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AN INTERVIEW WITH ALVIN WEINBERG

FOR THE

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INTERVIEWED BY

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STOW: Today, we're talking with Dr. Alvin Weinberg. Alvin came to the Laboratory in the early 1940s and became director of the Physics Division, research director of the Laboratory in the late 1940s, and Laboratory director from 1955 to 1973. As we look back over the entire suite of individuals who've spent their careers at Oak Ridge National Laboratory, he is perhaps the most famous and most admired individual because of his ability to communicate well and his tremendous insights on science, politics, information science, scientific administration, and the entire area of nuclear technology. So, we'll enjoy a good discussion with Alvin today.

WEINBERG: You asked how I happened to make a transition from biophysics to nuclear energy. In brief, biophysics involves mainly diffusion processes, which are described by either transport or diffusion equations. The scale of diffusion is very small because cells are each about a micron in size. So, the mathematical apparatus for nuclear energy theory was analogous to the mathematical apparatus for biophysics; therefore, the transition was not very hard. It's just that a neutron travels about this distance, whereas a cellular component travels about an angstrom or so.

STOW: But the principles and mathematics are the same.

WEINBERG: The same.

STOW: So, the transition really was not a difficult one for you.

WEINBERG: True, although I knew nothing really about nuclear physics.

STOW: Who did know anything about nuclear physics?

WEINBERG: Well, Eugene Wigner and Enrico Fermi did.

STOW: Speaking of Fermi, you were not there on December 2nd in 1942 when [the world's first man-made nuclear chain reaction was sustained at the uranium pile on the squash court at Stagg Field at the University of Chicago].…

WEINBERG: That's because the admission badges were given out numerically and my number was too high. So, I missed being among the group who witnessed the very first chain reaction. But I went bowling with Wally Zinn and, I think, Fermi that evening went bowling. That was before the pile was critical and I was invited to see the pile the following day by Walter Zinn, who was Fermi's chief assistant and later director of Argonne National Laboratory.

STOW: When you visited the pile on December 3, did you have any concept of what was going to happen in the way of nuclear energy?

STOW: Oh yes, oh yes, oh yes, because we were much under the influence of Leo Szilard, who was the inventor in a way of the chain reaction. He had a patent taken out in 1933. He thought that the library would have a book called *Public Faces* by Harold Nicolson. This was a story of what will happen when the hydrogen bomb is invented. And so we realized that this was something very serious, but in recent months, I suppose, with the world in a situation as complicated as it is now, I sometimes ask myself, "Were we early pioneers all that correct in inventing the nuclear bomb?" And I describe that as a no-brainer because we knew that Hitler [authorized the development of an atomic bomb], and if it had to be done, then it was much better that the democracies do it than Hitler.

STOW: If we had not done it, somebody else would have.

WEINBERG: It would have been somebody else -- in particular, the Germans.

STOW: Speaking of Hitler and that program – you know we found out in 1945 that the Germans really did not have a nuclear bomb development program. Did that come as a surprise?

WEINBERG: No, that's not quite accurate. They had decided that the capability of Germany at that time during the war was just not up to what they realized had to be a large industrial project. I was one of the first people to read the captured German documents, where the reference to atomic bombs was rather slight. That was a question that people have reconsidered in view of all the fuss about Werner Heisenberg [head of the German atomic bomb project] and Niels Bohr [Danish physicist who worked on the Manhattan Project as a member of a British team] and their meeting in Denmark. And when you read the transcript of what Heisenberg said on the day after the bomb was dropped, you realize he was talking to his colleagues who had been incarcerated and farmed out.

STOW: In England.

WEINBERG: Yes. He simply had not thought enough about how to make a bomb to be able to give a plausible explanation until the following day when he had a chance to think it over.

STOW: Did you find it surprising that the Germans had not made more advances in nuclear technology?

