think that the memo itself was dated December 18.

GOLDEN: That sounds about right.

JOHNSON: Yet, on January 3, 1951 the National Science Board

[18]

of the NSF voted to oppose the plan for a science adviser to the President. The Board feared the science adviser post would reduce the status of the NSF and interfere with its funding. Then in February, the next month, the National Science Board dropped its opposition to the science adviser and decided that emphasis should be, of course, on nonmilitary research.

Now, it's not quit clear to me why this flip-flop occurred. Do you recall the nature of that controversy?

GOLDEN: Well, of course, I was not on the inside of the National Science Foundation, although I had been influential in the selection of the members of it, and very influential, as it turned out, in the selection of Alan Waterman to be the director. Very important on the National Science Board was Detlev Bronk. You've not mentioned his name, but Detlev Bronk was strongly influential in bringing the Science Board around to accepting this document.

You'll find that there is an article, which you may have, by Bronk, that appeared in *Science* many years later.* [It deals with] the genesis, or origin, of

*Detlev Bronk, "Science Advice in the White House: The Genesis of the President's Science Advisers and the National Science Foundation," *Science*, Vol. 186 (October 11, 1974), pp. 116-121, reprinted in William T. Golden (Guest Ed.), *Science Advice to the President* (New York: Pergamon Press, 1980), pp. 245-256.

[19]

the Science Adviser and the genesis of the National Science Foundation. Something like that was the title. That's, I think, quite an important document. It gives the history very clearly from one who was right on the inside. It's in my first book.

JOHNSON: In fact, I have a note here concerning Detlev Bronk, and his article on "Science Advice in the White House," in a special issue of *Technology in Society*, Volume 2, 1980.

GOLDEN: That's where it was reprinted. Yes.

JOHNSON: He claims that General Lucius Clay, Assistant Director of the Office of Defense Mobilization, was the one who changed the idea of an independent science adviser to the President into a committee within the Office of Defense Mobilization. Yet his view was in a minority among the 160-plus persons that you consulted. Do you know why or how Clay had so much clout, apparent clout, in getting this view accepted?

GOLDEN: Well, Clay was a very strong man in the Office of Defense Mobilization. What was done, ultimately, was a compromise. He accepted the idea of a science adviser, an individual who became Oliver Buckley, but had him located in the Office of Defense Mobilization rather than directly reporting to the President. What I

[20]

negotiated out of that was that, first, there would be a single science adviser, with a President's Science Advisory Committee, which I had recommended. But I accepted the compromise that he be located in the ODM, as it was called, but that he would also have -- he or she I'd say now, but I wouldn't have said then -- should have direct access to the President as well. That is the way it came out and is so recorded in the press release that announced the appointment, and so on.*

Now, why Clay had such clout, he was a very strong man. He had had a great reputation rightly as a military man, and he had made the conversion into civilian status quite effectively, which few military do so well. He later went on to be a corporate head. He was strong and able, and influential therefore. So I was disappointed in that, but was glad to get out with a compromise in the negotiations with the Budget Director and the President.

JOHNSON: You mentioned Oliver Buckley, who was the first chairman of that advisory committee.

GOLDEN: He was the first science advisor, yes.

*The press release issued by the White House on April 20, 1951 states, "Eleven of the nation's top scientists were named today by President Truman to a Science Advisory Committee of the Office of Defense Mobilization, to advise the President and Mobilization Director Charles E. Wilson in matters relating to Scientific Research and Development for defense." Also see below.

[21]

JOHNSON: Wasn't his title Chairman of the Science Advisory Committee?

GOLDEN: Yes, it was.

JOHNSON: Was it understood then at the time that he could report directly to the President as well as to the director of ODM with Charlie Wilson, "Electric Charlie" Wilson.

GOLDEN: That's right.

JOHNSON: I guess what you've already said was that he could report directly to the President without going through Wilson.

GOLDEN: Yes, that's correct. And the document of appointment states that.

JOHNSON: Somewhere along the way, apparently in February of 1951, you began referring to an Advisory Committee on Defense Scientific Research as if it were no longer feasible to refer to a science adviser to the President. So there's perhaps a little change in emphasis; or was there any kind of change in emphasis by putting Defense in that title?

GOLDEN: Yes, there was a little because it was in the Office of Defense Mobilization, and because actually what started all of this was the outbreak of the Korean

[22]

War, in the summer of 1950.

JOHNSON: June of '50.

GOLDEN: Yes. That caused some Congressional leaders to press President Truman: "What are you going to do? The scientists were so great in World War II, shouldn't you reestablish the Office of Scientific Research and Development?" The President thought about this, and talked to his staff. Then, they turned to me and asked if I would study the question and advise the President as to what ought to be done about reforming an Office of Scientific Research and Development, or what. It was the outbreak of the Korean War that was the...

JOHNSON: Was the idea initiated in Congress? Was it a joint committee in Congress?

GOLDEN: No, it was Congressional leaders.

JOHNSON: Some Congressional leaders.

GOLDEN: [John] McCormack was one of them, and there was something called the "Big Four," and they would have been the leaders.* I would have to stop and think of the names, but they had an influence with the President and seemed to be pushing him. I learned this from the

*In 1950 the "Big Four" included, besides McCormack, Congressman Sam Rayburn, Senator Scott Lucas, and Vice-President Alben Barkley.

[23]

Budget Director and the Assistant Director. So, they were stimulators of the President.

JOHNSON: Of course, in December 1950 the Chinese intervened; so the fortunes of the war turned, and we were on the defensive. In fact, the UN forces were being shoved back rather rapidly. Did that kind of create a sense of panic on this, or urgency?

GOLDEN: Oh, no, no.

JOHNSON: That didn't add to the sense of urgency, as far as you remember?

GOLDEN: Well, that might have, but I don't recall that there was a dramatic surge. It wasn't like Pearl Harbor, or Sputnik.

