

9531

Smithsonian Videohistory Program

Manhattan Project

Session Four

Collection Division 2: Oak Ridge

Stanley Goldberg, Interviewer

March 3, 1987

Babcock, Dale F.

SMITHSONIAN VIDEOHISTORY PROGRAM

Release of Interview Material

HOCKESSIN

In interest and consideration of the increase and diffusion of knowledge to which the Smithsonian Institution is committed I, Dale F. Babcock, of Hockessin, Del., hereby donate to the Smithsonian Institution any and all copyright and any other rights, title, and interest that might exist or I may have in the interview(s) granted by me to the Smithsonian Institution on the following date(s):

March 3, 1987

At the same time, I also hereby transfer and donate to the Smithsonian Institution any and all rights, title, and interest in and to any and all physical properties, including but not limited to videotapes, audiotapes, and transcripts, that fix the above-referenced interview in tangible form.

The information disclosed by me will be made available without restriction for research in accordance with the general procedures of the Smithsonian Institution Archives.

Dec 16-1987
Date

Dale F. Babcock
Signature

Manhattan Project: S-4

Borst, Lyle

SMITHSONIAN VIDEOHISTORY PROGRAM

Release of Interview Material

In interest and consideration of the increase and diffusion of knowledge to which the Smithsonian Institution is committed I, Lyle Borst, of Williamsville, N.Y., hereby donate to the Smithsonian Institution any and all copyright and any other rights, title, and interest that might exist or I may have in the interview(s) granted by me to the Smithsonian Institution on the following date(s):

March 3, 1987

At the same time, I also hereby transfer and donate to the Smithsonian Institution any and all rights, title, and interest in and to any and all physical properties, including but not limited to videotapes, audiotapes, and transcripts, that fix the above-referenced interview in tangible form.

The information disclosed by me will be made available without restriction for research in accordance with the general procedures of the Smithsonian Institution Archives.

December 16, 1987
Date

Lyle B. Borst
Signature

Manhattan Project: S-4

SMITHSONIAN VIDEOHISTORY PROGRAM

Release of Interview Material

In interest and consideration of the increase and diffusion of knowledge to which the Smithsonian Institution is committed I, Edward Creutz, of Rancho Santa Fe, Cal., hereby donate to the Smithsonian Institution any and all copyright and any other rights, title, and interest that might exist or I may have in the interview(s) granted by me to the Smithsonian Institution on the following date(s):

March 3, 1987

At the same time, I also hereby transfer and donate to the Smithsonian Institution any and all rights, title, and interest in and to any and all physical properties, including but not limited to videotapes, audiotapes, and transcripts, that fix the above-referenced interview in tangible form.

The information disclosed by me will be made available without restriction for research in accordance with the general procedures of the Smithsonian Institution Archives.

12/17/84

Date

Edward Creutz

Signature

Wattenberg, Albert

SMITHSONIAN VIDEOHISTORY PROGRAM

Release of Interview Material

In interest and consideration of the increase and diffusion of knowledge to which the Smithsonian Institution is committed I, Albert Wattenberg, of Urbana, Ill., hereby donate to the Smithsonian Institution any and all copyright and any other rights, title, and interest that might exist or I may have in the interview(s) granted by me to the Smithsonian Institution on the following date(s):

March 3, 1987

At the same time, I also hereby transfer and donate to the Smithsonian Institution any and all rights, title, and interest in and to any and all physical properties, including but not limited to videotapes, audiotapes, and transcripts, that fix the above-referenced interview in tangible form.

The information disclosed by me will be made available without restriction for research in accordance with the general procedures of the Smithsonian Institution Archives.

January 7, 1988
Date

Albert Wattenberg
Signature

Manhattan Project: S-4

Wigner, Eugene P.

SMITHSONIAN VIDEOHISTORY PROGRAM

Release of Interview Material

In interest and consideration of the increase and diffusion of knowledge to which the Smithsonian Institution is committed I, Eugene P. Wigner, of Princeton, N.J., hereby donate to the Smithsonian Institution any and all copyright and any other rights, title, and interest that might exist or I may have in the interview(s) granted by me to the Smithsonian Institution on the following date(s):

March 3, 1987

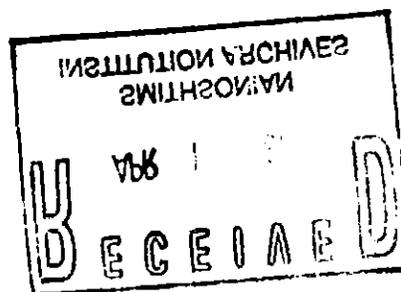
At the same time, I also hereby transfer and donate to the Smithsonian Institution any and all rights, title, and interest in and to any and all physical properties, including but not limited to videotapes, audiotapes, and transcripts, that fix the above-referenced interview in tangible form.

The information disclosed by me will be made available without restriction for research in accordance with the general procedures of the Smithsonian Institution Archives.

March 25, 1988
Date

Eugene P. Wigner
Signature

Manhattan Project: S-4



9531

Smithsonian Videohistory Program

Manhattan Project

FINDING AID

Sessions 1 - 18

Collection Divisions: 1 - 5

1987 - 1990

The Manhattan Project

The United States government began underwriting investigations of the feasibility of atomic weapons in October 1941. Within a year, promising research at several universities, particularly at the Metallurgical Laboratory of the University of Chicago, showed that it was possible to produce atomic bombs based on the chain-reacting fission of uranium 235 isotope or of plutonium. This led to the reorganization of the Manhattan District, or "Project," of the U.S. Army Corps of Engineers to make these bombs a reality. Brigadier General Leslie R. Groves directed and coordinated the Project from 1942 to 1945, spending 2.3 billion dollars on nuclear reactors and chemical separation plants at Hanford, Washington, and Oak Ridge, Tennessee, and on the weapon research and design laboratory at Los Alamos, New Mexico. The first plutonium bomb was successfully detonated at Alamogordo, New Mexico, on July 16, 1945. The B-29 bomber "Enola Gay" exploded the first uranium bomb, "Little Boy," over Hiroshima, Japan, on August 6, 1945; the B-29 "Bock's Car" exploded the second plutonium bomb, "Fat Man," over Nagasaki, Japan, two days later.

Stanley Goldberg, consulting historian for the National Museum of American History, recorded eighteen video sessions with fifty-five participants involved in the engineering, physics, and culmination of the Manhattan Project. Goldberg examined the research and technologies necessary to realize the uranium and plutonium bombs. He supplemented interviews with visual documentation of the industrial plants that refined and separated the isotopes, and of the machinery that delivered and dropped the bombs. Interviewees explained the other steps of designing, building, testing and detonating an atomic bomb. Discussions with participants also elicited a social history of the Project as recalled by men and women with different duties in different locales. Between January 1987 and June 1990 the sessions were recorded on-site or in-studio in Hanford, Washington; Boston, Massachusetts; Oak Ridge and Louisville, Tennessee; Alamogordo and Los Alamos, New Mexico; Washington, D.C.; and Suitland, Maryland. They are divided into five collection divisions described further below: Hanford, Oak Ridge, Cambridge, Los Alamos, and Alberta.

Collection Division 1 contains Sessions One through Three. Goldberg interviewed selected employees, engineers, and administrators responsible for the construction and operation of the nuclear reactors and chemical separation plants at the Hanford Engineering Works in the state of Washington, commonly referred to as the Hanford Reservation. Participants discussed the mechanics of the reactors, separation plants, and cooling systems; the living and working conditions; and the administrative tasks involved in producing plutonium. Session One took place at a reactor face and control room at the Hanford Reservation and featured extensive visual documentation of the instrumentation. Sessions

Two and Three were shot at a Columbia Cable Television studio in Kennewick, Washington.

Collection Division 2 contains Sessions Four through Eight. Goldberg continued to focus on nuclear engineering as it developed from Enrico Fermi's experimental reactor to the pilot reactor and the Clinton Engineer Works at Oak Ridge, Tennessee. Participants included physicists, engineers and plant operators. Their discussions covered the development of fission theory; the application of theory to reactor construction; living and working conditions; and the technologies used at the Y-12 Electromagnetic Separation Plant and the K-25 Gaseous Diffusion Plant for the separation of uranium isotopes. Sessions Four through Six took place at the Kennedy Maxwell Productions studio in Louisville, Tennessee. Visual documentation included diagrams of calutron components and photographs of Oak Ridge and Clinton under construction. Interviewees in Sessions Seven illustrated their explanations at the instrument faces of the K-25 Plant. Session Eight documented visually the interiors of the K-25 and Y-12 Plants.

Collection Division 3 contains Sessions Nine through Twelve. Goldberg interviewed physicists and their spouses who helped design the bombs at the Los Alamos Scientific Laboratory in New Mexico. Participants recalled the formation, organization, and research activities of the Laboratory, as well as the social and cultural life of the community. The plutonium implosion program; preparation for and execution of the Trinity test; the explosion over Hiroshima; and reactions of the Project physicists in the aftermath of the bomb are also discussed. The interviews were taped at the studios of the Audvid Film and Tape Production Company in Boston, Massachusetts, but are labeled the "Cambridge" sessions.

Collection Division 4 contains Sessions Thirteen through Sixteen. The participants covered the topics of the Cambridge sessions from different personal perspectives. Goldberg interviewed eight physicists in New Mexico who contributed to the design and testing of the atomic bomb. Participants in Session Thirteen visited various locations at the Trinity test site near Alamogordo. Sessions Fourteen and Fifteen took place at the Fuller Lodge in Los Alamos. Session Sixteen documented the landscape of Los Alamos in 1945 and 1988.

Collection Division 5 contains Sessions Seventeen and Eighteen. Four participants reviewed their roles in Project Alberta, the conversion of the atomic bomb from a test device to a deliverable weapon system. The two scientists and two military officers recalled design restrictions, flight training, and the missions themselves. Session Seventeen took place by a Fat Man plutonium bomb casing and in a conference room at the National Museum of American History; Session Eighteen was recorded in and around the Enola Gay at the Paul E. Garber Facility in Suitland, Maryland, where the plane was undergoing restoration.

These sessions were recorded on three different videotape formats, and the original tapes are reserved for the archives. VHS copies and full time-

coded transcripts with abstracts of important visual information are available for research use. Dubbing masters may be duplicated for presentation and exhibition. Fees are charged for copies.

Collection Division 1: Hanford

Interviewees in this collection contributed in various roles to the refinement of plutonium 239 isotope at the Hanford Engineer Works in the state of Washington. In January of 1943, General Groves chose the site for construction of three full-scale plutonium piles for the mass production of plutonium 239--an isotope for the chain reaction in an atomic bomb--as well as water-treatment plants for cooling the reactors. The E.I. Du Pont de Nemours Company also built four remote-controlled "canyons" for the chemical separation of plutonium from uranium 238. Sessions were shot at the Columbia Cable Television studio and on-site at the Hanford Reservation.

Participants for Session One assisted in operations at the "B" site nuclear reactor as operators or support personnel. **Lawrence Denton** began work at the Hanford construction camp in September 1942 as a receiving and shipping Clerk. **Wilson A. Cease** came to Hanford as a Du Pont employee in March 1944, and worked as a security patrolman in the area where uranium slugs were canned and sealed. **Jess R. Brinkerhoff** and **Ralph K. Wahlen** were both employed by the Remington Arms plant in Salt Lake City, Utah, and transferred to Hanford. Brinkerhoff arrived in November 1943, and worked in the fire department before becoming a power operator in a water treatment plant. Wahlen was employed in the fuel piece canning area. **R.M. Buslach** arrived in Hanford after the war and worked in plant maintenance for the General Electric Company.

Session Two participants worked for the Du Pont Company as chemical engineers at Hanford. **Wakefield A. Wright** and **Vivian Russell Chapman** were first transferred from Alabama Ordnance Works by Du Pont to the Manhattan Project facilities at Oak Ridge, Tennessee, for training before arriving in Hanford in 1944. **William P. McCue** was employed at the Oklahoma Ordnance Works before training at the Argonne National Laboratory in Chicago, Illinois, and relocating to Hanford. The responsibilities of the three men at Hanford included training the crews and supervising the operators in the nuclear reactors and chemical separation plants.

Session Three brought together a group of Hanford administrators. **Oswald H. Greager** had been a chemist for Du Pont after receiving his Ph.D. in that field from the University of Michigan in 1929. He came to Hanford in October 1944, from the Separations Development Division at the Clinton Engineer Works in Oak Ridge, Tennessee. Greager, on military duty at Hanford, served as Technical Officer and supervised the work of the contractor in the chemical separations area. **Richard T. Foster** joined the project in September 1943, on a contract with the Office of Scientific Research and Development at the University of Washington College of

Fisheries. He studied the effects of radiation on the Columbia River and eventually became concerned with evaluating radiological doses received by people from all environs at Hanford. **Leonard F. Perkins, Sr.**, came to Hanford in the spring of 1944 as an employee of the United States General Accounting Office to audit the contract of the Du Pont Company. In 1946, he transferred to the Atomic Energy Commission and returned to Hanford in 1951 to direct government-contracted construction there until 1973. During World War II **Frederic W. Albaugh** worked in the Metallurgical Laboratory at the University of Chicago as a group leader in the plutonium chemistry section. He arrived in Hanford to head its plutonium chemistry section in 1947 and continued to work there in various administrative capacities until 1971. Colonel **Franklin T. Matthias**, who had worked under General Groves in construction contracting for the Pentagon, was largely responsible for the site selection of Hanford. Groves appointed Matthias in February of 1943 to be commanding officer of the Hanford facilities.

The discussions detailed the nature of the workload at Hanford, the living conditions, and the administration of the Project. The sessions were shot on three-quarter-inch U-Matic tape and provided visual documentation of the "B" site nuclear reactor, tools used for the charge/discharge process, and period photographs of the interiors of the chemical separation "canyons."