WEINBERG: Yes and no. One of the reasons they didn't make more advances was that the people they had on the job [were not] the best. The thought that Heisenberg goofed in his first presentation on how a bomb works strongly suggests to me that he did not take it seriously. You asked why they didn't take it more seriously. Because the people they had were not driven the way Leo Szilard was driven, the way Eugene Wigner was driven, the way Edward Teller was driven, the way Ernest Lawrence was driven. And, they were better physicists in this instance.

STOW: But the German government had no organized effort to really….

WEINBERG: Well, that's not quite right. They certainly didn't have anything as centralized as the Manhattan Project, but they had Heisenberg's group. They had Bothe's group. They had one or two other groups. I believe there was too much fairly friendly competition between the groups. Maybe that explained why they didn't do it.

STOW: Well, at least in this country, we had a successful program.

WEINBERG: And, that's because there were people who believed that the whole thing was feasible. Wigner in his memoirs writes that when Szilard came to him in 1933 or '35, I guess, and suggested you could get nuclear energy by a chain reaction, Wigner said he thought long and hard about whether there was a law of nature, like the law of conservation, that prevents a chain reaction. He decided that there was no such law, and that it wasn't like building a perpetual motion machine.

STOW: Yes.

WEINBERG: Therefore, Wigner gave a talk at General Electric in 1938 or thereabouts, in which he said that within five years, we will figure out how to do nuclear energy. He had no basis, he said, for suggesting five years.

STOW: And, it was later in that same year of 1938 that fission was discovered.

WEINBERG: That's right. That's right.

STOW: Speaking of Wigner, when did you first meet him?

WEINBERG: I first met Wigner in either January or February of 1942 when he was in the process of moving his work at Princeton to Chicago. And, at that time I was working on a problem that Carl Eckert, who was my first advisor and boss on the Manhattan Project, had set for me. And so in the evening, I went up to his office in Eckert Hall in Chicago, and we discussed what I was doing, and I was immediately struck by what a powerful mathematician he was. I had known good mathematicians and Carl Eckert was an excellent mathematician, excellent theoretical physicist, and one of the great oceanographers, actually. But Wigner was, if it's possible, one step over.

STOW: Over Eckert.

WEINBERG: Over Eckert, but that doesn't diminish Eckert's contribution. He was a great man.

STOW: Put Eugene Wigner aside for a moment, Alvin. Other than Eugene Wigner, what other scientists or engineers had the greatest influence on you and your career?

WEINBERG: Well, Wigner certainly had great influence. Carl Eckert had great influence. In my biophysics days, which occupied about three or four years of my life, Professor Nicholas Rashevsky, the founder of what we called mathematical biophysics, had considerable influence on me.

STOW: When you came to Oak Ridge you had been here to visit, of course, during the early '40s, and then you moved here in, I think, May of 1945.

WEINBERG: That's right.

STOW: Did you have any concept at that point that you would spend your entire life and your career here in this God forsaken? …

WEINBERG: Well, vaguely, I had this idea. I realized that nuclear energy was going to be very important. I did have a choice to make right at the end of the war of either going back to Chicago and resuming my career as a biophysicist or staying in Oak Ridge. In this respect I was influenced by Eugene Wigner. And, Wigner, while he was still at Chicago, had outlined a plan for what became the Oak Ridge National Laboratory. Before that it was called Clinton Laboratories and was a pilot plant for the production of plutonium in an air-cooled natural uranium reactor [called the Graphite Reactor]. This pilot plant is still a national historic monument.

STOW: It is. After the war the Laboratory had the Oak Ridge School of Reactor Technology, or ORSORT, right?

WEINBERG: Right.

STOW: Can you explain how ORSORT got started?

WEINBERG: This was Wigner's idea. When he came down here to be the research director of the Laboratory in 1946 or 1947, he realized that the knowledge that had been amassed at Chicago on chain reactors really had to be spread among the engineering community in the United States. So, we established what we called the Oak Ridge School of Reactor Technology, or ORSORT, popularly known as the Clinch College of Nuclear Knowledge. Clinch is the name of the river town there. [The students at ORSORT] were many of the people who became leaders in nuclear energy, the most prominent of whom was none other than H G. Rickover, a Navy captain at that time. Admiral Rickover was later known as the father of the nuclear navy.