JOHNSON: On March 7, 1951, Robert Oppenheimer agreed with you about the usefulness of such a committee. You disagreed with him, however, on the need for this committee to have relations with the NSC, the National Security Council, and JCS, the Joint Chiefs of Staff, on matters of common interest. You said these organizations would have matters of their own to keep them busy, and so apparently you were telling Oppenheimer that you didn't think this committee should get involved with the NSC and JCS.

[24]

GOLDEN: Well, I don't know about that. I'm rather surprised to hear that. I would really want to read all of what I said because I think that's out of context. I'm surprised by that, because I expect I would have thought there should be a closer interrelationship, and so I really had better read what I said about that. If you'll give me that reference, because I think what we're going to have to do, if you think it's worth your time, is continue this over the telephone if you're prepared to do that.

JOHNSON: Well, I didn't have too much left here. How many more minutes do you have?

GOLDEN: I've got about three or four minutes. I'd rather, if you're prepared to do it -- we know each other now -- to call you on the phone. You could put your recorder on, if you want to and we can carry on almost as well. I'd rather do that than just truncate this. I really would like to look up the reference you just said about NSC and me and Robert Oppenheimer being in disagreement about that.

JOHNSON: The date is March 7, 1951.

GOLDEN: Well, that's good enough. I'll find it. Why don't we recess at this point.

JOHNSON: Okay.

[25]

Since Mr. Golden was short of time on June 24, '89, we decided that we would finish this interview with a telephone conversation on August 1, 1989.

[26]

Second Oral History Interview with William T. Golden, August 1, 1989, Washington, D.C By Niel M. Johnson, Harry S. Truman Library.

JOHNSON: I have Mr. Golden on the telephone. I'll start out by summarizing a couple of reports that you did which are in our collections. One report was directed to President Truman on December 18, 1950 in which you argued for a scientific adviser to assist the President on scientific matters. The second report was a memorandum entitled "Program for the National Science Foundation" that you submitted to the Director of BoB [Bureau of the Budget] in February 1951.

I might just summarize by saying that your memo seems to emphasize the value of basic versus applied science; the establishment of a scholarship, fellowship and grants program; and compiling an inventory of basic science projects in the United States.

Then, another major concern was the Research and Development Board [RDB] in the Defense Department and the need to overhaul it. I think that's the subject we want to get into. Again, I have some introductory comments before I get to this question. Your memos of conversation on the RDB indicate general dissatisfaction with the RDB among leaders in American science. There are implications that it suffered from inter-service rivalries, paperwork overload, excessive size of the staff, and maybe even some incompetence

[27]
among . . .

GOLDEN: Maybe some what?

JOHNSON: Some incompetence among staff members. Well, it was kind of a bureaucratic overload; that is the impression I get. And there apparently were problems with controlling its budgets and finances, and so you suggested two deputies under the director; one to review and approve budgets, and the other to deal only with scientific matters.

You had a number of discussions on this issue in January through March of 1951, and you found general agreement on a need for revamping the RDB. Another suggestion that you came up with was to make the RDB advisory only, with no general authority. On April 19, 1951, you submitted a memorandum to Robert Lovett, Deputy Secretary of Defense, about the Research and Development Board.

GOLDEN: What date was that did you say?

JOHNSON: April 19, 1951. And you recommended George Merck as a new chairman for the RDB and the one to reorganize it. But I look in vain for a description of your proposed radical changes in the organization of RDB, and my question is, "Was such a report prepared, and if so, are any copies available of that?"

[28]

GOLDEN: Well, I would not want to rely on my memory of almost forty years ago. I do have in front of me a copy of my volume of memos. Would you tell me again the date of what you referred to, on my memo about the RDB?

JOHNSON: Well, April 19, 1951 was your memorandum to Robert Lovett.

GOLDEN: Let me find that here. Okay, page 427. We'll see how well this is numbered. Your question is whether there is in existence, whether I wrote my observations in greater detail, is that it?

JOHNSON: Yes, basically.

GOLDEN: A copy of that can be found. I won't keep you waiting while I read these two pages of my memo, but does it indicate that I have written a more detailed proposal?

JOHNSON: No, I don't necessarily get that indication. There are clues and hints in some of your memos of conversation about the type of problems with the RDB. I've mentioned some of these concerns and I just wondered if a formal report, or memo, was prepared on that particular situation.

GOLDEN: Well, would it be sensible, Dr. Johnson, for me to

[29]

make a note of this, and look at this and then call you back in a week or so? I'll be away the balance of this week.

JOHNSON: I'll be gone until the 29th of August. I'm going to be gone until the end of August. But there's no rush. If you want to call me back in September and if there is a report like this, we might want to make it an appendix, or get it into your collection here.

GOLDEN: At any rate, let me just jot this down -- some of the things I ought to look into, and try to get back to you as soon as I can. The first thing then is page 427, my memo to Robert and the RDB, and the question is do I have any more detailed description of what I recommended?

JOHNSON: Right, and if you have any knowledge of whether this type of reorganization ever took place. That would be fine.

GOLDEN: Let me jot that question down here then.

JOHNSON: All right.

GOLDEN: I'm writing down this question: Do I find any more details, comments by myself about this. Second, was any action taken; and third, if I don't know, who might know who's still alive; that's the real question. So

[30]

I've got that down on that.

JOHNSON: Okay. Then I notice the last memo for file that you have in your collection is April 26, 1951. Is that when you considered your assignment to be completed?

GOLDEN: Well, let me look and see. April 26: "I've had relevant conversations with the following this week." Is that the one you're referring to?

JOHNSON: I believe that's the one.

GOLDEN: Yes, that's the last one chronologically, and it certainly looks as though, because I was so brief there, that I was winding down. I can look it up, but I would guess that from the look of this that that would be so. In fact, I see here, Friday, April 27, among the people I spoke to, "See Stauffacher" and I put "wind-up" in brackets after it. As he was the assistant director of the Bureau of the Budget with whom I had very close association, that looks that way.

JOHNSON: Well, we can say the end of April it appears that you wound it up. Did you return then to the AEC as a consultant?

GOLDEN: Well, yes, I remained as a consultant to the Atomic Energy Commission for some years. I would have to look it up.