Box 1 Transcripts of Sessions

Session One (January 13, 1987), at the Hanford Reservation "B" site nuclear reactor face and in the reactor control room, featured Brinkerhoff, Buslach, Cease, Denton, and Wahlen on the operation of the first large-scale reactor, c. 1942-1945, including:

- discussion and demonstration of procedures for making the reactor operational;
- canning of uranium slugs and the charge\discharge process of the reactor;
- cooling system operations and general reactor maintenance;
- methods for controlling the reactor's power levels and operating the various safety systems.

Visual documentation included:

- period photographs of Hanford Reservation under construction;
- "B" reactor face;
- reactor control room and instrument boards;
- specialized tools developed for canning uranium slugs.

Session Two (January 13, 1987) at the Columbia Cable Television station, Kennewick, Washington, featured Chapman, McCue, and Wright on their roles in producing plutonium at Hanford, c. 1942-1945, including:

- how they came to Hanford;
- conditions and operating procedures in the chemical separation "canyons";
- charge/discharge procedures for the reactor;
- development of specialized equipment;
- accidents and problems involving radioactive material;
- living conditions during and after construction.

Visual documentation included:

- period photographs of Hanford reactors and canyons under construction.

Pages 1 - 56

Session Three (January 14, 1987), at the Columbia Cable Television station featured Albaugh, Foster, Greagher, Matthias, and Perkins on administration at Hanford, c. 1942-1945, including:

- selection of site;
- staffing and conditions during the construction period;
- Manhattan District accounting and purchasing procedures;
- measures to abate or control potential radioactive hazards;
- efforts to ascertain impact of Hanford operations on the Columbia River and local fisheries.

Pages 1 - 39

Videotapes of Sessions

Session One - Tape One - January 14, 1987 - 120 minutes

Session Two - Tape One - January 14, 1987 - 120 minutes

Session Three - Tape One - January 14, 1987 - 105 minutes

Collection Division 2: Oak Ridge

Interviewees in this collection contributed in various roles to the refinement of uranium 235 isotope at the Clinton Engineer Works in Oak Ridge, Tennessee. Built concurrently with the Hanford Reservation, the Clinton complex was designed for continued research and the refinement of the fissionable isotope uranium 235 from uranium 238. The sessions were

shot at a studio of Kennedy Maxwell Productions, and on-site at the Y-12 Electromagnetic Separation Plant and the K-25 Gaseous Diffusion Plant.

Participants for Session Four were instrumental in designing and running the nuclear reactors at Oak Ridge and Hanford. **Dale F. Babcock** received his Ph.D. in physical chemistry at the age of twenty-three from the University of Illinois in 1929. Du Pont employed his services as a research chemist until 1942, when he became a technical specialist on the explosive potential of plutonium. Before the war, **Lyle F. Borst** stayed at the University of Chicago as a Research Associate after his doctoral studies in physics. In 1943 he was appointed chief physicist of the Clinton Laboratories near Oak Ridge, where he remained until 1946. **Edward C. Creutz** received his Ph.D. from the University of Wisconsin in 1939, taught physics at Princeton University, and joined the Manhattan Project as a group leader between 1942 and 1946. After he received his M.A. from Columbia University in 1939 **Albert Wattenberg** was a spectroscopist for Schenley Products, Incorporated. He spent a year with the Office of Scientific Research and Development at Columbia University before moving to the Metallurgical Laboratory in Chicago as a group leader under Enrico Fermi in January 1942. **Alvin M. Weinberg**, born in 1915, earned his three degrees in physics at Chicago by 1939. He stayed there during the war at the Metallurgical Laboratory and moved to Oak Ridge in 1945. **Eugene P. Wigner** was born in Hungary in 1902. He earned his doctorate in physics at the Technical University of Berlin in 1925 and came to the United States at the behest of Princeton five years later. Together with Leo Szilard he played a key role in sparking President Franklin D. Roosevelt's interest in atomic power and during the war he designed the Hanford reactors. Wigner won the Nobel Prize for physics in 1963.

The four participants of Session Five helped operate the isotope separation machinery designed by the physicists and engineers. **Colleen Black** was nineteen years old when she arrived in July 1944 and was assigned to find pipe leaks at the K-25 Plant. The other three worked in the Y-12 Plant. **Connie Bolling** was teaching at Coburn High School, Virginia, in 1943 when he gave six weeks' notice to join a government project that he understood would end the war. He trained cubicle and vacuum pump operators and remained after 1945 in the effort to maximize calutron output. **Jane W. Larson** arrived in September 1943, as a historian before switching to technical editor, reporting on the effort to maintain vacuum consistency. She also worked part-time for the Oak Ridge Journal. **Audrey B. Livingston**, born in 1926, started in 1944 as a cubicle operator.

Participants for Session Six helped design, build and operate the calutrons in the Y-12 Plant. **George M. Banic, Jr.**, worked on high voltage power supplies for the General Electric Company in Schenectady, New York, and came to Oak Ridge in March 1944, to help with the stable isotope program. He stayed after the war to continue isolating isotopes at the pilot plant until it closed in 1975. **Clarence E. Larson** and **Robert S. Livingston** received their Ph.D.'s from the University of California at Berkeley and continued their research at the Radiation Laboratory there until 1943. Larson then took charge of the technical staff at the Y-12 plant at Oak Ridge through

1950 when he became director of the Oak Ridge National Laboratories. Livingston oversaw Stone and Webster Engineering Corporation's design of the Y-12 Plant and continued working at Oak Ridge until his retirement in 1981. **John M. Googin** sought out a position in nuclear chemistry while finishing his B.S. at Bates College in Maine. He started work at Oak Ridge as a process chemist in the summer of 1944, assisting in the recycling of uranium waste. **Chris P. Keim** received his Ph.D. in chemistry from the University of Nebraska in 1940. In 1944 he left his fellowship at the Mellon Institute to become a research physicist for the stable isotope program at the pilot plant. Keim continued working at Oak Ridge until his retirement in 1971.

Session Seven's participants helped design and operate the K-25 Plant for the gaseous diffusion of uranium 235. **Paul R. Vanstrum** and **James A. Parsons** majored in chemical engineering at Columbia University where they participated in the manufacture of part of the diffusion barrier. Vanstrum began working for Union Carbide Corporation, the K-25 operating contractor, after graduating and transferred to Oak Ridge in August 1944. He stayed at the K-25 Plant until it closed in 1964. Parsons continued to work on the manufacture of diffusion barriers in New York until September 1944, when he went to Oak Ridge as a foreman. **Paul Huber** also had a degree in chemical engineering and began work at Oak Ridge in 1944.

Goldberg focused discussions on the theory and practice of reactor construction; nature of the workload; living conditions; and security measures at Oak Ridge. The sessions were shot on one-inch tape and provided visual documentation of the Y-12 and K-25 plants, calutron components, and period photographs of Oak Ridge.

Box 2 Transcripts of Sessions

Session Four (March 3, 1987), at the Kennedy Maxwell Productions studio, Louisville, Tennessee, featured Babcock, Borst, Creutz, Wattenberg, Weinberg, and Wigner on the conversion of fission theory to the construction of nuclear reactors, c. 1939-1944, including:

- pre-war fission research;
- Albert Einstein's letter to President Franklin D. Roosevelt;
- the Chicago pile or reactor (CP-1);
- problems with canning uranium slugs for reactors at Oak Ridge and Hanford;
- conflicts between university-based physicists and engineers employed by Du Pont;
- xenon poisoning of early reactors;
- impact of security measures on research;
- acquisition of raw materials;
- motivation of physicists by German progress in fission research.

Visual documentation included:

- painting of first successful testing of Chicago pile;
- period photographs of X-10 reactor at Oak Ridge.

Pages 1 - 138

Session Five (March 3, 1987), at the Kennedy Maxwell Productions studio featured Black, Bolling, Larson, and Livingston on the plant operators' lives and work at Oak Ridge, c. 1943-1949, including:

- living conditions during and after construction;
- layout of Oak Ridge and Clinton Engineer Works;
- job training at Y-12 and K-25 Plants;
- racial segregation;
- knowledge of goal of the Manhattan Project;
- security measures;
- reactions to news of Hiroshima bombing.

Visual documentation included:

- period photographs of Oak Ridge and Clinton Engineer Works.

Pages 1 - 74

Session Six (March 4, 1987), at the Kennedy Maxwell Productions studio featured Banic, Larson, Googin, Keim, and Livingston on the development of calutrons for uranium separation, c. 1943-1945, including:

- early designs at the University of California at Berkeley;
- improvement of calutron equipment;
- obtaining and returning silver from the U.S. Treasury Department;
- training of operators;
- the magnet short-circuit crisis;
- chemistry of recycling uranium waste;
- security measures and their effect on research;
- shipping uranium 235 to Los Alamos;
- safety precautions;
- reactions to Hiroshima bombing and the Smyth Report.

Visual documentation included:

- schematic drawings and period photographs of the Y-12 Plant;
- models of calutron components.

Pages 1 - 111

Session Seven (March 5, 1987), at the K-25 Gaseous Diffusion Plant, Oak Ridge, Tennessee, featured Huber, Parsons, and Vanstrum on the design and operation of the first gaseous diffusion plant, the K-25, c. 1943-1945, including:

- principles of the diffusion process;
- manufacture of pumps, diffusers, and seals;
- design and construction of K-25 building;
- working conditions;
- training of operators;
- operation of K-25 from control room;
- operation of a diffusion cell;
- safety precautions.

Visual documentation included:

- schematic drawing of diffusion process;
- period photographs of K-25 equipment and controls.

Pages 1 - 50

Session Eight (March 6, 1987), at the K-25 Gaseous Diffusion Plant and the Y-12 Electromagnetic Separation Plant, Oak Ridge, Tennessee, consisted of approximately thirty minutes of visual documentation of interiors, including:

- half-mile length of one wing of the K-25 Plant;
- explanation by Goldberg of construction of K-25;
- K-25 pipe gallery and catwalk;
- exterior and interior views of Y-12 calutron units;
- exterior and interior views of Y-12 control boards during operation.

There is no time code for the VHS tape of this session.

Page 1

Videotapes of Sessions

Session Four - Tape One - March 3, 1987 - 120 minutes

Session Four - Tape Two - March 3, 1987 - 120 minutes

Session Five - Tape One - March 3, 1987 - 120 minutes

Session Six - Tape One - March 4, 1987 - 120 minutes

Session Six - Tape Two - March 4, 1987 - 120 minutes

Session Seven - Tape One - March 5, 1987 - 105 minutes

Session Eight - Tape One - March 6, 1987 - 30 minutes

Collection Division 3: Cambridge

Interviewees in this collection worked on the physics of atomic bomb design at the Los Alamos Scientific Laboratory in New Mexico. The sessions were taped at the studios of Audvid Film and Tape Production, in Boston, Massachusetts.

Four physicists who played important roles in the "Trinity" atomic bomb test at Alamogordo, New Mexico, were reunited for Session Nine. **Kenneth Bainbridge**, a physicist at Harvard University, designed and built the Harvard cyclotron which was used at Los Alamos. In 1940 he joined researchers on radar at the Massachusetts Institute of Technology, and soon after went to Cambridge University in England to work on radar and uranium experiments. He was recruited for the Manhattan Project and moved to Los Alamos in the summer of 1943. In March 1944, he took charge of the Trinity test and administered it from site selection to detonation. **Donald Hornig**, also a physicist at Harvard before he joined the Los Alamos staff, designed the high-voltage capacitors that fired the Fat Man's multiple detonators. **Philip Morrison** received his Ph.D. in theoretical physics from the University of California at Berkeley in 1940, and worked on the Project at the Metallurgical Laboratory of the University of Chicago before arriving at Los Alamos in 1944 to serve as Physicist and Group Leader. **Robert Wilson** had recently completed his Ph.D. at the University of California at Berkeley and taught at Princeton University before he arrived in Los Alamos in April 1943. He headed various subgroups engaged in cyclotron research for the Trinity test.

Session Ten participants worked at Los Alamos with different levels of responsibility. **Robert Wilson** and **Robert Serber** were Division Leaders. Serber received his Ph.D. from the University of Wisconsin in 1934, and worked with J. Robert Oppenheimer as a Research Associate at the University of Chicago's "Met Lab" before arriving at Los Alamos. Serber's introductory lectures on the physics and chemistry of the Project in April 1943, became the Los Alamos Primer. **Anthony French** received his A.B. in physics at Cambridge University. He worked at the Cavendish Laboratory there before coming to the Los Alamos in 1944. **David Frisch** was still a graduate student when he arrived at Los Alamos as a Junior Physicist in 1943. He received his Ph.D. from MIT in 1947.

Four women from Los Alamos convened to discuss their professional and domestic lives in Session Eleven. **Lillian Hornig** received her M.A. in chemistry from Harvard in 1943 and her Ph.D. in 1950. From 1944 to 1946 at Los Alamos she served as Staff Scientist in the plutonium chemistry division and as Section Leader for high explosives development. **Rose Frisch** received her Ph.D. in physiological genetics from the University of Wisconsin in 1943. At Los Alamos, she monitored the effects of radiation in the medical laboratory. **Alice Kimball Smith** received her Ph.D. in history from Yale University and taught social studies at Los Alamos High School. After the war she served as historian for the Association of Los Alamos Scientists. Her book, A Peril and a Hope: The Scientists' Movement in

America 1945-1947, was published in 1965. **Jane S. Wilson** also taught at the Los Alamos High School.

Physicists who worked on the implosion program gathered for Session Twelve. **Bernard T. Feld** worked at the Met Lab at the University of Chicago before coming to Los Alamos in 1944. He received his Ph.D. in physics from Columbia University in 1945. **Cyril Smith** received his D.Sc. in metallurgy from MIT in 1926. He served as associate division leader in metallurgy at Los Alamos from 1943 to 1946. **Robert Serber** and **Philip Morrison** appeared again in this interview.

Goldberg encouraged discussion of the culture and the workload at Los Alamos, and the attitudes towards that work and its consequences. Sessions Nine through Twelve were shot on one-inch tape; sessions Thirteen through Sixteen were shot on three-quarter-inch U-Matic tape.