STOW: And what part did Rickover play in the decision to use pressurized-water reactors in ships and submarines?

WEINBERG: Well, at first he was skeptical. He had the General Electric Company at the same time working on a liquid-metal-cooled-reactor. He was skeptical [about my suggestion that he consider a pressurized-water reactor]. He said the thermal efficiency is too low if you base the [reactor coolant] on pressurized water. I used to call him Rick, so I said, "Rick, for goodness sakes, [thermal efficiency] is the last thing you want to worry about on a submarine. The main things you want to [have] are reliability, simplicity, and small size." A pressurized-water reactor [meets all those criteria].

STOW: What was Rickover like as a man?

WEINBERG: Terrible!

STOW: His reputation precedes him.

WEINBERG: He used rudeness as a very effective method [to get things done by the people working for him]. But he and I got along quite well. I think he had a good deal of respect for me. And in a way, I had great respect for him. Well, he certainly wasn't much of an engineer, really.

STOW: Well, he had quite a reputation, to say the least. Part of your career, especially the latter part, has been built upon communicating science to the public, to the layperson, and to politicians. Can you think back on how you got interested in this kind of communication?

WEINBERG: Well, I got involved, I guess, at the very beginning when younger scientists formed an association called at the time the Federation of Atomic Scientists. We'd go out and make speeches on what the new [nuclear] era was about. I went down to Louisiana, and we commuted from one small college to another and told everybody what was going on. And then I testified before the incipient U.S. Atomic Energy Commission. I guess that's how I got in the habit of talking to groups like that. I don't think that I'm primarily a public relations guy, but I kind of do that.

STOW: I didn't mean to imply that necessarily, but the Federation of Atomic Scientists was formed immediately after the war in '46 or thereabouts.

WEINBERG: Right.

STOW: As the creation of the Atomic Energy Commission was being debated.

WEINBERG: Right. The great success of the Federation was to crush the LeMay Johnson Bill, which would have retained the military as the arm of the government [responsible for the

development of] nuclear energy. Fortunately, Congress passed the McMahon Bill, which is the basic law of atomic energy now.

STOW: Let's jump to 1948. That was a tumultuous year in the history of Oak Ridge National Lab.

WEINBERG: Yes.

STOW: There were a couple things that went on. First, they couldn't find a Laboratory director for the longest time.

WEINBERG: That's right.

STOW: And secondly, a decision was made that all reactor work would go to Argonne.

WEINBERG: That's absolutely correct.

STOW: Reflect back on your thoughts and your feelings at that time.

WEINBERG: Well, with respect to the centralization of [research on] reactors, I just didn't believe that that was possible. The idea put forward by the general advisory committee, of which [J. Robert] Oppenheimer was the chairman, was that all reactor work would be concentrated at Argonne [National Laboratory near Chicago]. [The reactor experts and other researchers were] were supposed to leave here, and Oak Ridge was supposed to be the place where [staff] did unfashionable things like waste disposal, chemical processing, and so on. As I look back on that whole period, I ask myself, "Well it wouldn't have been so bad if Oak Ridge became primarily the waste-disposal site." But, wonderful ideas about different reactors were being bandied about here, and it was a goal that was too enticing to overlook. And so, I did all I could to keep Oak Ridge in the reactor business.

STOW: I think that was the same year that we had a Materials Test Reactor mock-up here.

WEINBERG: That's right.

STOW: You proposed that we install uranium fuel plates in the mock-up, and I think you got accused of trying to make sure that reactor technology was maintained at Oak Ridge.