[31]

JOHNSON: I think it was to 1958.

GOLDEN: But that was very much part-time. I then went back to various board memberships and the like.

JOHNSON: I see, yes. I notice Lewis Strauss was a member of the AEC from 1948 to '50 and then was a financial consultant to the Rockefellers in 1950. In 1953 he was named chairman of the AEC. Do you recall what kind of a relationship Strauss had with President Truman?

GOLDEN: Oh, yes. His relations with President Truman were quite cordial. There was an intimacy and a mutual confidence that was encouraged by the relationship that Admiral Strauss had with Sidney Souers, also a reserve Rear Admiral, who was a very close associate of President Truman. He would have known him from St. Louis for many years and who was, after a career in the Navy in World War II, the first head of the National Security Council.

JOHNSON: Right.

GOLDEN: Admiral Souers and Strauss were quite friendly, in a mutually respectful way. Admiral Souers and I had a very close relationship, I think growing partly out of that.

JOHNSON: Do you know why he resigned there in 1950 from the

[32]

AEC?

GOLDEN: No. I think he had, for quite some months, been building up to resigning, but why he chose that particular time I don't know. In fact, I was gone already at that time. But he had been talking about it on and off. He mentioned to me that well, he wanted to pick a time to retire from the commission, so he had been brooding over that. But I don't know why he picked that particular time.

JOHNSON: I see. R. Gordon Arneson, who was assistant to Webb, says he was persona non grata with Admiral Strauss.

GOLDEN: R. Gordon Arneson said he was persona non grata with Strauss?

JOHNSON: Do you recall anything about that?

GOLDEN: No, I don't recall. Admiral Strauss had rather clearly defined judgments about people. I just don't recall what his feelings were about Gordon Arneson. Of course, I remember Gordon Arneson, but I don't recall that.

JOHNSON: Strauss, I understand, did lobby for developing the hydrogen bomb.

GOLDEN: Yes, he did.

[33]

JOHNSON: Were you involved in any discussions about that with Admiral Strauss?

GOLDEN: Well, I was involved very much in conversations about that "Super" as it was called in those days. It was just in the normal course of our conversations and considerations at the Atomic Energy Commission while I was there. Then, I wrote a letter to Admiral Strauss which was in the general documentation, I find; I handwrote him a letter in September, 1949, a classified, top secret letter from Florence, Italy which I sent by the diplomatic pouch. That was written immediately, immediately the day after, or the night of, the announcement by President Truman that the Russians had exploded an atomic device.

I wrote him, setting forth why I thought active steps should now be taken to go after the hydrogen bomb on our part, while also carrying on concurrently negotiations with the Russians, to try to prevent the necessity for actually making any such weapons. In other words, I wrote, urging that we go ahead with the research stages, but not do any testing.

Are you familiar with that at all?

JOHNSON: Well, somewhat.

GOLDEN: Well, I mean the letter is of some significance and has found its way into the Government records. I'm

[34]

trying to think if it's in the National Archives or just where. That's referred to in McGeorge Bundy's recently published book on the atomic bomb which is a very important book.*

JOHNSON: Sure, we'll check on that.

GOLDEN: Now, the letter itself if you want it, I could send you a copy of it. That was classified at the time, but it has long been declassified now. Would you like me to send you one?

JOHNSON: That would be fine if you would.

GOLDEN: Well, I will do that. I will send a copy of the H-Bomb letter. That's a matter of some significance.

JOHNSON: As you recall, what problems were the most important when you were assistant to the commissioner of the AEC from '46 to '50? I guess that means when you were assistant to Gordon Dean and David Lilienthal?

GOLDEN: No, I was assistant to Admiral Strauss exclusively, and that was a staff relationship. The other commissioners did not have assistants.

JOHNSON: I see.

*See McGeorge Bundy, *Danger and Survival Choices about the Bomb in the First Fifty Years* (NY: Random House, 1988), p. 204.

[35]

GOLDEN: I was there from the first day of the commission. I had met Admiral Strauss in the Navy; that's how we knew each other. He knew of my interest in public service, at least in part-time. He knew of my interest in science and in national defense matters. When he was asked to become a member of the Commission, he called me. In fact, he called me in his kind of, what shall I say, courtly manner, to ask my advice as to whether he should accept the appointment and if he accepted it whether I would come down and perhaps spend three months in, as he put it, "helping to get the commission organized."

Well, I was delighted to do that and the three months turned out to be three years. But I did not want to stay to make a career of it as I had been asked to do.

JOHNSON: One of the persistent issues seemed to be the military's involvement in policymaking with the AEC. There was a Military Liaison Committee I believe.

GOLDEN: That's right.

JOHNSON: I don't have much information on this, but did this issue come to a head again in 1952? Do you recall this military versus civilian issue coming to a head, perhaps in 1952?

[36]

GOLDEN: I do not recall that one way or the other. It certainly wasn't something so prominent that it registered with me. I don't think I would have forgotten it. The big issue occurred prior to the passing of the McMahon Act in the summer of 1946. There were the two bills, as you know, the May-Johnson bill and the McMahon. The May-Johnson would have put control in the hands of the military, and the McMahon was the one that created the five-member civilian commission.

That was very, very hotly debated. I was not involved in that, but when the issue was decided, with the McMahon bill being passed, and enacted, that's when the commission started.

I don't recall the issue arising in any active way, but that doesn't mean that it didn't.

JOHNSON: As far as you know the civilian-military relationship was rather steady or harmonious in the years that you were with the AEC?

GOLDEN: I would say so. Now you must realize that Admiral Strauss, and I, too, for that matter, had been in the service, in the Navy, throughout World War II. Both of us were there before Pearl Harbor and he long before Pearl Harbor. I think that we both had done understanding of military people, of military affairs, and

[37]

had the respect of some of the members of the commission who had not been.