Box 3 Transcripts of Sessions

Session Nine (December 1, 1987), at the Audvid Film and Tape studio, Boston, Massachusetts, featured Bainbridge, Hornig, Morrison, and Wilson on preparations for and execution of the Trinity test at Alamogordo, c. 1944-1945, including:

- working groups at Los Alamos;
- instrumentation devised for the Trinity test, the hundred-ton test, the "Jumbo" blast containment canister, and the spontaneous fission of plutonium;
- Los Alamos reorganization and formation of the G division during the summer of 1944;
- measurements of the effects of the bombing of Hiroshima.

Visual documentation included:

- copies of Manhattan Project documents;
- Los Alamos personnel graph;
- photographs from Bainbridge's Trinity test report.

Pages 1 - 95

Session Ten (December 1, 1987), at the Audvid Film and Tape studio featured French, Frisch, Serber, and Wilson on the organization of and the scientific activities at Los Alamos, c. 1943-1945, including:

- rationale for forming the Laboratory and the considerations for site selection;
- whether to make Los Alamos a military or civilian camp;
- background to the Serber lectures of April 1943;
- performance of Oppenheimer as administrator and colleague;

- living conditions;
- changing perceptions of the nature of making an atomic bomb;
- the spontaneous fission of plutonium and its effect on the organization of the laboratory;
- the British contingent and comparisons between the Los Alamos and the Cavendish Laboratories;
- controversies over the use of the bomb;
- objections to pursuing research for the hydrogen bomb;
- formation of the Association of Los Alamos Scientists.

There is no visual documentation in this session.

Pages 1 - 79

Session Eleven (December 2, 1987), at the Audvid Studios featured Hornig, Frisch, Smith, and Wilson on the roles of women in both the domestic and scientific life of Los Alamos, c. 1943-1946, including:

- homemaking facilities and child care arrangements;
- social and recreational activities in the area;
- school and library system;
- Hornig's role as a scientist in the implosion program and high explosives development;
- Frisch on radiation research at the medical laboratory;
- agreement on constraints on women's participation in scientific activities;
- perceptions of the Trinity test and post-Trinity conflicts between the scientific community and the military.

Visual documentation included:

- period photographs of Los Alamos.

Pages 1 - 93

Session Twelve (December 2, 1987), at the Audvid Studios featured Feld, Smith, Serber, and Morrison on the design and fabrication of a viable implosion system for the plutonium bomb, c. 1944-1945, including:

- metallurgical experiments to determine characteristics of plutonium;
- technologies for creating plutonium hemispheres;
- strategies to prevent plutonium oxidation;
- design and use of the initiator;
- preparations at Tinian Island for Nagasaki bombing;
- division of responsibilities for various bomb components;
- early surveys at Hiroshima to determine effects of the bomb.

Visual documentation included:

- copies of Manhattan Project documents;
- gold foil ring for the initiator in the Trinity device;
- uranium 238 sample.

Pages 1 - 67

Videotapes of Sessions

Session Nine - Tape One - December 1, 1987 - 120 minutes
Session Nine - Tape Two - December 1, 1987 - 45 minutes

Session Ten - Tape One - December 1, 1987 - 120 minutes
Session Ten - Tape Two - December 1, 1987 - 35 minutes

Session Eleven - Tape One - December 2, 1987 - 120 minutes
Session Eleven - Tape Two - December 2, 1987 - 35 minutes

Session Twelve - Tape One - December 2, 1987 - 110 minutes

Collection Division 4: Los Alamos

Interviewees in this collection also contributed to the atomic bomb design and testing program in New Mexico. J. Robert Oppenheimer, the physicist from the University of California at Berkeley charged with supervising this part of the Manhattan Project, picked the Los Alamos location because of its isolation and its beauty. The sessions were shot at the Trinity test site and in a Los Alamos conference room.

The participants in Session Thirteen worked on the Trinity test at different levels of responsibility. **Kenneth T. Bainbridge** administered the test from site selection to the write-up of the official report. He came to Los Alamos in the summer of 1943, having previously worked on radar at Massachusetts Institute of Technology and in England; he had also designed and built Harvard University's first cyclotron. **Robert Wilson** was not yet thirty when he arrived at Los Alamos in April 1943, where he headed various subgroups using cyclotron research. Wilson had come directly from a Ph.D. at Berkeley and a teaching and cyclotron research post at Princeton. During the Trinity test he helped install the bomb and measured implosion and fission behavior during the explosion.

Session Fourteen's participants worked at various levels on the theoretical underpinnings of an atomic weapon. **Hans Bethe**, head of the Theoretical Division at Los Alamos, left Germany in 1933 for Cornell University before applying his research to the war effort in 1940. He gave up his work on explosives and radar in July 1942 when he became convinced of the feasibility of an atomic bomb. Bethe returned to Cornell after the war and won the Nobel Prize in Physics in 1967. **Frederick Reines** made his reputation as a doctoral student at New York University when he developed

a new equation in applied mathematics. Reines joined the Theoretical staff in late 1943 and remained at Los Alamos as a Group Leader until 1959. Canadian **J. Carson Mark** came to Los Alamos via research at the Metallurgical Laboratory in Chicago and a doctorate under Oppenheimer at Berkeley. Fellow Canadian **Robert Christy** arrived by way of George Placzek's Montreal study group and summer seminars in applied mathematics at Brown University in 1941 and 1942.

In addition to **Bainbridge**, Session Fifteen included experimentalists **Robert Bacher** and **Norris E. Bradbury** and administrator **David Hawkins**. Bacher led fission studies at Cornell and worked at MIT's Radiation Laboratory before heading the Experimental and Gadget Divisions at Los Alamos. His opposition to militarized working conditions manifested itself further in his post-war efforts for civilian control of nuclear research. Bradbury put his Berkeley Ph.D. to work at the U.S. Navy's Dahlgren Proving Ground for four years before his assignment to the Ordnance Division at Los Alamos. He replaced Oppenheimer as director of the Los Alamos Laboratory in September 1945. Hawkins grew up in New Mexico and went to Berkeley to earn his Ph.D. in philosophy. He returned as administrative assistant to Oppenheimer in the summer of 1943, becoming the primary liaison between the scientists and the military administration of the Project.

Goldberg elicited comparisons of the work experience from the theoretical and experimental physicists as well as discussions of social life at Los Alamos. The series was shot on half-inch Betacam tape and provides visual documentation of the Trinity test site, Los Alamos in 1989, and period photographs of Los Alamos and preparations for the Trinity test.

Box 4 Transcripts of Sessions

Session Thirteen (August 15, 1989), at the Trinity test site, Alamogordo, New Mexico, featured Bainbridge and Wilson. Their commentary on the preparations for and detonation of the Trinity bomb, c. 1944-1945, was sparked almost solely by the surviving structures at the site and included:

- post-blast sample collection;
- test site selection;
- Jumbo blast containment cannister;
- Julian Mack's camera system;
- quality of life at and uses of base camp;
- U.S. Air Force overflights and bombing practices;
- effect of the blast on base camp.

Visual documentation included:

- Ground Zero;
- Jumbo;
- West 800 camera bunker;
- Base Camp;
- McDonald Ranch house;
- West 10,000 observation bunker.

Pages 1 - 60

Session Fourteen (August 18, 1989), at Fuller Lodge, Los Alamos Scientific Laboratory, featured Bethe, Christy, Reines, and Mark on the theoretical physicist's view of the Laboratory's operation, c. 1943-1945, including:

- reasons for participation;
- division of labor between and relationship of theoretical and experimental physicists;
- insulation from other Project researchers;
- uranium hydride program;
- theoretical and experimental emphasis on physical power of atomic blast;
- implosion research;
- reactions to the Trinity test;
- post-blast measurements of yield.

Pages 1 - 51

Session Fifteen (August 18, 1989), at Fuller Lodge, Los Alamos Scientific Laboratory, featured Bacher, Bainbridge, Bradbury, and Hawkins on the experimental physicist's experience at the Laboratory, c. 1943-1945, including:

- methods of recruitment and reasons for participation;
- debate on civilian or military nature of Lab;
- differences between internal and external exchanges of information;
- organization of Laboratory;
- gun experiments;
- relationship of Groves and Oppenheimer;
- weekend recreation;
- understanding Klaus Fuchs;
- predicting the Soviet bomb;
- post-blast depression and efforts to regulate nuclear power.

Pages 1 - 62

Session Sixteen (August 19, 1989), consisted of visual documentation of the landscape of Los Alamos in 1945 and 1989, including:

- Nine still photographs of the preparations for Trinity;
- views of Los Alamos Scientific Laboratory and environs;

Pages 1 - 3

Videotapes of Sessions

In Sessions Fourteen and Fifteen, Camera A focussed mainly on the speaker. Camera B focussed mainly on the listeners or the group as a whole.

Session Thirteen - Tape One - August 15, 1989 - 120 minutes
 Session Thirteen - Tape Two - August 15, 1989 - 10 minutes

Session Fourteen - Tape One - Camera A - August 18, 1989 - 120 minutes
 Session Fourteen - Tape One - Camera B - August 18, 1989 - 120 minutes

Session Fifteen - Tape One - Camera A - August 18, 1989 - 120 minutes
 Session Fifteen - Tape One - Camera B - August 18, 1989 - 120 minutes
 Session Fifteen - Tape Two - Camera A - August 18, 1989 - 15 minutes
 Session Fifteen - Tape Two - Camera B - August 18, 1989 - 15 minutes

Session Sixteen - Tape One - August 19, 1989 - 25 minutes

Collection Division 5: Alberta

Four participants from "Project Alberta" convened for Sessions Seventeen and Eighteen. This phase of the Manhattan Project dealt with the conversion of the Trinity test device into the practical weapons systems that were used twice on Japan. The interviewees were among those who designed the bombs to fit the B-29, wired them with redundant electronics, rehearsed the mission, established a base on Tinian Island, and released the bombs over Hiroshima and Nagasaki. The sessions were shot at the National Museum of American History in Washington, D.C., and at the Paul E. Garber Facility in Suitland, Maryland.

Norman F. Ramsey, Jr., received his Ph.D. in physics from Columbia University in 1940. During World War II, Ramsey consulted with various government groups concerned with military technology. In 1943 he moved from the offices of the Secretary of War to Los Alamos, where he became a

group leader for bomb delivery. After the war, he returned to Columbia and won the Nobel Prize for Physics in 1938. **Harold M. Agnew** received his A.B. in chemistry from the University of Denver in 1942. His advisor referred him to Enrico Fermi, under whom he was responsible for some of the measurements of the atomic explosions over Japan. After the war Agnew earned his Ph.D. in particle physics at the University of Chicago before returning to Los Alamos Scientific Laboratory. He directed the Laboratory there from 1970 to 1979.

Frederick L. Ashworth graduated from the United States Naval Academy and completed the Naval Postgraduate School course in ordnance engineering shortly before the Japanese attack on Pearl Harbor in 1941. After service in the Pacific Theater of Operations, he worked for William S. Parsons and Ramsey on the detonation components of the atomic bombs. Ashworth acted as weaponeer on the Nagasaki mission and as General Groves's representative on Tinian Island. His book, The Atomic Bombings of Hiroshima and Nagasaki, was published in 1947. **Charles W. Sweeney** was born in 1920 and grew up in eastern Massachusetts. He enlisted as an air cadet in April 1941, and rose to commander of a bomber squadron in the European Theater of Operations. With nearly three thousand hours of accident-free flight time to recommend him, Sweeney joined Colonel Paul Tibbetts's 509th Composite Group of B-29's in September 1944. He piloted an observation plane at the Hiroshima bombing and dropped the Fat Man over Nagasaki from Bock's Car. After he completed his enlistment, he returned to Massachusetts to begin a wholesale leather business and served in the Air National Guard until 1976.

Goldberg used the Enola Gay site to draw from the participants details of their involvement with the technologies of Project Alberta. Other questions stimulated recollections of experiences on Tinian Island and on the two missions to Japan. The sessions were shot with half-inch Betacam tape and provide visual documentation of the Little Boy and Fat Man bomb models and the B-29 Enola Gay.

Box 5 Transcripts of Sessions

Session Seventeen (June 5, 1990), at the National Museum of American History, Washington, D.C., featured Agnew, Ashworth, Ramsey, and Sweeney on their assignments in Project Alberta, c. 1944-1945, including:

- designing the Fat Man plutonium bomb: contents and aerodynamics;
- definition of weaponeer's role and mechanics of bomb;
- William S. Parsons' contributions to Manhattan Project;
- reasons for interviewees' participation in Manhattan Project;
- flight training for the 509th Composite Group;
- preparation of Tinian Island as flight base ("Project Silverplate");

- life on Tinian with conventional B-29 bomber crews;
- comparison of Hiroshima and Nagasaki missions;
- photography of atomic explosions;
- disposal of facilities at Tinian after Nagasaki explosion;
- reaction to use of the bombs.

Visual documentation included:

- the Fat Man bomb casing on display at the Museum.

Pages 1 - 109

Session Eighteen (June 6, 1990), at the Enola Gay restoration project, Paul E. Garber Facility, Suitland, Maryland, featured Agnew, Ashworth, Ramsey, and Sweeney on the specific technologies required for Project Alberta, c. 1944-1945, including:

- designing the Little Boy uranium bomb: contents and aerodynamics;
- development of bomb release mechanism;
- Bernard O'Keefe's claim to last-minute rewiring of Little Boy;
- flight qualities of B-29;
- pilot-bombardier communication;
- the Nagasaki mission;
- operation of Norden bombsight;
- comparison of the two bombs' electronic characteristics;
- mechanics of monitoring devices at Hiroshima.

Visual documentation included:

- the Little Boy bomb casing on display next to the Enola Gay;
- the bomb bay, cockpit, and weaponeer's cabin of the Enola Gay.