WEINBERG: That's right. Well, I don't remember being accused of that, but in a way, that was something that did happen. And let me go back to the first part of your question about directorship. Eugene Wigner left Oak Ridge in 1947 to go back to Princeton, and I tried to persuade him that he really ought to commit to being the permanent director of the Oak Ridge National Laboratory, which was the new name of Clinton Laboratories by that time, and he just wouldn't. Very politely he said, "No." And then, there was a rush to find a new director, and I think they went through half a dozen names of rather prominent people. But nobody wanted to come down to Oak Ridge and throw their lot in with a bunch of young would-be scientists or new Ph.D.'s, because they didn't think that Oak Ridge had a future. Well, I had nothing to lose, really, when in desperation, they came to me and asked, "Wouldn't you like to be either the director or the research director?" And I said, "Well, why not, I'm young." I was only thirty at the time, but I wanted to take a crack at it. I decided to [choose the position of] research director because I thought, mistakenly, that I'd have time to do other things in addition to being the director. That's how I became research director at that time.

STOW: That was very young in your career as you point out. What were your feelings about transitioning from research to administration?

WEINBERG: Well, I never really felt that I was in administration wholly. I felt that I was supposed to inject -- I guess, this is a little grandiloquent -- whatever scientific and engineering instincts I had into what was going on at the Laboratory. And, that involved administration, to be sure. But, it also involved more than that. I guess a way to put it is that, when Wigner was research director for a year, he had a style. And, the style consisted of his becoming very acquainted with the details of what everybody was doing. I modeled myself after Wigner in this respect.

STOW: I think you developed a reputation for having taken the Laboratory from a nuclear laboratory to a very balanced laboratory.

WEINBERG: That came much later.

STOW: It came in the '60s, didn't it?

WEINBERG: Yes. That was when the center of gravity of reactor development shifted to the nuclear navy and the vendors --General Electric, Westinghouse, Babcock and Wilcox, and Combustion Engineering – that designed and built nuclear reactors.

STOW: You also developed a reputation of attending information meetings for the different research divisions and sitting on the front row…

WEINBERG: That's right.

STOW: And asking very penetrating questions.

WEINBERG: Well, I don't know if they were very penetrating. They did strike fear into some of the younger speakers. But that was my purpose, really -- to give people the feeling that what they did was important to the management, and they better be sure that what they did was done well.

STOW: As we've interviewed people it's been evident that there have been many research reactors built here at the Lab, starting with the Graphite Reactor and ending with the High Flux Isotope Reactor. There were more than a dozen reactors.

WEINBERG: Yes.

STOW: As you reflect back on all of these different reactors, is it accurate to say that one of them made the greatest contribution to technology?

WEINBERG: The greatest contribution to reactor technology really was the Materials Test Reactor.

STOW: MTR.

WEINBERG: The MTR's prototype – the Low Intensity Test Reactor -- and its subsequent plate-type, water-moderated reactors were the first water-moderated reactors. In a certain sense, they

anticipated the nuclear navy. In fact, the [reactor type in the submarines of the] nuclear navy is basically a pressurized version of the MTR.

STOW: What about our involvement with a reactor for nuclear-powered aircraft? How did we get involved in that?

WEINBERG: Well, nuclear-powered aircraft is really as good an oxymoron as I've ever heard of, because -- in order to have a manned airplane -- you had to pile on an awful lot of shielding. And, heavy shielding [makes it difficult for an airplane to get off the ground and stay in the air]. At that time, we were so hungry to do reactor work -- and this was an ultimate reactor -- that we gladly pitched in and did what we could. Now, the nuclear airplane was canceled by John F. Kennedy -- after a billion dollars was spent -- and those were the days when a billion dollars was real money. This project got me into very hot water because I wasn't sufficiently enthusiastic about the aim of the Air Force to make a nuclear airplane. But, as I think back on it, the reputation of the Oak Ridge National Laboratory in high-temperature materials really stemmed from and grew out of the interest in the nuclear airplane [and the work we did on the project].

STOW: You anticipated my next questions. I was going to say the U.S. never did create a nuclear-powered aircraft, but the Laboratory benefited considerably from that approach.

WEINBERG: And, the science and technology of metallurgy and high-temperature materials profited greatly from the Aircraft Nuclear Propulsion project.

STOW: It did. You mentioned President Kennedy. You got involved with the Eisenhower administration also with regard to desalination of seawater?

WEINBERG: This was not while Eisenhower was president. This was after his presidency.

STOW: Oh, I see.