Now, let's just consider them. David Lilienthal was not in the military during the war. Bob Bacher was not; he served in a very active, equivalent scientific capacity. [William] Waymack, I'm almost certain, was not in the military, and Sumner Pike, I'm almost certain the record will show he was not in the military either. They were all patriotic citizens doing other things to aid the war effort. But it is a very different thing when you live with the military and you get to see their weaknesses and also their strengths; at any rate, to form opinions about them. So, I think that Admiral Strauss and I got along well and were, let's say, respected and trusted by the military, probably more than the civilian commissioners were. I mean, I was not a commissioner you see. But the relations, I think, were fine. That is to say, I think Dave Lilienthal was somewhat uneasy with the military, and they with him, but that was not an issue. The act was passed and the law was being followed. It was "res judicata," the matter was settled.

JOHNSON: Sure.

GOLDEN: But I think we got along well. Now, General [Leslie] Groves would come back frequently, [as would]

[38]

other military people, from the Military Liaison Committee and others. As far as I was concerned, we were teammates.

JOHNSON: Did you ever have a chance to talk to President Truman, and if so, what were the circumstances?

GOLDEN: Well, just very briefly, when I was asked to undertake this job for him, to study this organization of the Government scientific activities following the outbreak of the Korea war, that you and I have been talking about.

JOHNSON: Right. You stayed on in through the Eisenhower administration. Do you recall...

GOLDEN: I didn't stay on. I was involved again in the second Hoover Commission. (in 1954 as I recall it), concerned particularly with military and scientific matters.

JOHNSON: Did that bring about any substantial or significant change in executive organization, especially relating to scientific matters, this Hoover Commission?

GOLDEN: Well, it had some influence, from the parts that I saw. As you know this was a very big undertaking, covering all the Government. It had some influence but

[39]

not a great deal. I don't think the follow-through was up to the effort that went into the study.

JOHNSON: I see.

GOLDEN: On the other hand, these studies always have an influence on the people being studied. So the relationships that I had during that period, and other members of our military taskforce, military and procurement taskforce had, with the Joint Chiefs and with other people, the Joint Staff people, all had some influence but not of a dramatic character.

JOHNSON: Right. During the Eisenhower Administration we had Charlie Wilson as Secretary of Defense and then that famous quote about "bigger bang for a buck?"

GOLDEN: Yes.

JOHNSON: Which apparently meant more reliance upon nuclear deterrent, upon nuclear weapons, and less on conventional weapons. Did you see any great departure there from Truman's policies, or is this sort of exaggerated?

GOLDEN: Now say that again. Any great departure...

JOHNSON: From the policies, the nuclear weapons policies in the Truman Administration, as compared to Eisenhower and Wilson's policies on nuclear weapons and priorities

[40]

for nuclear weapons. Did you see any change that you noticed in the thinking between the Truman and Eisenhower administrations in regard to reliance, greater reliance, on nuclear weapons?

GOLDEN: Well, I think I'd have to speak rather generally about that. As time passed, I'd say immediately after the war, the feeling was that the nuclear weapon would give the United States such an advantage that it wouldn't have to be used and our influence would be very great toward a peaceful world. It was recognized that the Russians were going to get it, unquestionably. The scientists thought the Russians would get it much quicker than General Groves thought, and General Groves thought it was going to take ten to twenty years. That can be looked up, because he made a public statement. At the moment, I'm not sure whether he said ten or twenty, but it proved to be much sooner. It was hoped that there would be efforts for negotiations along the lines of the Acheson-Lilienthal report and the Baruch report which is essentially based on it.

But gradually the feeling has become one of increasing recognition of peaceful uses of atomic energy in the atomic age being more important, and that atomic weapons should never be used again. So I think that feeling of avoidance of use, and the need for negotiations to prevent them, has grown and continues

[41]

to grow as the power of the weapons increases, especially with the hydrogen bomb so-called. So that trend, I think, was evident during the period from Truman to Eisenhower and certainly subsequently -- that it's just unthinkable that the weapons should be used.

JOHNSON: Right. Nuclear energy for electrical power production, that apparently came into use during the 1950s. I don't recall right now when the first nuclear power plant was completed.

GOLDEN: Well, I think it was in the early 1950s, and certainly it was during the period of Admiral Strauss' chairmanship as I recall, that the first nuclear reactor producing electric power went into effect. There had been, of course, great enthusiasm for that, and especially for the breeder reactors, great enthusiasm by the Atomic Energy Commission, even during the time I was there, looking toward the production of power. It was looked to as a wonderful development for society. And there was not much worry given to the possible dangers, and how to avoid them.

JOHNSON: Nuclear waste was not an issue?

GOLDEN: Nuclear waste was not a big issue. People didn't think about the garbage problems very much at the time; there was very little. I mean they were not unaware of

[42]

it, but it was not regarded as the big problem that it has in fact become.

JOHNSON: And the breeder reactor has not proven feasible has it?

GOLDEN: I think it has not. I'm really not qualified technically. I think it's feasible, but the question is economic feasibility. There seems to still be a problem of economic feasibility with safety factors considered; I think that is somewhat in the future, but it's not at all impossible.

JOHNSON: But there was quite a bit of optimism in the 1950s about it?

GOLDEN: Oh, yes there was. Obviously it's a very attractive concept. It's like perpetual motion in a way -- you get more out than you put in. It still is, and the time may come when it will be a very important development to be sought.

JOHNSON: Was fusion being considered there in the '50s as a possible source of power production?

GOLDEN: Well, fusion was very much being considered and there were projects. Our principal project was at Princeton, with controlled fusion. That is still eluding us, but it is being closed in on.

[43]

JOHNSON: Okay then...

GOLDEN: The Russians seemed to be ahead of us in that field at that time with the so-called "tokamak." But practical, controlled fusion is still being sought.

JOHNSON: Now, if I can just back up for the final two or three questions, mainly to the Truman period...

GOLDEN: May I just go back? You referred to it before. We got on briefly to the subject of the second Hoover Commission and the military procurement taskforce. I should mention here that I filed an opinion of non-concurrence; I didn't want to call it a dissenting opinion. The general feeling within the taskforce was a very harmonious one, and also a thoughtful and serious one, but I had some different views and was not able to persuade my colleagues in the report. So the report as published carried a separate opinion by me, which I called an opinion of non-concurrence.