Pages 1 - 39

Videotapes of Sessions

Session Seventeen - Tape One - June 5, 1990 - 120 minutes

Session Seventeen - Tape Two - June 5, 1990 - 70 minutes

Session Eighteen - Tape One - June 6, 1990 - 80 minutes

Manhattan Project: Session Four

Pile Design and Construction

Interview with
Eugene Wigner, Lyle Borst, Edward Creutz,
Alvin Weinberg, Albert Wattenberg, Dale Babcock

March 3, 1987
in Oak Ridge, Tennessee

by Stanley Goldberg
Interviewer

for the Smithsonian Institution Videohistory Program

NAME LIST

RU 9531

Manhattan Project: Session Four

Interviewees: Eugene Wigner, Lyle Borst, Edward Creutz, Alvin Weinberg, Albert Wattenberg, and Dale Babcock

Date: March 3, 1987

<u>Page</u> ¹	<u>Name</u>
2	Szilard, Leo
2	Bourke, Neils Henrich David
2	Hahn, Otto
2	Strassman, Fritz
3	Hitler, Adolf
3	Fermi, Enrico
3	Teller, Edward
5	Joliot-Curie, Frederick
7	Bothe, Walter Wilhelm
8	Wilson, Robert Rathburn
13	Neumann, John Von
14	Lavender, Robert A.
16	MacPherson, Herbert Grenfell
19	Einstein, Albert
19	Roosevelt, President Franklin Delano
23	Anderson, Herbert Lawrence
25	Compton, Arthur Holly
26	Snell, Arthur Hawley
26	Allison, Samuel King
26	Fisk, James Brown
26	Schockley, William
28	Gurinski, David Harris
29	Zinn, Walter Henry
32	Kowalski, Lou
32	Halban, Hans von
33	Bush, Vannevar
33	Conant, James Bryant
33	Heisenberg, Werner
35	Rommel, Erwin
36	Turner, Louis Alexander
37	Weizacker, Carl F. von
38	Feld, Bernard Taub
42	Urey, Harold Clayton
43	Greenewalt, Crawford Hallock
47	Rashevsky, Nicholas
47	Young, Gale
48	Wheeler, John Archibald
51	Hutchins, Robert Maynard

¹Page number indicates first reference to name for interview Session Four. Subsequent references may be found in the text of the interview.

53	Seaborg, Glenn Theodore
53	Oppenheimer, Robert
55	Leverett, Miles Corrington
55	Cooper, Charles Burleigh
55	Moore, Thomas
60	Marshall, Leona Woods
60	Morrison, Philip
62	Spedding, Frank H.
65	Feld, Bernard
65	Marshall, John
65	Hilberry, Horace Van Norman
65	Nobles, Robert
68	Groves, Leslie R.
69	Graves, Alvin Cushman
70	Marden, D.W.
70	Renssler, John
73	Alexander, Peter Popow
74	Sengier, Edgar
74	Nichols, Kenneth David
77	Snyder,
77	Jones, R.V.
77	Doan, Richard L.
78	Neddermeyer, Seth Henry
79	Manley, John Henry
79	May, Alan Nunn
81	Ohlinger, Leo Arthur
84	Greager, Oswald Herman
92	Cohen, Karl Paley
92	Vernon, Harcourt C.
92	Smyth, Henry DeWolf
93	Lichtenberger, Harold V.
94	Benedict, Manson
95	Matthias, Franklin T.
105	Newson, Henry Winston
110	Placzek, George
110	Mendelev, Dmitri Ivanovich
115	Foster, Michael
124	Graves, George
128	Chipman, John
131	Gast, Paul Frederick
134	Worthington, Hood
134	Squires, Lombard
136	Knuth, Gus
136	Getzholtz, Wallace
136	O'Donnell, Tom
137	Bridgman, Percy Williams

00:05:30:00 [Begin VHS Tape 1 of 2]
[Begin U-Matic Tape 1 of 4]

[Interview begins with Wigner, Borst, Creutz, Weinberg, Wattenberg and Babcock seated at two tables placed in a V-formation at the Kennedy Maxwell Studios in Oak Ridge, Tennessee. Goldberg is seated in the center of the two tables]

GOLDBERG: To begin, let me turn to Dr. [Eugene Paul] Wigner, and let's start at the beginning, I guess it was in 1939 when you and Leo Szilard first got together and decided that America had to do something about the question of fission.

WIGNER: Well, I think I can say something even earlier. [Neils Henrick David] Bourke came to Princeton [University] for half a year, and he gave a talk at the physics colloquium about the new inventions of--who is it?

CREUTZ: [Otto] Hahn and [Fritz] Strassmann.

WIGNER: Who?

GOLDBERG: Hahn and Strassmann.

WIGNER: Hahn and Strassmann--about the invention of the fission of uranium, and that was a great surprise. I didn't hear his talk because I had jaundice and was in the hospital, but Szilard listened to it and he came to me and explained what he had heard, and we soon realized that there is a possibility of having a chain reaction and having not only energy production but explosion production.

WEINBERG: You realized that while you were still in the hospital?

WIGNER: Yes, essentially.

- WEINBERG: Or you mean while you were talking, you realized it?
- WIGNER: It doesn't--I didn't stay long in the hospital, but it was pretty evident.
- CREUTZ: It gave you a jaundiced view, though.
- WIGNER: Huh?
- BORST: It gave you a jaundiced view.
- CREUTZ: That means a skeptical view.
- WIGNER: [Laughter] No, we were afraid of it, and we decided it would be very good if this were investigated, because we were afraid that [Adolf] Hitler will have it investigated and will develop new types of weapons and will conquer the earth. You know that he claimed that Germany would conquer the earth. "Deutschland, Deutschland, uber alles, uber alles, in der Welt." Well, "in der Welt" was exaggerated, but we were afraid that he wanted to conquer the earth.
- BORST: When did [Enrico] Fermi come into the picture?
- WIGNER: A good deal later, but then very effectively and very vigorously. Fermi was not present at these discussions as yet, but soon enough he learned about it.
- GOLDBERG: Was [Edward] Teller among the people? You discussed this with Edward Teller as well?

WIGNER: Well, that also came a good deal later. We realized this danger and we thought what kind of factors should be included in the calculation of an explosion.

WEINBERG: Did you try to calculate the size of an explosive device at that time?

WIGNER: Yes, yes.

WEINBERG: So you were probably the first one to make an actual calculation of an explosive device.

WIGNER: I don't know. That wasn't very difficult, and it was pretty obvious.

WEINBERG: How large did you estimate the explosive device to be?

WIGNER: In the beginning, we couldn't estimate it at all, but soon enough, under experiments which were made at Princeton in particular by Dr. Edward C. Creutz. . .

CREUTZ: And Eugene Wigner.

WIGNER: No, I did not. I just persuaded you to make the experiments. Of course, those of us who came from Europe realized the danger much more than American people, because it was so far away and they can't attack us as yet. But we were afraid that they'd develop atomic weapons and threaten the rest of the world and conquer it.

00:09:50:00

WEINBERG: That's a kind of important historical point there, Eugene, because there's a fair amount--I shouldn't say controversy, but argument, I guess, as to who was the very first person

to try to estimate the size of a uranium bomb. My guess is that you probably were, but you didn't realize it at the time.

WIGNER: That I am the first? No. I thought that many people must have thought of it, in particular, also in Germany.

WEINBERG: Yes, but who made the first calculation of the size?

WIGNER: I don't know. It may be that in Germany it was made also, but apparently, Hitler was not in. . .

WEINBERG: In England, there were calculations also.

WIGNER: Also. Yes.

WATTENBERG: I was going to say Szilard had been looking for a chain reaction since 1934 or something.

GOLDBERG: Yes.

WATTENBERG: But as well as that, you have--and I've never been clear about this--[Frederic] Joliot-Curie was very conscious of the possibility.

WIGNER: Who?

WATTENBERG: Joliot-Curie was very conscious of the possibility of the chain reaction. I don't know when Szilard corresponded with him.

WIGNER: I don't think so.

WATTENBERG: That they ever discussed that aspect of it.

WIGNER: No, I don't think so.

00:11:15:00

CREUTZ: Eugene, I remember asking you at one time how big a reactor has to be.

WATTENBERG: A reactor or a bomb?

CREUTZ: A reactor.

WATTENBERG: A different question.

CREUTZ: Yes, it's a different question. You suggested about the size of a 55-gallon oil drum, which is so big around. [Laughter]

WIGNER: [Laughter] I didn't know. I didn't remember.

GOLDBERG: But in those early days, Szilard had already applied for a patent.

WEINBERG: No, he had received a patent, a British Admiralty Patent in 1935.

WATTENBERG: 1935?

WEINBERG: Yes. Based on the beryllium N,2N reaction.

GOLDBERG: He thought it would be beryllium.

WIGNER: Yes. Which, of course, did not work. There are some errors in the cross-sections.

00:12:02:00

GOLDBERG: But in that early period, did you really believe that it was a likely and practical outcome that there would be a chain reaction?

WIGNER: Oh, yes. Oh, yes. And it was very natural to believe that, because, you see, if you see how many neutrons are in the collision and how many neutrons would not be in the collision-- in the separation of the two fission products, it was evident that there is a surplus of neutrons. In fact, a surplus of two-and-a-half, about. And we realized that some of these will be emitted, and those can be absorbed again. Of course, it is not so easy, we realized. Bohr pointed out that it's very likely that Uranium-235 is a principal fissionable nucleus, not U_{238} . And we soon realized that Uranium-238 is, in fact, absorbing neutrons. But all this was a little bit in the air, that the experiments on absorption and these things, I tried to induce the Princeton people to do it, and I succeeded. And they were very excellent experiments--made in particular by Dr. Edward Creutz.

CREUTZ: You may remember you showed us how to do them. You were putting samples under a Geiger counter, trying to keep the geometry very constant, and you said, "But that's not very good, because small deviations will affect the intensity that reaches the counter." So you said, "Why don't you wrap the sample around the counter, then small deviations will have less effect?" And then the experiments began to work. Thank you. [Laughter]

WIGNER: No, I don't remember this at all.

00:14:04:00

GOLDBERG: Let's turn to that period, because one of the things that's striking to me, as an historian looking at it, is the fortunate fact that in Germany, they looked at carbon as a moderator. And I guess it was [Walter Wilhelm] Bothe who made the measurements

and decided that it would not work. But in this country, almost from the beginning, Szilard and you and Fermi and everyone else were convinced that you could use carbon.

WIGNER: Yes.

GOLDBERG: Even though in the beginning, you weren't getting the multiplication that was required.

WIGNER: Well, we realized that hydrogen is a natural moderator, but it absorbs neutrons with reasonable vigor. And we realized that perhaps it's not good to try to use water as a moderator. Ed Creutz and his collaborator, one of them became a very famous person also. Who was that? Very much older than you are.

WEINBERG: You mean Bob [Robert Rathburn] Wilson?

GOLDBERG: Robert Wilson?

WIGNER: Yes, maybe Bob Wilson.

CREUTZ: Wilson worked on the experiments trying to pin down whether it was Uranium-235 or 238 that was the main fissionable isotope. Wilson worked on that, but then he invented a method of separating isotopes, so he left the nuclear work on uranium.

WIGNER: Yes.

GOLDBERG: What method was that?

CREUTZ: It was called the isotron. You shoot a beam of uranium ions down a tube, and if they all have the same energy, the lighter ones will be going faster, and so they all reach the

target sooner. So you send a series of pulses down, then have a deflector that keeps knocking the fast pulses into a collector.

WIGNER: Perhaps I mention again, even though you may not like it, that those of us who came from Europe were much more afraid of the danger than those who were here in America, who were born here in America. Because Germany was so far away, the fact that they might want to conquer the United States and the whole country, the whole world, was far from them. And Dr. Creutz once came to me when I persuaded them again and again to make experiments and measurements, he said, "Eugene, you are pleasantly disagreeable." [Laughter]

CREUTZ: I thought it was. . .

GOLDBERG: Well, Dr. Creutz, why don't you say a little bit about those experiments?

CREUTZ: Well, they were really very simple and extremely crude by today's standards.

WATTENBERG: Do you know what year that was then?

CREUTZ: I certainly do. We started this work--we began thinking about it, actually, in 1939. We had some experiments going by '41.

WATTENBERG: What month did Szilard come to Princeton?

CREUTZ: In February.

WATTENBERG: So it was early in '39.

CREUTZ: It was early, yes.

WIGNER: He came in February.

00:17:20:00

GOLDBERG: And the announcement of fission had been just at the end of 1938.

CREUTZ: It was in The New York Times; that's where I first read about it, yes. And then Bohr gave us some more of the details, and Eugene Wigner persuaded us to stop some work that we were doing. We were doing some proton scattering work. He persuaded us that that was interesting, but much more important would be to try to understand some of the nuclear physics of uranium. Well, the idea was to take a continuous spectrum of neutrons, or nearly continuous spectrum, and let those strike the uranium sphere, and then to see how many of the neutrons were absorbed by the Uranium-238 and therefore lost for the fission reaction. The idea was to see how big such a sphere should be, what would happen at various temperatures, what would happen at various densities of the material.

WATTENBERG: Were you a research associate of Princeton then or a professor?

CREUTZ: I was actually called an instructor at the time.

WATTENBERG: So you were actually teaching there at the time.

CREUTZ: It was a temporary appointment.

BORST: You had your degree, though.

CREUTZ: I had my degree. Well, as a matter of fact, when I first went to Princeton, I did not have my degree, and I was registered at Princeton as a partial student.

BORST: Who was your official professor at Princeton?

CREUTZ: Well, I did not have a professor at Princeton, because I was still a student at [The University of] Wisconsin. Gregory Breit was my thesis professor at Wisconsin.

GOLDBERG: Gregory Breit.

CREUTZ: Yes. So I was called a partial student. That sort of meant you were half-witted.

BORST: These experiments with uranium were not with metallic uranium?

CREUTZ: We did a lot of experiments with U_3O_8 , uranium oxide, because that's what was available. Finally, we were able to get some uranium dioxide, UO_2 , which is better and it's denser, and we finally got--it's another story--we got some uranium metal from Westinghouse [Electric and Manufacturing] and did some experiments with uranium metal.

GOLDBERG: That's a story we'll want to get to later.