WEINBERG: The general idea was that after the Six-Day War in the Middle East, many people had this idea: "Gee, [scarcity of] water is really what's at the root of the [Israeli-Arab] problem. And so, if we can create technical ways of making more water, then that will have a soothing effect on the politics. And so, Louis Strauss, who was the first chairman of the Atomic Energy Commission and a confidant of Ike, proposed that we build nuclear reactors in the Gaza Strip and the West Bank [of Israel] and use them for desalting water [from the ocean to produce drinking water and water suitable for irrigating crops]. And Howard Baker took up this proposal. He was then a young senator in the Senate, and he had the Senate pass a resolution promoting nuclear desalination in the Middle East. Actually, the State Department was enjoined by Congress to support such an effort. We did have a considerable effort at the Oak Ridge National Laboratory in those days on what we called the agri-industrial complexes for the Middle East. We were the only place in the world at the time where Israeli engineers and Egyptian engineers worked together.

STOW: So, you actually advised Eisenhower after his tenure was passed.

WEINBERG: Well, not really Eisenhower.

STOW: Not directly, but…

WEINBERG: It was Louis Strauss and Howard Baker whom I advised. And, Ike was very taken by it, but the economics wasn't there at the time.

STOW: You've had the honor in your career of actually advising U.S. Presidents, such as Kennedy, Lyndon B. Johnson, and Richard Nixon, on several occasions.

WEINBERG: I was on the President's Science Advisory Committee during the Kennedy and Johnson administrations.

STOW: Well, how about during Nixon's terms in office?

WEINBERG: Then I had a role during the Nixon administration. After Nixon fired all of his science advisors, he asked me to come to Washington. Well, it was actually Bill Simon who asked me to come to Washington to head a small office in the White House that would serve as a kind of substitute science advisor. I did that for about a year.

STOW: That was in 1974, I think.
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WEINBERG: That's right. [It was after I left the Lab.]

STOW: And, [the White House called it] the Office of Energy R&D, as I recall.

WEINBERG: That's right.

STOW: You describe in your book that 1974 was the worst year of your life.

WEINBERG: Well, I was lonesome.

STOW: Your wife had died.

WEINBERG: Yes, she had died a little while before. I just couldn't mesh with the Washington milieu very well.

STOW: Was there anything you learned that year from dealing with the challenges that helped you deal with issues later on in your career?

WEINBERG: Well, I guess the main thing I got to understand was that in Washington everything is politics.

STOW: Yes.

WEINBERG: And, it's not so much what you know, as who you know. I guess I'd have to say that.

STOW: I guess that's been true all along and will continue to be.

WEINBERG: Well, that's right.

STOW: 1973 was your last year as director of the Laboratory.

WEINBERG: That's right.

STOW: Can you describe to us the circumstances that led to your leaving the directorship?

WEINBERG: Well, my ideas about nuclear energy were orthogonal, I guess I'd say, to the mainstream of ideas. This meant that I was at odds with the people in the AEC Reactor Division, particularly the division director, Milt Shaw.

STOW: Milton Shaw.

WEINBERG: But there were others, too, such as the general manager. I think they just didn't think that I was doing what they thought I should be doing. And so, my very good friend, John Swartout, who was my deputy director before he left to become a vice president of Union Carbide, took me aside one day and said, "Alvin, your time is up." I had realized that this was going to happen, so it wasn't all that big a deal for me. And sometimes I think my best work, certainly my best philosophical insights, have been concocted after I left the Laboratory.

STOW: At the Institute for Energy Analysis at Oak Ridge Associated Universities?

WEINBERG: Yes, that's right. The Institute for Energy Analysis was an idea of Bill Baker, who was sort of the ray of eminence of science in the Nixon administration. He said, "Alvin, we need a think tank devoted to energy and how about you doing it?" And, that's what happened.

STOW: You founded or set up the Institute for Energy Analysis at ORAU?

WEINBERG: That's right.

STOW: And had contacted a number of different national laboratories.