JOHNSON: I see.

GOLDEN: And that's in there. It's well to record it here.

JOHNSON: That's part of the printed record?

GOLDEN: That's part of the printed report of the taskforce on military procurement.

Now, you were going to ask me something.

[44]

JOHNSON: The Scientific Research Advisory Board was set up, of course, in ODM as an outcome of some of your research and your polling of these various scientific leaders. Do you feel that it did do the job that it should have done or was expected to do?

GOLDEN: Well, I think it served a very useful purpose in that it initiated the era of a science adviser to the President and a President's Science Advisory Committee. Now, it was watered down. President Truman approved very promptly my recommendation, in my report, and it reads

"Approved HST." But then the negotiations began within the upper levels of the staff and General Clay, who was a very distinguished military man, who became the Deputy Director of the Office of Defense Mobilization, did not want to have a science adviser reporting directly to the President, and thereby competing with him.

JOHNSON: Right.

GOLDEN: And so the infighting in a polite way began. And the final negotiation that I was able to get as a compromise to putting the organization entirely in the Office of Defense Mobilization was to provide -- and the published announcement from the White House states this -- was to provide that the chairman of that committee would have direct access to the President as

[45]

well as being located in the Office of Defense Mobilization. In fact, I happen to have it in front of me. It reads, "Eleven of the nations top scientists were named today by President Truman" -- today being the release date April 20, 1951 -- "to a Science Advisory Committee of the Office of Defense Mobilization to advise the President and Mobilization Director Charles E. Wilson, in matters relating to Scientific Research," and so forth.

So, that was of course, watered down from what I had recommended to the President, and what he had approved -- that this be an appointed person directly reporting to the President alone.

Subsequently, after Sputnik, President Eisenhower then went back to the original recommendation in full and created the President's Science Adviser reporting to the President and the President's Science Advisory Committee. So, I know that my report proved to be the architectural design which was followed by President Eisenhower, though it was used in a diluted form by President Truman.

JOHNSON: Right.

GOLDEN: And that, of course, has prevailed. It's been reinvigorated now again by President Bush.

JOHNSON: The National Science Foundation, of course, was

[46]

the other thing that you were very much concerned with and involved in. Do you feel that it, in its early years, performed as it was expected to?

GOLDEN: Well, let me go back a moment to the Science Advisory Committee, and your question, did it perform. I want to comment on that. It's true, Oliver Buckley, who was chosen to be the chairman of that, was a very respected scientist, who was the chairman of Bell Telephone Laboratories, and he had been an important figure in the Office of Scientific Research and Development during World War II. He was not my first choice for the job. My first choice for the job was a man named Mervin Kelly, who was the President of Bell Telephone Laboratories, and subordinate therefore to Buckley. Mervin Kelly was a man who was deeply involved in

military matters, military science and engineering matters, who I had talked with many times as this document you have shows, and who had my and other people's very great respect.

He was talked with about the job, but he decided that he did not want to take it, because he didn't want to take it as a full-time job, which I felt, and my recommendation said, it should be, and I hold to that. So, in the negotiation Bell Labs didn't want to give him up and they said, "But we'll give you instead somebody, his superior, Oliver Buckley, who is getting

[47]
ready to retire."

Well, Buckley was a lovely man and I knew him well too from these talks. But he, it turned out, had Parkinson's disease, which was not publicly known and he had slowed down already. While Mervin Kelly was courageous, Buckley was timorous. The consequence was that this Science Advisory Committee was much less effectual than I had visualized that it should have been and that it later became under President Eisenhower's revitalization.

So, the problem was, we were dealt a man off the bottom of the deck in getting Oliver Buckley who was a fine, patriotic man, but was ill and excessively cautious.

JOHNSON: The National Science Foundation, did that meet your expectations better?

GOLDEN: I would say, yes. I would say so. I think that did quite well. It started in low gear and moved along. I think it did quite well.

I want to refer you here, although I'm sure you have it, to a very important article by Detlev Bronk, which appeared in the journal *Science*.^{*} You must have reference to it. It's called, approximately, The Genesis of the President's Science Advisory

^{*}See footnote on page 18.

[48]
Organization and the Beginnings of the National Science Foundation. That isn't the exact title; that's the subject of it. Dr. Bronk was a very respected figure who was a member of the first National Science Board, who was president of the National Academy of Sciences, who later became president of the Rockefeller University, and so on. That paper of his in *Science* is a crucially important historical document on both of these matters.

JOHNSON: I think that's in the book *Science and Technology: Advice to the President. Congress and Judiciary*.

GOLDEN: That's right. It was reprinted in there. You are quite right.

JOHNSON: And Waterman was the first director.

GOLDEN: Alan Waterman was chosen as the first director. I had considerable influence in that choice, through being consulted by President Truman's personnel person (Donald Dawson) to give what proved to be the decisive opinion about who President Truman should appoint.

JOHNSON: Right.

GOLDEN: I think that was a very correct and wise and fruitful decision by the President.

JOHNSON: Maybe as a final question here; this is one of the

[49]

questions I raised with you in our previous interview, and I think you wanted to check up on it. I notice that on March 7, 1951 Robert Oppenheimer agreed with you about the usefulness of an advisory committee to the President on science. Well, he also suggested this committee should have relations with the National Security Council and the Joint Chiefs of Staff on matters of common interest, but you pointed out something. I'll read that sentence here: "When I pointed out that these organizations had matters of their own to keep them busy and that one could not expect them to turn with any enthusiasm to such a committee, he said he realized that that would be so." So the NSC and the JCS, you didn't expect much enthusiasm from them for this type of committee. Of course, the JCS already had the RDB didn't they, the Research and Development Board, in the Defense Department?

GOLDEN: In the Department, not reporting to the Joint Chiefs, but in the Department reporting to the Secretary.

JOHNSON: Yes. Do you know if there was much coordination with NSC and JCS after the committee was established, or was that sort of out of their bailiwick?