CREUTZ: Okay, fine. Well, the experiments consisted of having a cubic meter of graphite, which we could raise to temperatures up to 1000 degrees centigrade. That, of course, presented some interesting problems; uranium graphite oxidizes at that temperature in air. So we had to insulate it with something that could stand the 1000 degrees centigrade and also would not absorb too many neutrons. So we used lamp black and had a six-inch layer of loose lamp black, which, of course, was dirty material to work with, but it worked.

We also had to have detectors for neutrons inside the graphite block. One of the techniques was to use indium foils, but since indium will anneal by the temperature of your hand, we couldn't very well use

that. [Laughter] So we tried various indium salts, and nothing really worked very well. We used the sulphite way.

So we turned on the cyclotron, let the proton beam strike a lithium target. That produced neutrons which came into the graphite block. They're fast neutrons. They're moderated by the graphite, and then many of them would strike the uranium sphere. And this idea, mostly Eugene's and partly Szilard's, that if you have the uranium lumped this way, you'll catch a lot of resonance neutrons on the surface.

00:20:43:00

GOLDBERG: Why don't you say something about what resonance neutrons are.

CREUTZ: Okay. Sure. If you plot the cross-section for capture of a neutron by almost any element, plot energy this way and capture cross-section this way, it'll fall off of the so-called one-over-v, one over the velocity law. Then when the energy gets in the vicinity of maybe an electron volt or so, many elements will show resonances.

WIGNER: (inaudible) resonances.

CREUTZ: And uranium is such a material as resonance. It's just above the thermal energy. So the idea is to--somehow if you could eliminate those neutrons, then the other neutrons would be available to produce fission. If you don't eliminate them, they, of course, will be captured by the Uranium-238 to make 239, and then other neutrons will be slowed down by the moderator, and that resonance will keep soaking up the neutrons and essentially removing them. So Wigner and Szilard's idea was to have a lump of some kind of uranium; the neutron spectrum coming in, hitting the surface, would be depleted quickly--immediately--of those resonance neutrons. The neutrons then enter the sphere where the uranium is, will be deficient in those neutrons, so they can't be absorbed because they aren't there.

[Laughter] Now, the problem is some neutrons will be slowed down within the sphere also. But the denser you make it, the less problem you have of that. That's why we wanted to go from U_3O_8 to UO_2 to uranium metal.

We also did this as a function of temperature, and having a lot of lampblack at 1000 degrees centigrade meant there was a fair amount of carbon monoxide generated. We read about people in coal mines using canaries to tell them if there was carbon monoxide and dioxide. We didn't get canaries; we got white rats. We put white rats around the experiment. Well, they didn't die, so we said we should use them for something scientific, so we took some doughnuts and shook them up in uranium oxide, fed them to the rats, and they died.

WATTENBERG: They did die?

CREUTZ: They did die.

WATTENBERG: How much uranium did they take in?

CREUTZ: They probably took in almost a gram, because the doughnuts were thickly covered with uranium oxide.

00:22:55:00

BORST: Well, before we go too far, I think we ought to talk about the U.S. patent, about the United States patent.

WIGNER: Patent?

BORST: Yes.

GOLDBERG: For the reactor?

BORST: In the name of Fermi, Szilard, [John] Von Neumann, was it?

- GOLDBERG: No, just Fermi and Szilard.
- BORST: There was a later patent that had four inventors. [Aristid Victor von] Grosse was one of them.
- WIGNER: I don't remember. [Laughter]
- BORST: How much later did this patent show up after what you've been talking about?
- GOLDBERG: Much later. I know when it was granted; it was granted in 1954.
- WIGNER: I don't remember.
- WEINBERG: I don't think the application was made until late in the war.
- WIGNER: That wasn't important.
- GOLDBERG: The Office of Scientific Research and Development [OSRD] and the Manhattan Project had a patent lawyer. He was in the Navy. Captain Lavender was his name, and he used to go around to the various sites.
- BORST: That's after we got going. You see, this is when the experimental work was being done at Columbia. You say it was Fermi and Szilard who were authors on this patent?
- WIGNER: I don't remember.
- WEINBERG: The basic patent on chain reactions is Fermi and Szilard.

WIGNER: Oh, good. I didn't remember.

WEINBERG: They got--what was it?--\$25,000 bucks apiece?

CREUTZ: Yes.

00:24:23:00

GOLDBERG: I want to go back to this question of carbon as a moderator. Why were you convinced? I mean, the Germans were quickly convinced that it wouldn't work, and yet it seems in this country everybody immediately--Fermi, anyhow, and the people who were working at Columbia [University] and, I guess, the people who were working at Princeton, immediately decided that the capture cross-section you were measuring for carbon was a function of impurities.

WIGNER: Well, you see, that was natural. We realized that hydrogen has a reasonably high thermal absorption--I think .3, which is not so high, but still, still much higher. And carbon has a cross-section of, I think, 3 of 1,000. Isn't it? Or 4 of 1,000.

CREUTZ: It was unmeasurable at that time.

WIGNER: The energy effect, the slowing down effect is smaller, but only by a factor of ten. So it was much better to use carbon. It would have even been better to use oxygen, but that is a gas, so it wasn't practical we realized.

WEINBERG: Fermi at Columbia was the first to measure the process.

WATTENBERG: One of Szilard's first enterprises was to get a large quantity of graphite, to see if carbon was any good.

And I think he did that in 1939, and he also rented a radium or radium beryllium source. This I read about; I was not there.

WIGNER: The reason that beryllium and his patent was for beryllium, which turned out not to be manageable, because. . .

WEINBERG: It had a different patent, though, Eugene. At that time, he actually thought that beryllium would undergo fission.

WIGNER: Yes.

WATTENBERG: So as far as the records are concerned. . .

WEINBERG: But those experiments were done at Columbia, weren't they? Weren't they involved in those experiments?

WATTENBERG: That's right. I think it was '39. Yes, I think they were collaborating. They were already collaborating with the graphite-building.

WEINBERG: Let me introduce here another point which most people are not aware of. There's a man by the name of H.G. MacPherson, who later became the Deputy Director of the Oak Ridge National Laboratory, and he, at that time, was probably the world's largest expert on graphite. He was the Director of Research at [The] National Carbon [Company]. And I've often talked to him about those early days. It was he, really, as much as anyone, who said, "Well the graphite. . . ."--he had a very sensitive understanding of what the impurities in graphite were. I don't know if you've ever talked to Mac about that, Ed. I think that he deserves a great deal of credit for having claimed that it was the impurities in graphite that could be removed that were involved.

- GOLDBERG: There is this enormous correspondence--you're right--in the National Archives, an enormous correspondence, beginning in 1939 with Szilard and National Carbon.
- WEINBERG: Right. Right.
- GOLDBERG: First to get the hydrogen out, and then later to get the boron out, because boron is such a terrible poison.
- WEINBERG: That was MacPherson, who was involved in that.
- GOLDBERG: One of the suggestions has been that Bothe's measurements were really--he really knew better, but that he did it on purpose. I mean. . .
- WEINBERG: I have no idea whether that's correct or not.
[Laughter]
- GOLDBERG: Well, I don't know either.
- BORST: After the fact. . .
- CREUTZ: There were no very good analytical methods for measuring boron and carbon in those days. We had worked one at the Bureau of Standards, for example. They said the best they could do would probably be about ten parts per million, and we were looking for less than one part per million.
- GOLDBERG: You had to get the boron out to at least one part per million.
- CREUTZ: That was the belief, yes.

- GOLDBERG: And if you didn't do that, then you couldn't get a multiplication?
- CREUTZ: Well, the boron captures the neutrons, so they're wasted.
- GOLDBERG: So we're talking about a new standard of purity for carbon.
- CREUTZ: That's right. A new standard of chemical analysis.
- WATTENBERG: I want to say something. I had worked with graphite as the container in spark spectroscopy for very high-purity liquids, and we were using it because carbon is rather pure. We were working then in parts per million of many constituents. I don't know that we could see boron in the spectra though.
- GOLDBERG: When was that?
- WIGNER: Perhaps. . .
- WATTENBERG: I was doing that before I joined the project.
- WEINBERG: At Columbia, was it, Al?
- WIGNER: Perhaps it. . .
- WATTENBERG: No, it was in Schenley's, Schenley Distilleries.
- WEINBERG: Oh! [Laughter]
- WATTENBERG: I was a chemical spectroscopist.
- GOLDBERG: For Schenley Distilleries?

WATTENBERG: Yes.

00:29:34:00

WIGNER: Perhaps I also should mention that Szilard and I and probably others thought it would be very good to interest the government in it, and we tried to talk to people, but we were very unsuccessful. Then Szilard had the idea we should try to get [Albert] Einstein interested, so we went out to see Einstein in Long Island. _____ was his place. And I was very much afraid it would take a long time to explain to him what our concern is, but that wasn't so. In 15 minutes or perhaps 20 minutes, if I want to be general. . .

CREUTZ: A short time.

WIGNER: In a very short time, much shorter than any of us had expected. He understood it and was ready to dictate a letter which should be submitted to the President. I wrote down the letter which he dictated, of course in German, and translated it in Princeton into English, had it typed. And then I think it was Teller and Szilard who took the letter to him, and he signed it, and it got to [President Franklin Delano] Roosevelt.

WEINBERG: There's a point there, because you often see in some of the popular histories that that letter, in fact, was composed by Szilard, but Professor Wigner points out that it was really Einstein who composed it.

WIGNER: Einstein dictated it.

GOLDBERG: This is the second time I've heard him say this.

WIGNER: Szilard virtually was quiet during all this time, because I think I could explain it to Einstein better.

WEINBERG: You knew him better than Szilard could.

WIGNER: I knew him so much better than Szilard.

GOLDBERG: Szilard doesn't have a reputation for being quiet ever, you know.

WIGNER: Well, you see, he was not really, at that time, any more a good physicist, but he was a good politician, and he foresaw dangers and difficulties very clearly. But anyway, Einstein and Roosevelt had organized a meeting at the Bureau of Standards, and we got there and explained--I think it was again me who explained what our concern is. Of course, it was a joint concern, at least. I think Szilard was, in fact, perhaps more interested in the matter than I was. And it was accepted, and soon enough we got the invitation to go to [The University of] Chicago and work on the nuclear chain reaction.

GOLDBERG: Before that, there was a lot of work that was being done at Columbia and Princeton, all this work.

WIGNER: Oh, yes. Oh, yes. As a matter of fact, we were convinced that a chain reaction is possible with a mixture of uranium--well, with a composite of uranium and carbon. And this turned out to be, in fact, true. And we were so convinced that at Princeton, we realized that it will be Fermi who will create it, the chain reaction, first. So I bought some material that could be presented to Fermi, who produces the chain reaction. I took it to Chicago and, indeed, when the chain reaction was produced, I presented it to him, the present which was bought in Princeton already.

WEINBERG: I think you had the bottle, didn't you, Al?

WATTENBERG: I was there, cleaned up afterwards and hung the bottle on the wall, and also, when we moved from the site, I moved it with us.

GOLDBERG: Let's be specific about this. We're talking about a bottle of Chianti, right?

WIGNER: Right!

GOLDBERG: Where's the bottle now?

WATTENBERG: It's at Argonne. It is at the Argonne National Laboratory now.

WIGNER: Still?

WATTENBERG: Yes.

CREUTZ: You all autographed it, didn't you?

WATTENBERG: Everyone except Eugene Wigner. He didn't sign the bottle.

WIGNER: Oh, is that right? [Laughter]

00:34:24:00

GOLDBERG: Let's go back a little bit, because that's much later. One of the interesting things about this to me, looking back on it and not having been part of it, is that that didn't happen until the end of 1942. By that time, we had decided to commit vast sums to this project, and yet long before there was any self-sustaining reaction.

WATTENBERG: I wanted to ask Ed something. In early 1942, I remember Wigner coming over to Columbia, and I remember Teller joining us, even doing some laboratory work, believe it or not. [Laughter] And he was a good chemist. All right? But I don't remember the experimentalists from Princeton coming over.

CREUTZ: Bob Wilson and myself, I believe.

WATTENBERG: Did you come over to Columbia some of the time?

CREUTZ: We came over about every two weeks, as I recall.

WATTENBERG: You did. Okay.

CREUTZ: About every two weeks, we'd come to Columbia, and then alternately, Columbia would come to Princeton. This started soon after the law that prevented us from speaking to each other. [Laughter] We sort of wrote the law. It wasn't a law, but it was a request.

00:35:38:00

GOLDBERG: Let's talk about that for a minute, because that's another important consideration. One of the people that's very important in that development was Gregory Breit, the physicist from Wisconsin, who then went to Washington. Were any of you involved in the reference committee? The National Academy of Sciences had a reference committee on nuclear physics, in which they agreed to approach people who were publishing in what they considered critical areas, to withhold publication. This was before the war. Did any of you have any experience with that? I don't know.

WIGNER: I don't remember it at all.

WATTENBERG: Only what I've read, and I think the best write-up I ever saw was by Herb[ert Lawrence] Anderson in All In Our Time. Because it was Szilard who prevailed on him to use his thesis as an example of something being withheld from publication. So that then Szilard and others could write to ask the Physical Review editors to say that others had withheld publication. So Herb Anderson writes this up, if you want a reference on it.

GOLDBERG: But Breit was, I think, my impression, at least looking back on it now, more important than most people normally realize. That is, he was at Wisconsin. Gregory Breit was at Wisconsin before he moved to Washington, but he moved to Washington. And so he was at headquarters with people in the National Bureau of Standards. And it's my impression, anyhow, that he had a lot to do with, on the one hand, keeping the program going, and on the other hand, instituting what was called compartmentalization or putting people in cubicles.

In fact, there are some correspondence that I've read in which he prevented you, Wigner, from talking to Fermi directly. That is, if you wanted to talk to Fermi, you had to talk to Breit, and then Breit would talk to Fermi, and then Fermi would talk to Breit, and then Breit would talk to Wigner. Do you remember that?

WIGNER: No, I don't.