WEINBERG: That's right. And, I was invited to go to several of them, but I decided that I liked Oak Ridge and I liked ORAU.

STOW: But you never got an invitation from ORNL to house the IEA, did you?

WEINBERG: No, I don't think so. But I can understand that. Also, had I stayed at ORNL then there would be a question of divided loyalties on the part of the people.

STOW: Let's look back for a second briefly at the 1960s. That was a time when we felt that nuclear energy was really on the forefront of the future.

WEINBERG: That's right.

STOW: And we grossly underestimated the cost of nuclear energy.

WEINBERG: That's correct.

STOW: Why did we so grossly underestimate it?

WEINBERG: Because we were young and we were optimistic, and GE and Westinghouse misled us by offering lost leaders. They were lost leaders.

STOW: Yes.

WEINBERG: They quoted their pressurized and boiling water reactors at about one hundred dollars per kilowatt on a fixed price. And this was very much a lost leader because the price of the

reactor gradually escalated until it finally reached something like five thousand dollars per kilowatt, for example.

STOW: You coined the term “big science.”

WEINBERG: Right.

STOW: And “technological fix.”

WEINBERG: Right.

STOW: Can you explain how those terms came to be?

WEINBERG: Well, “big science” came to be because I wrote an article a long time ago that was published in *Science* magazine. I called the article, “The Impact of Large-Scale Research on Science,” and I had to have a name for large-scale research. So I decided to call it “big science,” and that’s how this term came about. And, it caught on. As far as “technological fix” is concerned, I am first and foremost a technologist, because I was intrigued by the idea illustrated by the Lab’s effort on desalting -- that a technological advance can have big social consequences. And so, I coined the word “technological fix.” It turned out later that I learned that the idea of the technological fix was not original with Alvin Weinberg. There was a fellow by the name of Dick Meyer, a professor of everything at University of California, who had written a book about this.

STOW: You also coined the term “Faustian bargain.”

WEINBERG: Well, that term simply came about when I wrote an article titled, “Social Implications of Nuclear Energy,” in which I stated that nuclear energy is special, as we see now. In my articles on nuclear proliferation, waste disposal, and radioactivity, I always quoted Enrico Fermi, who in 1944 told us, “Look, this is a great new energy source that we’re creating, but there is no a priori reason to believe that the public will accept a new energy source that’s encumbered by huge amounts of radioactivity and that has a possibility of proliferation” [spreading hazardous nuclear material to terrorist groups and nations]. I will never forget that. In a certain sense, if I examine what my career has been all about, it’s been to prove that Fermi was unduly pessimistic because, as you know, nuclear energy does account for something like 20 percent of the world’s electricity now.

STOW: True.

WEINBERG: And, that’s pretty good. I realized further -- and this is turning out to be a very important element in the whole picture, but one that I will not be around to enjoy -- that nuclear reactors in the United States are turning out to last much longer than they were supposed to.

STOW: That’s true.

WEINBERG: When I was born, my life expectancy was about sixty-odd years.

STOW: Yes.

WEINBERG: I will be in my eighty-eighth year, and that means I’ve outlived my life expectancy. In the same way, the nuclear reactors in the United States are turning out at least to be outliving their licensing period, and so you then ask, “Well, when will that stop?” Chairman Richard

Meserve of the Nuclear Regulatory Commission estimates that all the reactors in the United States -- that's about 100 reactors -- will apply for re-licensing in twenty more years. Then you ask what's going to happen at the end of those twenty years? Why won't they just continue to be re-licensed, re-licensed, re-licensed, and, if you have a reactor that has already been paid for, then the power from the reactor will, in fact, be very, very cheap.

STOW: It will already be at that point.

WEINBERG: That's right. So, the old ideas voiced by Louis Strauss that nuclear energy will be too cheap to meter could actually happen in the next generation or the generation after that. I wrote a paper, which I believe, is as important a paper as I've ever written titled, "Immortal Reactors and Intergenerational Justice." The idea was that some philosophers had gotten together and asked, "What right does this generation have to force on future generations for all time the disadvantages of nuclear energy?"
And, what I wrote is that they earn that right, because in the far future -- twenty, forty, sixty, eighty, one-hundred years from now -- nuclear energy clearly will be the cheapest energy there is. In this way, we compensate the future generations for their promise to be careful.