GOLDEN: That I have in front of me now, too. That will

[50]

only take me a minute to read. Now your question is, was there much of a relationship between the President's Committee on Defense Scientific Research and the NSC and the Joint Chiefs.

Well, I don't know, because I was not a member of it. I don't know, but my impression is that the relationships, because of Dr. Buckley's timorousness, that the relationships were neither extensive nor profound.

JOHNSON: Right. Nor productive?

GOLDEN: Well, I have no reason to think they were but neither do I know that they weren't. I would say under his leadership they would not be. You have to push when you're dealing with the Joint Chiefs or with the NSC, even then. And certainly with the Joint Chiefs, he would not have gotten very far. With the NSC, relations should have been cordial, but I don't recall, and people who would know, like Jimmy Lay and Admiral Souers, are dead. So I'd just better say I don't know.

JOHNSON: Well, is there anything that you want to add to conclude this interview? Any question that you feel might be pertinent that I haven't raised?

GOLDEN: Well, I don't think so. I think that you have asked the appropriate questions. I've responded to the

[51]

best of my ability, recognizing the lapse of years with an appropriate humility. I have made notes of these questions, the two questions, and I will get back to you on those promptly, and I will send you a copy of my then-top secret, now declassified, letter to Admiral Strauss.*

JOHNSON: I'd appreciate that.

GOLDEN: I refer to Mac Bundy's book, and I really don't think of anything else, but when I get the typed transcript of this conversation that may set me to thinking. If so, I will incorporate any comments in my markup of it.

JOHNSON: That's right.

GOLDEN: I hope this work will prove of some use to some historians or political scientists in the years to come.

JOHNSON: I'm sure it will. I appreciate your time, and the information you have given me.

GOLDEN: Well, I'm very glad. It's been a pleasure meeting you, and been a pleasure to think back on these things. We're always concerned with the future, but

*A typescript copy of this letter is appended to this transcript.

[52]

the past can be of some help in that

JOHNSON: Certainly. Thanks again.

APPENDIX

[Transcribed June 1982 from 1949 handwritten letter]

Florence, Italy
Sunday, September 25, 1949

Dear Lewis,

My knowledge of the Russian success is limited to what I have been able to make out from the Italian newspapers of yesterday and today. Tomorrow the Paris *Herald-Tribune* should be here -- Saturday's, that is -- with the combined announcement.

But it is clear enough to have generated some thoughts, all obvious enough, and, for want of you to talk to, to impel me to set them down. So here they are, at random, to relieve my internal pressure and on the remote chance that you will find something useful or suggestive in them.

1) We -- the USA -- should intensify our efforts toward development of super-weapons. This is much more important than increase in production rate of existing weapons. A quantum jump in intensity is called for as a matter of urgency comparable in every way to the wartime Manhattan project. This I regard as urgent and of supreme importance. I can conceive of (though I hope unwarrantedly) one or more of your fellow Commissioners wishing to go slow, awaiting some international control arrangement with Russia. You must drive for concurrent development work with wartime urgency.

2) This is likely to delay, indefinitely, your departure from AEC -- unless a position of superior significance were offered to you.

3) This pretty much destroys your campaign against production in U.K. The popular feeling will be that if Russia now has the bomb, it is better to have the U.K. make them too, for they will be friendly to the USA and whatever they produce will be net gain for our side. The validity of this argument is more apparent than real; but, regardless, your former position becomes indefensible (irrespective of its soundness) and therefore should be abandoned. This you can do without loss of face, for conditions have changed.

This does not mean that the U.K. should necessarily be made privy to all information -- particularly with regard to

[2]

new weapons of super potency. But we should now help them to make the "Mark I" if they insist on so doing. Doubtless their scientists can help on "super" development and they should be enlisted. There is very real, though intangible, value in a partnership rather than an arm's length business relationship; and I feel, therefore, in the light of the Russian bomb, that you should not insist on too formalized an arrangement. I do not mean that the relationship should be a vague gentleman's agreement either, for that would be both imprudent and unacceptable to the Congress. But England must be our ally. Let us make her a happy one.

4) To be expected is revival of international control schemes. I am most skeptical of any good coming of these -- for I would have no trust whatever in Russia's word -- but they should be studied and pursued with zeal -- both as a matter of moral conduct and in response to public clamor. It is likely that some study group like the Acheson-Lilienthal ad hoc committees of 1946 will be set up by the State

Dept. or State-Defense-AEC. It is certain that this will be nominally non-partisan or bipartisan, but the choice of members will be important and you should actively interest yourself in it. Mere good will toward men should not be a qualification. It would come nearer to being a disqualification. For example, I would regard Bob Bacher as an excellent member but Bill Waymack (despite my affection and esteem for him) as a most undesirable one.

S) Our (U.S.) policy should be to load and aim the guns (but not to proclaim it) while earnestly negotiating for a non-use pact.

6) What is the status of the underwater submarine mine? Of other weapons under development? These and others should be pushed. You should be in the forefront of this. Otherwise it will surely lag. Probably it needs a leader -- someone in charge of new weapon development: a kind of acceptable (to scientists and the military) General Groves. Offhand, in the military, only Admiral Parsons comes to mind as qualified, but he is probably not available and, anyway, a civilian, provided the military trusts him, would be much better. It would be a mistake to seize on some retired or soon-to-be-retired Navy or Army man. This is an industrial problem in the scientific field.

7) It is noteworthy that General Groves has been completely discredited by this early Russian achievement versus

[3]

his forecasts of many years. His almost complete disappearance from the public eye is also notable. Surely he'll feel impelled to make some statements. But this is not important.

8) I presume that the knowledge has come through LRD: [Long Range Detection] and your pressure for attention to this -- despite lethargy in the military and civilian agencies -- should give you great comfort and justify to yourself the thankless years you've spent at the AEC.

9) Naturally I hope we have also a good idea of the efficiency achieved -- and that it was low.