CREUTZ: I remember that.

WIGNER: Good!

CREUTZ: Because you used to complain a lot.

WIGNER: Well, you see, one forgets things.

- CREUTZ: Well, that's why we set up the arrangement with Fermi, that we would come up there every weekend, every other weekend, and their group would come to Princeton on alternate weeks.
- GOLDBERG: That was a way of getting around this compartmentalization?
- CREUTZ: I'm afraid it was.
- WEINBERG: And which year was that, Ed?
- CREUTZ: This would have been in the end of '41 and early '42.
- WIGNER: When did we move to Chicago?
- CREUTZ: Early '42. You left around February. I left in June.
- WIGNER: Yes. But February of which year?
- CREUTZ: 1942.
- WIGNER: 1942. I see. I should remember that.
- BORST: When was Breit in Chicago?
- GOLDBERG: Breit moved from Washington to Chicago in January of 1942, as a theoretician in, I guess, the nuclear theory group.
- WEINBERG: He played, as far as I could tell, a very central role in classifications, and whenever we would have a meeting of the staff he would get up and give us a long lecture on how secret this has to be. And I remember how he would say, "Whoever has the bomb

controls the world." I don't know if you remember that Al, but Gregory was always in everybody's hair about it. I think he finally left the project because of fights over that.

BORST: That's right.

WATTENBERG: That's right. [Arthur Holly] Compton felt we couldn't distinguish between the fast neutron bomb research and the slow neutron research, and he and Compton just disagreed.

GOLDBERG: And so he left in about June of 1942.

CREUTZ: I want to say a very good word for Breit. Breit was my thesis advisor at Wisconsin, and he was a great man. He was extremely helpful to students.

WEINBERG: Yes.

WIGNER: Yes.

CREUTZ: I have a very fond feeling for him.

WATTENBERG: Did he make contributions to the experiment, or did you do a theoretical thesis?

CREUTZ: He made no contributions, except he helped me find a leak in the vacuum once.

WEINBERG: He was one of the first to calculate the critical mass of pure uranium, it's my impression.

00;40:24:00

GOLDBERG: Yes. While we're back in these early days, one of the other striking things that's in the archival material is

referred to as "The Beryllium Program". [Laughter] It seems to me that early on there was an investigation to see if one couldn't use beryllium rather than carbon as a moderator.

CREUTZ: Art[hur Hawley] Snell, at the University of Chicago, using the cyclotron there, did have a quantity--I don't remember how much--of beryllium, and was looking for multiplication in it, at Szilard's suggestion.

WEINBERG: Well, there also was another person who's not mentioned very much in those early days, and that was [Alfred] Carl Eckert [Jr.], from Chicago, and he was my professor, one of my professors. And I remember it was the fall of 1941, before Pearl Harbor that he asked me to come work with him half-time, at which time, after six months, we would show that the thing wouldn't work. And he was then making calculations quite independently of everybody else, I believe, on the conditions for the establishment of a chain reaction.

WIGNER: Establishment.

WEINBERG: Yes. And one of the moderators, as I recall--I'm a little hazy about that--that was at issue was beryllium. I know that Sam[uel King] Allison, along with Art Snell, and Arthur Compton, I think, were very interested in the use of beryllium. But it is true that Carl Eckert worked out much of the theory quite independently of everybody else. I think that's more or less correct. And another pair that worked out the theory--I've never seen their papers in great detail, or I haven't examined them in great detail--was Jim [James Brown] Fisk and Bill [William] Schöckley.

GOLDBERG: Jim Fisk who was president of Bell Labs?

WEINBERG: Bell Labs, yes. They also made independent--but I think it's fair to say that Eugene's formulation of the

theory was the dominant formulation, although, of course, Fermi had his own private formulation. Fermi always did things privately, by himself and independently. And then, of course, the Germans did it, the Russians did it, the British did it. So--I think Fermi once said that--no offense, Eugene-- he said that any intelligent 14-year-old could work this out. [Laughter]

WIGNER: Yes, that is correct.

WEINBERG: I think he even said. . . .

GOLDBERG: Yes. Didn't he also say, "Never do a calculation until you know what the answer is"?

WEINBERG: Al knew him better than I.

GOLDBERG: That's attributed to him, isn't it?

WATTENBERG: He knew how to make quick estimates. [Laughter] In regard to the beryllium, we got enough beryllium in the summer or fall of 1942 to make a sandwich inside a graphite pile that was about four feet by four feet by about a foot thick.

GOLDBERG: This was in Chicago?

WATTENBERG: Yes. So we actually, at the request of the theoretical group, did a diffusion length calculation in beryllium.

CREUTZ: That would be a ton and a half.

WATTENBERG: Thank you.

CREUTZ: A ton and a half, about.

GOLDBERG: You also worked in beryllium.

00:44:00:00

CREUTZ: Well, Szilard got me interested in beryllium at Chicago in 1942, and the only beryllium available then--made by Brush Beryllium Company in Cleveland--was extremely brittle and very coarse-grained. The grains were as much as five or six millimeters across. So it could not be worked. If you dropped it on the floor, it would break like glass. So we tried methods of trying to improve the ductility and the workability, the machinability of beryllium, and we were able to improve it some by extruding it at Wolverine Tube Division of Calumet and Heckler Copper Company, Detroit, Michigan. [Laughter] It was kind of interesting because, first of all, Battelle tried to extrude beryllium.

WIGNER: Who?

CREUTZ: Battelle, in Columbus, Ohio. And it is very hard, and so they kept raising the temperature of the assembly, and then they put on the pressure, and the die itself extruded, but the beryllium didn't-- the dye being an object of the hole of which you're supposed to extrude things through. [Laughter] So we decided--this was Dave [David Harris] Gurinski and I at Chicago--that the secret was in getting a good lubricant. Of course, in extruding ordinary metals like copper and steel, you use oil for a lubricant, because you extrude those at much lower temperatures. But we felt you had to extrude near the melting point, say, about 1000 degrees centigrade with beryllium. And there aren't too many good lubricants at that temperature. [Laughter] But we decided there were three possibilities. One was liquid magnesium, which does not form any alloys of beryllium, and also kind of unpleasant to handle in the air, since it's explosive. But we did extrude beryllium with liquid magnesium as a lubricant. It's far too dangerous, though. So then we went to graphite, which sounds obvious. So we made a graphite container, perhaps two millimeters thick, put the

beryllium in that, and extruded that. Graphite would of course, crumble and serve as a lubricant. But we finally ended up using copper, which was most convenient.

Later on, Wally [Henry] Zinn built a reactor using beryllium for a cladding material for the uranium, and it looked pretty good, because beryllium does not absorb slow neutrons. The difficulty was that in the extrusion process, small amounts of copper impurities became imbedded in the beryllium, and when you have two dissimilar metals in a conducting hood, you produce a galvanic cell and increase the corrosion. So the beryllium corroded badly. So that was, I think, the last attempt to use beryllium in a reactor as such.

However, the beryllium experience and the experience with Brush led very inventive Szilard to think of another way of producing uranium--Brush made beryllium by producing beryllium chloride with magnesium. And so Szilard said, "Why don't we do the same with uranium chloride or fluoride?" So we designed some crucibles of the same type we were using for melting uranium, and the melting was done in an argon atmosphere to prevent oxidation of the uranium. So we went up to Brush and sure enough, they heated up the crucible, melted the uranium tetrafluoride, and started dropping in chunks of magnesium, and it worked fine. Each time they dropped in a chunk, it'd get a little hotter because of the reaction heat. And finally, it reached 1100 centigrade, where magnesium boils, and the whole thing blew up. Well, Szilard was sitting like Dale sitting here next to me. Right after the explosion--nobody got hurt--but Szilard wasn't there.

WIGNER: What?

CREUTZ: Szilard was no longer in his chair.

WIGNER: I see. [Laughter]

CREUTZ: My first thought was: "He got blown away by the explosion", but he spoke to us. He was outside the

door, looking in. So he had jumped in essentially zero time from the chair to outside. But that did become the preferred method of reducing uranium salts to metal.

WATTENBERG: I want to say one thing--that it's lucky that beryllium didn't work, in a sense.

CREUTZ: Yes

WATTENBERG: You know, we were working with many non-normal or abnormal substances, and it could have been that there would have been a major toxicological effect from them, and we were very lucky that it was graphite and uranium oxide, which there are really no human problems with handling them. The beryllium, if it had worked, we all probably would have had berylliosis, and many of us might have died from it.

GOLDBERG: Very toxic.

WATTENBERG: It turned out to be a very bad substance. Pardon me?

WEINBERG: Herb did come down with berylliosis.

WATTENBERG: Herb came down with it, yes.

WEINBERG: He was cured, apparently.

WATTENBERG: Well, he's on oxygen. He walks around with oxygen tanks on his back. Herb, of course, was mixing the beryllium powder for radium beryllium sources, and that's how he got--he breathed in all the beryllium.

WIGNER: May I make a very unscientific remark? Much of what I heard now was new to me. I may have known it at one

time, but I have forgotten it. Do you take a record of what we are discussing?

GOLDBERG: Oh, yes.

WIGNER: And could you have that without abbreviation, printed and distributed? Because it would be interesting to us to read it again, what we could have learned.

GOLDBERG: One of the things we do with the record, with the video record, is to make a transcript of the proceedings, so we get all the audio onto paper. Very important, you're right.

WIGNER: We won't publish everything, but for us it would be nice to be able to read it again.

00:50:09:00

GOLDBERG: Absolutely. But I want to go back to something that you mentioned before. You mentioned that Szilard had used a radium source for some of this. Was it for a source of neutrons?

CREUTZ: Yes.

GOLDBERG: Well, you know, he always had terrible trouble with administrations, and he didn't have clearance. Nobody had approved his doing this, and he went out with his own money and, I think, maybe with some of your money and rented a radium source. Later on, when things got a little more formalized and it was found out, there is a letter in The Archives insisting that the Manhattan Project pay Szilard back \$2,000 that he had spent out of his own money to rent that radium source.

CREUTZ: I hope he got it.

GOLDBERG: He got it. He got it.

00:51:02:00

BORST: I wanted to revert again to early days. D₂O was recognized to be a very good moderator.

GOLDBERG: That's heavy water?

BORST: Yes. Halban in Paris apparently got some gallons of D₂O, how or where or when. Now, when was D₂O recognized as an excellent moderator? It must have been very early.

CREUTZ: It was very early. It was thought to be just too expensive in the early days.

WEINBERG: Well, it was. Yes.

BORST: Only we didn't know how high the costs would go on that project. [Laughter]

WIGNER: There wasn't a sufficient amount of D₂O present to use it, and that's why it was concentrated on carbon, and that had some good advantages.

WATTENBERG: The complete Norwegian production got shipped to Paris, and I think Joliot-Curie had connections with people in Norway. In fact, in the end, they didn't even charge them for it; they were glad they got rid of it, got it out before the Germans came. And Joliot was worried about the Germans getting it, and that was partly why it got down to Paris. And they brought that to England, incidentally. [Lou] Kowalski and [Hans von] Halban got it.

BORST: The claim is that it was rowed across the English Channel in a rowboat. [Laughter] Now, I have never heard a certification of that, but it makes a very good story.

GOLDBERG: This is one of the things that slowed the Germans up, because they were convinced they couldn't use carbon, and so they were relying on heavy water.

WIGNER: Is that so? I didn't know that. The Germans were convinced that carbon is no good?

GOLDBERG: Right.

WIGNER: I didn't know that.

WATTENBERG: For all of the secrecy and compartmentalization, somehow or other though the British Army got instructions to destroy the Norwegian plant. I have never heard or seen that little bit of history.

GOLDBERG: I can say a little bit about it. I forget now when it was but, in fact, there was a commando raid, I think in 1943. There was a commando raid which essentially destroyed their ability to produce heavy water in that plant. And what frightened people in the administration of the Manhattan Project was that the Germans gave it top priority to repair it, and within a month, they were producing heavy water again. And so that was one of the reasons that people, like Groves, for example, and [Vannevar] Bush and [James Bryant] Conant were convinced that the Germans were probably working hard on a device.

WIGNER: The Germans did work on it, and [Werner] Heisenberg, in particular, was involved. But he had no enthusiasm, because the way I understand it, he didn't want Germany

to conquer the earth, and altogether, it was neglected. And Hitler himself said, "Oh, we will have conquered the earth before nuclear energy will become effective," and there he was mistaken. But he was right that the war for Germany ended before nuclear energy was effective, because they had to give up.

00:54:39:00

WEINBERG: Perhaps I interject an incident that remains very clear in my mind, although it's more than 40 years old.

Around Christmastime of 1943, we had a meeting in Compton's office, and present at the meeting were Compton, Wigner, Fermi, and Alvin Weinberg. How did I happen to be there? Because I was one of his assistants at the time. Their discussion had to do with the following: when could we expect a German atomic bomb? And Wigner, who always looked at the matter in a pessimistic way--and I think partly because then if it turned out better than that, then he would feel good about it--went up to the board. And he said, well, it would take them three months to work out the details of how to make the reactor. They could build the reactor in three months, three months to run the reactor and extract the plutonium; three months to get the bomb; and by Christmas of 1944, we could expect the German atomic bomb. Now, the mistake he made was that he assumed that every person on the German project was a Eugene P. Wigner, and that was a serious mistake. But I remember that incident very, very clearly.

WIGNER: I don't remember this at all.

WEINBERG: I remember it so clearly.

GOLDBERG: Did you evacuate Chicago? [Laughter]

WEINBERG: Well, I don't know what Compton did with that information, but maybe somewhere he may have written.

GOLDBERG: It seems to me very clear, that everyone was frightened to death that the Germans would get this.

WEINBERG: Terribly. Terribly. Yes.

CREUTZ: In fact, about the same time as that meeting, General Rommel [Field Marshall Erwin Rommel] was running his tanks through North Africa. You remember that?

WIGNER: No.

CREUTZ: Well, you remembered it then, because you said he could not possibly get that far from his supply lines unless those tanks were run by atomic energy. You were very concerned.