STOW: What about solar energy? What role do you think it will play?

WEINBERG: It will play a marginal role, unless society changes very significantly. Although, I guess in a way, one of the things I did while I was in Washington was to propose that the government create a national laboratory devoted to solar energy.

STOW: SERI.

WEINBERG: It was called the Solar Energy Research Institute, or SERI, yes.

STOW: And now it's known as the National Renewable Energy Laboratory, or NREL, right?

WEINBERG: Yes.

STOW: As you look back over your career, Alvin, can you put your finger on a single accomplishment that you are most proud of – something that you've been involved with or have spearheaded? There are so many there that I wouldn't know where to start.

WEINBERG: Well, I suppose the contributions I made to reactor science and technology would be my main technical achievement. That research contribution, of course, is summarized in the book Eugene Wigner and I wrote titled *The Physical Theory of Neutron Chain Reactors*. This book, for a long time, was the standard work on the theory of neutron chain reactions. One or two copies a year are still being sold because the book was written before big computers became available.

STOW: And now everything's computerized.

WEINBERG: Yes, everything's computerized. But that's not what I, in my own mind, consider to be the most original thing that I ever did. It was rather obvious. The most original thing that I did was to point out that the philosophy of science is deficient in that it is concerned primarily with epistemology, which is how do we know that something is true, and with logic. But ethics, in the fundamental sense, is not covered in the philosophy of science. And yet, there is a need for a

value system within science that is sometimes called the philosophy, not of science, but of scientific administration.

STOW: Administration.

WEINBERG: The basic problem that the administrator has at every level is not simply to accomplish the science that he's doing but also to decide which science to do. And, I had proposed, at a suggestion of my assistant Eugene Guth, that Jon von Neumann's aphorism about mathematics is important to the extent that a given mathematical enterprise involves many of the fields adjacent to it. I decided that that same principle was relevant as far as empirical science was concerned. Therefore, I wrote a series of papers and sort of started a cottage industry on what I called criteria of scientific choice. And, I established a set of criteria. I won't go into them now. But, I was very pleased that these criteria, which [dealt with] purely philosophic questions, were taken over by the National Science Foundation. And so, when the National Science Foundation asked a scientist to justify his request for funds, they asked him a series of questions that were modeled more or less after my "criteria of scientific choice."

STOW: Using your criteria.

WEINBERG: Yes, that's what I really am proud of.

STOW: And, this came along in later years while you were with IEA and even afterward. Is that correct?

WEINBERG: No, it was actually before then in 1967. I was still director of the Laboratory. I published a little book called *Reflections on Big Science.*

STOW: This work that you did on the criteria for science is one of your shining points as you look back on your career. What's your greatest regret? What would you like to have accomplished that you didn't?

WEINBERG: Well, I'd like to have been a great scientist -- something I'm not. It's just that I don't have the right genes to be a great scientist, in the sense that Wigner was a great scientist.

STOW: Few people are.

WEINBERG: Well, that's right, but that doesn't mean that we don't aspire to be that sort of person.

STOW: Now, we're in the year 2003, and the Laboratory has been here for 60 years.

WEINBERG: Yes, I'm proud of that.

STOW: Well, in the year 2043, there's going to be somebody interviewing people about Oak Ridge National Laboratory, and your name's going to come up as a director of the Laboratory. How do you want to be remembered 40 years hence in the hundredth anniversary of the Laboratory?

WEINBERG: Well, I want to be remembered as a person who felt that the national laboratory – a new invention -- had a place in the scientific firmament and that keeping people honest and on their toes in a big research enterprise is something that a director of a laboratory always has to be aware of and must do.

STOW: Okay, thank you very much.

WEINBERG: Well, my time with you is somewhat foreshortened, but I tried to talk fast.

STOW: Appreciate that. We touched on a lot of important things .

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