10) I am, as I know you are, a strong believer in competition as a stimulant to ingenuity and enterprise. Our development program in the U.S.A. has, of course, lacked competition and cross-fertilization with foreign ideas after the initial phase. Obviously I'm not advocating the welcoming of any or all nations into collaboration. But perhaps something can be worked out with the British in current negotiations which will set them to work on super developments, and lesser improvements, in ways different from ours. The danger in isolation is that one day -- even though it may be many years off -- we may be awakened from our Maginot complacency with a relatively super weapon in Russian hands. Something of, say, ten times (to be modest!) the potency of the existing weapon

really would be a peacemaker if the Russians had it and we didn't -- for, could they deliver it over London and our cities, it would be all but impossible to stand up against.

11) This brings up the matter of immediate use, or threat of use, of our weapons. Let us not delude ourselves, to bring about a true international control agreement with Russia we would have to use them. The consequences would be dreadful indeed, even though I assume that the Russians have so few A bombs now that they could do little or no damage to the U.S.A. even if they could put them on the target.

In theory we should issue an ultimatum and use the bombs vs Russia now: For, from here on, we inevitably lose ground. And this is true no matter at how much greater a rate we produce no matter how much more potent weapons. For once Russia is in a position to put A bombs on our cities, no matter how inefficient those bombs may be and how few in number, she is in a position to do us unspeakable injury. That we can retaliate a hundredfold, or wipe out every Russian, will not repair the damage.

[4]

So a good, though amoral case can be made by the Disinterested Man from Mars for our shooting at once. Studies along this line, and in more dilute version, should and doubtless will be (or have been) made by the Joint Chiefs. And when we get home, Sibyl will without doubt intensify her campaign to this program on Art Davis now that he, I hear, is to head the Joint Staff! However, we won't do it, of course: no matter what the alternative cost in the long run, the public would never support so far-seeing a bombardment. Still it should be kept in mind: by the AEC, the State and Defense Departments, and the altruistic negotiators for international control. And, certainly, we should have advance bases in readiness, and planes and crews trained and "aimed on target."

12) Also, unfortunately, we shall lose standing and influence in Europe and the rest of the world. For power talks. And if Russia has the Bomb, too, she will be more a factor to be reckoned with and a power not to irritate. The nations of Europe will be pragmatic, to coin a word! Consider the Germans. We'll get the lip service but the USSR will get a lot more respectful attention than she would have a few days ago. And the Communist elements in Italy and France, for example, will gain followers from the large in-between groups. This is too bad but we'd best not deceive ourselves. Russia is still nearer to Paris than we are.

13) Today in Florence, by coincidence, there was a very large Communist parade, with delegations from the major cities and industrial plants of all parts of Italy. These people, most of them, seemed to have real fervor. They were for something and against something else. Most of them doubtless were mere camp followers. But many appeared to have great zeal, the revolutionary spirit. You recall the Spirit of '76 painting, the one with the drummer, the fife player, and one or two

others marching bandaged, over the debris of battle. Something of that fire was in the eyes of these Communist marchers in Florence today. To combat this spirit we must supply to the people a similarly stimulating program. We in America, with our flair for advertising and promotion, should do a better job in showing the decaying nations of Europe the blessings of our way of life and in stimulating them actively to strive for it. We have here, as in many cities we've been in, in France and Austria for example, a U.S. Information Service Office, under the State

[5]

Dept. Tomorrow I plan to stop in to see the director of the office here to find out what he is doing, what he thinks of the big Communist parade here, and what he believes to be the way to combat the popularity of Communism and Russia in Italy. I should say that, though there were several floats and displays condemning "American Imperialism" and the Marshall Plan (it has put people out of work, the Red posters proclaimed, through mechanizing industry and in other anti-social ways), the principal theme was not anti-U.S. but pro-Russia. There were many large picture banners of Stalin and Lenin. There was no ambiguity, the paraders loved Russia, not just some philosophic idea of Communism. What they proclaimed they wanted was Peace and Work. And this they believed Russia would give them. We had better find out what kind of whiskey Stalin drinks, to honor the memory of Abe Lincoln (was it he who said it?) and dish out double potions. For the Russians sure have these people bamboozled and we'd better learn how they do it.

I apologize for the length and rambling character of the foregoing; but I feel better for having it off my chest.

I now have your two-page letter of 15 September and was distressed to learn some of the details of how unpleasant a time you've had. But you know that; so I'll not say any more. As to Virginia's loyalty, and Mitch's, there could never be any infinitesimal shadow of doubt; so your story of the attempted seduction, or rather its failure, didn't surprise me any more than it did you.

You speak of leaving the AEC, perhaps in December. I doubt it, now. You ask about filling "my" office. Of course, you have always been free to do this and, when I left, had at least one idea in active negotiation after another invitee had declined with regret. The shorter the term you have in mind for yourself, the more difficult it will be to find anyone to take the job, and the less you could in conscience urge him to do so.

My own attitude is precisely what it was when I left: If you want me back and have use for me, I am at your command. There is nothing I would like better, now and for the unlimited future -- for life, that is -- than to continue my association with you. I do not think you are perfect and, as I reflect on it, I have perhaps been insufficiently forceful in volunteering differences of opinion and taste. You have few friends; I am one of them. You have many admirers, many servants. You value loyalty above all virtues but find it scarce. In consequence you,

[6]

figuratively, employ many mercenaries and this does not make for a stable foundation--though it may well be the best, as you will recall from A. E. Housman's poem, Notes on an Army of Mercenaries, or some such title, about "...What God abandoned, these defended / And saved the sum of things for pay...." Valuing loyalty so highly, you are yourself, when you believe it at issue, the most loyal of friends. I have learned a great deal from you, all of it of practical value in a practical world. I could go on this way, but this sort of musing and reflection is far better done side by side and by a fireside -- say at Brandy -- of a winter's night without distraction of others. In soliloquy it does not flow well, lacks the stimulus of response, and is in hazard of ambiguous phrasing or misconstruction. It isn't really irrelevant, as I was about to say, and perhaps, in fact I'm sure it is, it is very basic to my whole viewpoint.

But I will also resist the inclination to tear up all the foregoing personabilia. It would be easier to do so, safer. But it is better left said, both for itself and as an entree to continuation sometime.