WIGNER: [Laughter]

00:56:54:00

GOLDBERG: Let's go back. I want to take a break in a few minutes, but let's go back and pick up one other strand of the story, which, I guess, happened in the middle of 1941, and that was the realization that one could use Uranium-238 and transform it into something--although it itself wasn't fissionable--transform it into something that was fissionable, what became plutonium, but at that time what was referred to as "Element 94."

WEINBERG: Element 9, we called it.

GOLDBERG: That was later. I think when you started, you called it copper. I don't know if you remember that.

CREUTZ: Copper and nickel.

- GOLDBERG: Copper was supposed to be plutonium, and magnesium was supposed to be Uranium-235. I get very confused in that code. I wonder if you. . .
- BORST: We all were.
- WATTENBERG: Things were easier later.
- CREUTZ: The person, I believe, who first thought about Element 94 was Lou [Louis Alexander] Turner at Princeton University.
- [Wigner pats Cruetz on the back]
- WIGNER: Right! Right! Right!
- CREUTZ: And, of course, none existed so far as we know in nature. He proposed this would be fissionable and could be produced by capturing neutrons in Uranium-238.
- GOLDBERG: Why did he think it was fissionable?
- CREUTZ: Well, there's a good reason for that in nuclear physics.
- BORST: You see, there are four radioactive series: one starts with U_{238} ; one starts with U_{235} ; a third one, is thorium, and the fourth one is missing. Now, if 235 is fissionable, then one in this fourth series ought to be, because they have the same kind of nuclei. So it was perfectly predictable but, of course, it had to be checked. And that was done at Berkeley.
- CREUTZ: There's a general theory in nuclear physics that the odd number, like 235, 239, are more likely to be fissionable than the even numbers, like 238, 240.

WEINBERG: Actually, the idea of this was in the original Bohr-Wheeler paper, the Bohr-Wheeler theory of fission. Because they pointed out that if you had an odd atomic weight and an even atomic number, then it would tend to be fissionable. But it was Lou Turner who--although, again, there was independent discovery, because in Germany, they had the same idea, but never did much with it. I've forgotten who it was in Germany that. . .

WIGNER: Heisenberg was supposed to work on it, but he was very unenthusiastic, I think partly because he did not want Germany to conquer the earth.

WEINBERG: Gosh, I guess I've forgotten who it was in Germany. I think it may have been [Carl F. von] Weizacker who made the same proposal in Germany.

GOLDBERG: That 94 would be. . .

WEINBERG: Yes. Never taken terribly seriously there.

WATTENBERG: He always pushed a very statistical theory. Did he really get into understanding the odd/even difference?

WEINBERG: This is just the story that I get from my German colleagues, as I recall it.

WATTENBERG: He certainly was their theorist.

BORST: Odd/even, of course, was understood very early, the difference between different families of isotopes.

01:00:25:00

WEINBERG: Al, it seems to me that we've sort of missed the period from '39 to the end of '41 at Columbia.

WATTENBERG: I only read about it. I didn't join 'til '42. I joined in January '42.

WEINBERG: So you were still in the liquor business.

WATTENBERG: I was still in the liquor business. No, it was only what Bernie [Bernard Taub] Feld and Herb[ert] Anderson may have told me, and I think I'm more influenced by what I've read than what I remember from conversations. There was a lot of carry-over. They were well along by the time I joined them in building these exponential piles. They had developed this.

WEINBERG: They had built--what?--about six or seven by that time?

WATTENBERG: Yes, it was at least that many.

GOLDBERG: Why are they called "exponential piles?"

WATTENBERG: Oh, because the thing is that as you put a source or something at the bottom of a column, it'll decay away exponentially. There's a half-distance.

WEINBERG: If it doesn't, you have a chain reaction. [Laughter]

BORST: And you were measuring how much that was modified from just a pure exponential, by the fact that there was some production along the column.

WIGNER: I didn't remember that.

WATTENBERG: It was a very precise technique. They could make precise measurements, and they had first done it with these big cans of uranium oxide. And then by the time I joined them they were just beginning to press the oxide. That's when I came.

WEINBERG: I think the exponential experiments, as I understand it, were Fermi's invention. It was his idea.

WIGNER: Yes.

CREUTZ: Szilard was very active in that and was responsible, I think, for getting the large shipments of graphite.

WEINBERG: Right. But I think the basic idea was probably Fermi's.

CREUTZ: Probably.

WEINBERG: It's interesting. The Germans did not do exponential experiments. They had a different technique which actually was not as easy to carry out.

WIGNER: What was it?

WEINBERG: What they did, actually, was they had the source, and they measured the total multiplication. There were multiplication measurements that they made, which were not as easy to interpret. I don't know. But these were not easy experiments, by the way. You had to take small differences of large numbers in order to get meaningful results.

CREUTZ: Like a bathtub experiment.

WEINBERG: Yes.

WATTENBERG: There's one thing I wanted to say that isn't--and I guess I'll be saying it several times--that Fermi had very strict rituals.

WIGNER: What did Fermi have?

01:03:28:00

WATTENBERG: Fermi had very strict rituals on making measurements.

WIGNER: Yes.

WATTENBERG: And you first checked that the instrumentation was doing the same thing with the standard. I think you probably had the same thing. He then checked what the background would be of the instrumentation when you had no source. Then you would put in your sample, and you'd do two or three samples. And then you'd go back to measuring the standard and the background again, and there was always this cycle. And never without continuously checking that you knew what the instrumentation was doing. It really was a continuous check, not only of the effect, but of the fact that the instrumentation was functioning correctly.

BORST: The beauty of this was that he staffed his laboratory largely with people from the steel mills, who perhaps could read, but it really didn't matter whether they could or not because he had this ritual, and they watched the clock, and they put the indium foils in at a certain second and took it out after 60 seconds, or whatever it is--ran 55 seconds to the counter. At the 60th second you started counting, and he had it all absolutely rigorously planned so that any four-year-old could do it. And the results were, of course, that he got very reliable data, and he was able to take these small differences that Al told about seriously.

GOLDBERG: What was the indium used for?

WATTENBERG: Indium was the one that Lyle [Borst] was referring to as a rodium foil. Rodium is terribly expensive; it's like platinum. So it was very expensive to have rodium foil. When we wanted to do a lot of measurements, the indium was cheaper, and there was a more relaxed schedule. Indium was about 53 minutes half-life or something like that. And, so you could leave it in for five or ten minutes, and you didn't have to quite run so fast down the hallway with it, because you were using a radium beryllium source in the exponential piles as the source of neutrons near the bottom. So the instrumentation had to be quite far away so that it wasn't affected by this radium. So usually you were going 50, 100 feet carrying the stuff.

CREUTZ: Can I say something about the instrumentation we all used in those days? You didn't order a Geiger counter or something; you made your Geiger counters. There were no such things as scintillation detectors or all the fancy gadgets we have now. So you had to have a good glass blower, and he would make these Geiger counters for you, with a very thin wall, perhaps 10,000ths of an inch thick to let the electrons in. And you built your own electronics, your amplifier and your high-power supply and so forth, and these didn't always work so, as Al says, you continuously checked your equipment before and after any measurement you hoped would be meaningful. So that took a lot of time.

BORST: Then the results were taken to Al Weinberg, who cranked his computer--not any fancy computers like we have now--and figured out what it meant.

WEINBERG: No, no, you're incorrect. Then I would present it to Professor Wigner, who would explain what it meant.
[Laughter]

GOLDBERG: Let's take a break, and when we come back, we'll talk about plutonium again and why then the pile became so much more important.

01:07:16:00 [Pause in interview]
[Begin U-Matic Tape 2 of 4]

01:35:06:00 [Interview resumes]

GOLDBERG: Let's begin again. Before we go on, I think I'd like to go on and talk about Chicago and the Manhattan Project. In the break, I was talking to Dale Babcock about heavy water. You worked at heavy water at DuPont.

BABCOCK: Oh, yes.

GOLDBERG: Why don't you tell us a little bit about that.

BABCOCK: Well, how long do I have?

GOLDBERG: As long as you want.

BABCOCK: I'll try to be brief. My experience with heavy water started with the conversation with [Harold Clayton] Urey shortly after war was declared, and I had had experience down at our plant in West Virginia, where we were trying to get pure hydrogen from coke oven gas. What I did was turn the liquid air plant into a hydrogen plant in order to get reasonably pure hydrogen. And Urey knew about that. In fact, we were making hydrogen at the scale of--oh, about half a million cubic feet a day by this process, so it was a fairly good-sized plant. And he wanted to know if that would be a way to get heavy hydrogen or heavy water. I brashly said, "Why sure, it doesn't take very much change. It would probably cost \$20 million or something like that."

And Urey said, "Oh, I'd like to talk to you about it so who should you meet?"

And I said, "Now then, wait a minute. You start in with the president of the DuPont Company, and you then ask to see me. That's the only way you can do it." Which he did, and eventually got down to me.

And, I was given the job of looking at it, and I spent a couple of weeks at it. The time schedule to do it was just way beyond what Urey was talking about. We had a conference in Wilmington between Urey and Conant and Crawford [Hallock] Greenewalt and I, and I laid the thing out on the table.

WEINBERG: This would be 1942, was it?

BABCOCK: This would be 1942, yes.

WEINBERG: The beginning of the year?

BABCOCK: Yes. I'm going to say, oh, April or something like that.

WIGNER: Wasn't it 1942 that we moved to Chicago?

BORST: Yes. Just after Pearl Harbor.

BABCOCK: Yes, just after Pearl Harbor. In fact, I had an assignment with NDRC for a while. Well, I asked to look at other ways of doing it that would be less using up of equipment and whatnot, because there was an awful lot of new equipment that just plain had to be designed and built and that takes time. So I studied the method of making it with distilling water, and I came up with the idea that it could be done relatively easily. The oil company distills oil in great big stills, and the technique for making large stills was all worked out, and that part of it would be very easy. I further came up with a

number of about 200,000 pounds of steam was required to make a pound of heavy water.

Well, now then, somewhat on the aside, some time later I saw some transcripts of the German work, and you were speaking about the heavy water coming to them from Norway. The Germans had looked over exactly this same route that I went down, and they finally said the way to do it was to distill water but it took 200,000 pounds of steam to make a pound of heavy water, and they couldn't afford it. Whereas we said, "What? Only 200,000 pounds of steam?"

WIGNER: The amount of heavy water in ordinary water is one in 300,000?

WEINBERG: No, it's one in 5,000.

BABCOCK: Near one in seven, but that's all right. But when you distill water, you have to have a pretty high reflex ratio in order to get it. The figure I gave isn't quite right, but it's pretty close.

GOLDBERG: But we then went ahead, is that what you're saying?

BABCOCK: Yes. We went ahead. Actually, the estimate for the cost of the plant, which I'd given, was \$20 million, and the DuPont engineers had upped my figure to \$30 million before they started on it. They built it for \$17 million. I think it's the only time in my life I have ever underestimated the cost of a project. Well, we built the plants and made about 30 tons of heavy water for the Canadians.

WEINBERG: Where was the plant built?

BABCOCK: We had three plants. One of them was down in Alabama at a DuPont explosives plant; another one was

out in Indiana--Dana, Indiana; and the third one was at a DuPont plant in West Virginia--Morgantown, West Virginia.

WEINBERG: When you built the plant, what did the ordinary workers there think you were doing? I mean, was there any discussion of what it was all about, do you remember?

BABCOCK: I wasn't even around when they built them.

WEINBERG: So you never saw the plants actually?

BABCOCK: Oh, yes, I saw them, but I wasn't very close to them.

WEINBERG: How about the plant manager? He knew what was going on?

BABCOCK: Well, yes, but--see at that time--this wasn't too secretive.

GOLDBERG: You could build a heavy water plant and say you're making heavy water, but not say why you're making heavy water.

WEINBERG: Hmm.

BABCOCK: Yes, that's right. [Laughter] But anyway, we made the heavy water, and after we got 30 tons or thereabouts, they were shut down.

GOLDBERG: Was this made under the auspices of the Manhattan Project, or was this separate?

WEINBERG: It must have been Manhattan Project.

WATTENBERG: It was for the Canadians, though.

BORST: No, that's true.

BABCOCK: It was for the Canadians. The Canadians had requested it from the United States Government, and the United States Government said, "Yes, we can do it."

WATTENBERG: But that was later. We built the heavy water reactor, started in 1943, because we knew of the production.

BABCOCK: That heavy water came from the heavy water that I'm speaking about.

WATTENBERG: And that was less than that amount you're talking about.

BABCOCK: Well, the 20-odd tons went to the Canadians, and we made 30-odd tons.

01:42:31:00

GOLDBERG: Let's turn now to the period in late 1941, early 1942, when it was decided to open the Metallurgical Laboratory in Chicago. I think almost everyone here ended up in Chicago. Did you?

BABCOCK: Well, I made frequent trips out there.

GOLDBERG: But you worked out of Wilmington.

BABCOCK: I always worked there.

GOLDBERG: Let's begin to look at that period.

BORST: Al and I were students at the University [of Chicago].
I was working on the cyclotron on experimental problems, and Al was, of course, working on theoretical problems.

WEINBERG: I was a biophysicist. I didn't know anything about anything!

WATTENBERG: You knew about diffusion.

WEINBERG: I knew about diffusion. And that's how I happened to get totally by mistake involved in the thing, plus the fact that one of my professors was Carl Eckert, who was very much involved on his own on these things.

GOLDBERG: What role did diffusion play in the biology you were doing?

WEINBERG: Well, I was a student of a man by the name of Nicholas Rashevsky, who was trying to create a mathematical theory, a mathematical analysis of biological processes. And he would visualize a cell as consisting of a spherical object into which were diffusing metabolites, and out of which were diffusing waste products. And you could say quite a few quite interesting things about cells on that simple basis.

Now, the experiments that Ed Creutz did, he had a sphere of uranium, and neutrons were diffusing in. So you see, there was a very close analogy, really, between the diffusion into a cell and diffusion into uranium.