So, to return: I would like to resume a relationship with you. Actually it is unthinkable that I should not in some way do so. I should also like to retain some association with AEC, for it interests me as it always has. I should prefer the AEC work to require something less than the virtually full time it has heretofore. Actually I should not object to almost full time if there were work to keep me occupied: but in the past there has not been, and I think it most unlikely that there will be.

You may recall that toward the end of the war and then again just before you accepted the Commissionership, I expressed my feeling that the way I thought I would like to employ my time for the foreseeable future would be roughly half for interesting, useful non-business pursuits and the remaining half for business. There is no change whatever in my taste in this regard. If anything, it has been confirmed by the 22 years with you at AEC. And if I would modify the ratio at all, it would be, assuming I continue to be able to afford it, in the direction of reducing -- but not eliminating -- the business quota.

You certainly have no obligation to me whatsoever in relation to AEC or in any other way. If anything, the obligation lies the other way, for doubtless to some extent, however

[7]

slight, I may have influenced you to accept the Commissionership. Despite the recent tension, and its unpleasantness for you, I would repeat the recommendation, for, in retrospect if not right now, I am sure you'll count it a most interesting and rewarding experience.

I think the above is clear though verbose. I stand exactly as I have all along and, specifically, as I told you before leaving for this educational expedition, if you have use for me and want me back to whatever extent, I shall be completely and happily, at your disposal.

One thing more: You mention in your letter that Herb and Ann Marks "have been reported" to you as active in spreading stories to the effect that you are an isolationist. I haven't the remotest idea where the truth may lie (as to the stories, not as to your convictions!), but I do recall that Herb was a most enthusiastic admirer of yours. The best way to find out would be to ask him.

And, if you are at all concerned about the spread of stories in whatever circles he would spread them -- and you must be or you would not have mentioned the matter -- the best way to have these stories corrected and correctly spread would also be to have a talk with him.

It is now very late indeed and I shall go to sleep. We arrived here last evening, have already seen some beautiful things (not including the Communists' parade, but that was interesting), shall stay a few days, and should reach Rome about Thursday or so of this week. I imagine we'll stay there ten days more or less, probably at the Hotel Hassler or Grand or Excelsior -- opinions on their virtues differ. Then we may fly over to Greece for a few days and then proceed to the South of Italy.

Sibyl is asleep but would certainly want her most affectionate greetings to go along with mine, to you and Alice. It's a long time since we've heard from Alice and we miss her letters. Please tell her so and remind her that we think of her and hope she'll write.

Ever,

Rear Admiral Lewis L. Strauss
U. S. Atomic Energy Commission
Washington

Index

- Acheson, Dean, 13
- Acheson-Lilienthal report, 40
- Arneson, R. Gordon, 32
- Atomic Energy Act of 1946, 6, 8
- Atomic Energy Commission (AEC): 5-6, 30-31, 33, 35-36
 - Peace time use of nuclear energy, 41-42
- Atomic Weapons, Military/Political Reliance on, 39-41

- Bacher, Robert, 14-15, 37
- Barkley, Alben, 2, 22
- Baruch report, 40
- Breeder reactor, 41-42
- Bronk, Detlev, 18-19, 47-48
- Buckley, Oliver, 19-21, 46-47, 50
- Bureau of the Budget, 4, 13, 20, 26
- Bundy, McGeorge, 34, 51
- Bureau of Ordnance, 4
- Bush, George H. W., 45
- Bush, Vannevar, 10

- Civilian control of atomic energy, 7, 36
- Clay, Lucius, 19-20, 44
- Conant, James B., 10-11, 17
- Conflict of Interest, 13-14

- Dawson, Donald, 16, 48
- Dean, Gordon, 34
- Defense Department:
 - Research and Development Board, 9, 11, 17, 26-27, 49

- Eisenhower administration, 38-39, 41, 45, 47

- Groves, Leslie, 11, 37, 40

- Harvard Business School, 3
- Hoover Commission (second): 38-39, 43
 - Military procurement task force, 39
- Hydrogen Bomb Decision, 32-34
 - Appendix – September 25, 1949, letter to Adm. Strauss from W. T. Golden
 - Thirteen Points on Russia with the A Bomb and U.S. Response
 - Personal comments in response to concerns of Adm. L. Strauss

- Joint Chiefs of Staff, 23, 49-50

Kelly, Mervin, 11, 46-47
Killian Review Group Conferences, 17
Korean War, 21, 38

Lauritsen, Charles, 15
Lay, Jimmy, 50
Lilienthal, David, 34, 37
Lawton, Fred, 4-5, 16
Lovett, Robert, 27-28
Lucas, Scott, 22

May-Johnson bill, 36
McCormack, John, 22
McMahon Act, 6, 9, 36
Merck, George, 27
Military Liaison Committee, 8, 9, 35-36, 38

National Academy of Sciences, 17
National Security Council, 23, 31, 49, 50
National Science Foundation, 15, 17-19, 26, 45, 47
Nuclear Waste, 41-42

Office of Defense Mobilization
 Scientific Research Advisory Board, 19-20, 44-45
Oppenheimer, J. Robert, 11-17, 23-24, 49

Pike, Sumner, 37
President's Committee on Defense Scientific Research, 21

Radio, 1
Rayburn, Sam, 22

Science Advisory Committee, 21, 44-48
Science advisor to the President, 7-8, 14, 17-20, 45
Souers, Sidney, 31, 50
Staats, Elmer, 5, 16
Stauffacher, Charles, 5, 16, 30
Steelman, John, 16
Strauss, Lewis, 5-6, 10, 31-33, 34-35, 37

Teller, Edward, 12-13
Truman, Harry S., 6, 10, 13, 22, 26, 31, 33, 38-41, 44-45

United States Navy, 3-5, 31
University of Pennsylvania, 2

Wall Street, 3, 5

Waterman, Alan, 18, 48

Waymack, William, 37

Webster, William, 10,

Wilson, Charles E., 20-21, 39

Appendix: September 25, 1949 letter William T. Golden to Admiral Lewis Strauss