It's quite interesting that in 1939, shortly after the theory of fission had been proposed by Bohr and Wheeler, a man by the name of Gale Young, who should be here today, but he wasn't able. . .

WIGNER: What is the trouble with Gale?

WEINBERG: I don't know, I haven't seen him in quite a while. But he was also a student of Rashevsky's in mathematical biophysics, and he pointed out that there is a remarkable analogy between Rashevsky's theory of the fission of a cell under these so-called diffusion forces and the theory of Bohr and [John Archibald] Wheeler. And in fact, you know, had Gale Young known that they had something called the liquid drop model of nuclei, where the fission occurs because there is a tension between the surface tension pulling it in and the cooling forces pulling it out, Gale Young conceivably could have predicted fission. But he didn't, so he wrote this paper afterward. That's a little bit of an aside. The paper is in the Physical Review as a letter to the editor. That's how a biophysicist happened to be waylaid into this business for a whole lifetime.

01:45:45:00

GOLDBERG: Now when did that happen? When did you actually start working on this?

WEINBERG: I started working around September of 1941, or it may have been June of 1941, and Carl Eckert asked me to help him in making some calculations on beryllium.

GOLDBERG: On beryllium?

WEINBERG: Yes.

GOLDBERG: Was Eckert then in contact with Compton?

WEINBERG: He was in contact mostly with Sam Allison, I believe, and Compton also, but I think Sam Allison was the director of the Chicago project at that time.

WIGNER: Isn't there a very significant difference that in the case of fission, what disintegrates has no further

effect of again disintegrating, whereas if you have a cell and it separates, that cell grows?

WEINBERG: You're absolutely right, Eugene, but all we were trying to do was figure out was: why did it split in the first place?

BORST: In the autumn of 1941, I think the real kickoff of the project was Compton got together all the people from [The University of California,] Berkeley, from Princeton, from Columbia, in Chicago during the Thanksgiving recess, and everybody sat down and everybody reported what they had done.

GOLDBERG: Right out loud?

BORST: Oh, yes. In one room, the seminar room.

BABCOCK: Was Urey there?

CREUTZ: I would expect so.

WEINBERG: I would think so.

WIGNER: I don't remember.

GOLDBERG: Well, the minutes say he was there.

WIGNER: I see.

BABCOCK: Were you there, Lyle?

BORST: Oh, yes.

WIGNER: Which year?

BORST: In the fall of '41.

WIGNER: Oh, that's so long ago! [Laughter]

BORST: I was a graduate student at that time. I was hiding behind the curtain so that nobody would kick me out. [Laughter] But there was a report from each of the centers on the status of the understanding of the fission process, and particularly the question of how many neutrons and so on. Then, of course, a few weeks later, Pearl Harbor. Of course, then we took off with jet propulsion.

GOLDBERG: So after Pearl Harbor, things increased by several orders of magnitude?

BORST: Yes.

01:48:06:00

WEINBERG: I think there was a fair amount of discussion, which I read about, as to where the project should be centralized. And I think it was Compton who said, "Let's move to Chicago," because he came from Chicago.

WIGNER: Born in Chicago.

BORST: Yes.

GOLDBERG: Lawrence wanted it in Berkeley.

WATTENBERG: When Fermi hired me in January, he thought he was going to stay in New York at that time. I know there was a series of meetings in which you and Fermi went to Chicago to discuss, and apparently it took a while--I think it took Compton being sick at the time to finally put his foot down.

- CREUTZ: There was also concern that the east coast was too close to Germany.
- WEINBERG: Yes, that's right.
- WATTENBERG: And Columbia had three other war projects. Columbia was willing to grab as many as possible, but they already had three. So. . .
- BORST: I know there was a story in Chicago about a financier who congratulated Bob [Robert Maynard] Hutchins on his great war project, and Hutchins said, "Oh, do we have a war project?"
- He said, "Of course you do. Your budget was \$4 million last year, and it's \$24 million this year." [Laughter] So, follow the dollars.
- GOLDBERG: Well, it quickly became a case that money was no object.
- BORST: Oh, yes. When Pearl Harbor occurred, why, then the spigot was open and the money came.
- GOLDBERG: You were already in Chicago?
- WEINBERG: No. What happened was Wigner would commute to Chicago for about two months or so before he settled there, and I remember, again, as clearly as though it were yesterday, this was probably January of 1942. And Wigner had an office in Eckert Hall, it had been assigned to him, and he would come. And then he would invite all of the young theory people, of whom I was one, to sit down with him, and he would tell them what they were doing wrong. I remember the very first time Alvin Weinberg met Eugene P. Wigner. It

was one of these evening sessions. It was about 8:00 o'clock. He was sitting there.

WIGNER: What year was this?

WEINBERG: This was 1942, only 45 years ago, Eugene.

WIGNER: Only! [Laughter]

WEINBERG: Of course, I knew about Eugene Wigner because he was one of the most prominent theoretical physicists in the world, even by that time. So I sort of crept in with what I was up to, and I was making some calculations, really on the diffusion length of beryllium is what it was. And he looked at what I was doing, he went like this, said, "Very interesting," which means, "Not quite right." [Laughter]

BORST: We understand.

WIGNER: Did I say that?

WEINBERG: Yes. And he showed me how to move in a, shall I say, more fruitful direction. That lasted for about two months, where we would see him, I think, perhaps two or three times after that. And then, of course, he came. I'll never forget that first meeting. [Laughter] See, I remember talking to Carl Eckert, and Carl Eckert said, "Well, I'm going out to San Diego to be in underwater sound."

I said, "My goodness, how is the project going to continue without you?"

He said, "Don't worry. Eugene Wigner will take over." He was right.

01:51:40:00

GOLDBERG: So now we have the situation set in which it's clear now that by this time, [Glenn Theodore] Seaborg and his people had confirmed the fact that [Element] 94 could be made.

BORST: That's right. It was confirmed by that time.

GOLDBERG: And it was fissionable?

BORST: Yes.

WEINBERG: Yes. But when I first joined the project, that fact still was sufficiently compartmentalized so that in fact, I didn't quite know about that until, you know, a month or so later. That was considered a very high security issue.

WATTENBERG: I'm trying to recollect whether Art Snell didn't write up, being asked by Oppy--[Robert] Oppenheimer-- with some unobservable sample of plutonium and also 235, to do some experiments at the Chicago cyclotron in probably the summer or fall of 1942.

GOLDBERG: '42.

WATTENBERG: Yes.

WEINBERG: I think so. Well, Art Snell's problem was basically to determine whether you could make a bomb out of natural uranium, and much of his work was directed to that question, without enriching, because in those early days, it was not perfectly clear that you could not make a bomb just by piling up enough natural uranium.

WIGNER: Who had the first suggestion to use graphite to make a chain reaction with natural uranium?

WEINBERG: My recollection was that it was either Fermi or Szilard, Eugene, but I wasn't there at the time.

WIGNER: That's very possible.

WEINBERG: By the time I got to the project, graphite was in. Although you have to also remember that at that time, there was no chain reaction, and many of us who were not quite so centrally involved in this we thought that the whole thing was sort of crazy.

BORST: Oh, no! [Laughter]

WEINBERG: Yes, yes. And I did not have confidence until, really, March or April of 1942, that the whole thing was really--March, let us say. Then, of course, in May of 1942, they finally did the exponential experiment, which showed that the thing really would work. And again, I will never forget that meeting--and you probably remember this--when Arthur Compton--this was the meeting of the staff, and Arthur Compton got up and said, in effect, "Gentlemen, the multiplication constant is now greater than one."

WIGNER: The most?

WEINBERG: "The multiplication constant is now greater than one." And I don't know, somehow there was sort of a gasp or something. I'm overdramatizing it. You probably remember that incident. And we knew from then on that it was desperately serious.

WIGNER: To establish a chain reaction. What time was Fermi's?

WEINBERG: The establishment was December 2, 1942.

WIGNER: December second.

01;55:03:00

GOLDBERG: That's what I wanted to do, is get the order of events here straight. The Met[allurgical] Lab opened in early 1942. The project was turned over to the Army in May or June of 1942. Groves came on the scene in September of 1942, and the power went critical in December of 1942. And it was only after that that you began designing the Hanford. . .

WEINBERG: No, no. Wrong, wrong, wrong. That was the remarkable thing. This man--as a young man, I might say, in March of 1942--was already designing the Hanford reactor. Now, at the same time, there were other engineers at the laboratory, notably--well, Miles [Corrington] Leverett was one of them. Charlie--what was his name?--Cooper [Charles Burleigh Cooper]--no, there was another fellow who was Miles' associate. [Thomas] Moore was his name.

CREUTZ: Charles Cooper was on separation.

WEINBERG: Yes. No, they put forward the idea for a helium-cooled reactor.

BORST: Mae West.

WEINBERG: And the reason they chose helium was because helium doesn't absorb any neutrons and it's a good heat transfer medium. This guy said, "Look, the Germans will be in San Francisco by the time they get a helium-cooled reactor running," which probably was correct at that time.

CREUTZ: There was a reason. It would have taken the entire output of the compressor industry five years to produce the compressors needed.

WEINBERG: And he said, "We boil water in aluminum cans, in aluminum pots, all the time, so let us make a water-cooled reactor." And he, you know, is really not a physicist; he's a chemical engineer. And he always has liked to do chemical engineering. He was the only person on the project who had a complete command of engineering, or at least sufficient for these purposes because he likes to do engineering, still does, and at the same time, had a complete command of the nuclear physics. And therefore, he was able in his mind to realize that you could make the compromises on the physical side, on the multiplication constant, but they would not be fatal to making the whole thing work. And so it was that he singlehandedly turned the whole project around from the helium-cooled system, which, as Ed says--well, I don't know, it was a high-temperature thing also, Ed, and from what we know, over to a low-temperature water-cooled thing. And he began the design not later than March of 1942.

GOLDBERG: That was before criticality.

WATTENBERG: Well before.

WEINBERG: He knew criticality.

WIGNER: No, you knew it, too.

WEINBERG: Well, I was doubtful.

WIGNER: Everybody knew criticality.

GOLDBERG: That was also before DuPont came on the scene.

WEINBERG: Oh, yes. Oh, yes. Then, of course, what happened was that DuPont, in September, I believe, of 1942, was brought in by the Army to look at the different propositions. They decided in favor of the Wigner design, although Wigner was ably helped. He was helped by Ed Creutz. You see him here as a physicist. He's not really a physicist; he's really a metallurgist. He became a metallurgist. He invented uranium metallurgy.

GOLDBERG: We're going to talk about that.

WEINBERG: And there are people like Gale Young, who was Eugene's engineering assistant, and then Miles Leverett, and there was a fellow by the name of Boysevan, who was from Holland or Belgium, who was in the group. That resulted in the Hanford reactor.

CREUTZ: Also there was Lee Owing.

WEINBERG: Lee Owing, yes.

WIGNER: It is true that I was in charge of the design, but the design was made by Gale Young and Alvin Weinberg principally.

WATTENBERG: In early 1942, was the idea of Szilard of a bismuth-cooled pile aborted rather quickly?

WEINBERG: Well, Szilard took it seriously, and Eugene was very polite, and so he always would say, "Well, that's a good idea." But it was a terrible idea, as we realize now.

WIGNER: What was a terrible idea?

WEINBERG: Using bismuth as the coolant.

GOLDBERG: So it would have been liquid bismuth?

WEINBERG: Yes. Again, Szilard's idea was that bismuth doesn't absorb neutrons, but you see, Szilard did not have the control of the multiplication constant question as strongly as this guy did, and therefore he did not. . .

CREUTZ: Also, Eugene thought, as Al said, of the engineering problems. The problem of pumping a heavy material like bismuth was one thing that threw it out. Eugene was the first one to propose, so far as I know, "It would be nice to know the thermal conductivity of uranium; it would be nice to know if you can machine uranium; it would be nice to know if you could actually make tubes of uranium." Things of that sort.

GOLDBERG: We're going to get on to that in a few minutes, but I want to get this. . .

WIGNER: I think you exaggerate the desert of this particular. . .

WEINBERG: He is the first nuclear engineer.

BORST: That's right. Absolutely.

WIGNER: I don't think so.

WEINBERG: I mean, he makes off that he's just a physicist who won the Nobel Prize in physics, you know, but he's really an engineer.

BABCOCK: That came later. [Laughter]

GOLDBERG: But there was a lot of attention paid in that area to getting the pile on the west stands of the. . .

WEINBERG: Oh, yes. Oh, yes. Oh, yes. And there was very close collaboration always between Al Wattenberg's group--that's Fermi's group--and the Wigner group that was designing the reactor.

02:01:12:00

GOLDBERG: I have a very naive question to ask of this process. You said before that you had to get the graphite purer than, say, one part per million with regard to, for example, boron and other materials as well. Now, were you then being supplied graphite with that purity from the graphite companies?

BORST: No, no.

GOLDBERG: So how did you get it that pure?

CREUTZ: Again, as Al said, it was MacPherson at National Carbon who developed techniques for purifying it.

GOLDBERG: Where was it purified? Was it purified there?

CREUTZ: Oh, yes.

GOLDBERG: Once you got it to Chicago, then my picture of this from what I've read is you were sawing it up with bandsaws. Didn't that contaminate it?

WATTENBERG: No. I want to make clear: we did not have the best graphite at Columbia. All right. There was better graphite that had been shipped to Chicago. We shipped all of our stuff out to Chicago from Columbia.

GOLDBERG: Didn't they ship the Princeton stuff, the stuff that you had used?

CREUTZ: That was a small quantity, only a ton.

WATTENBERG: So it was really the combination of the better grade of graphite, which was not the best grade that came in later, which we should talk about, and that made the main reactor, made the exponential pile the first one which, if it were infinite, would have been a reproduction factor greater than one.

I'd like to say that it isn't until October that we get a reproduction factor, multiplication factor, that is sufficiently great so that it could be built with a finite amount of material that was being produced. So in October we knew for sure that the amount of material was going to be enough. So that was really a clincher for that.

WEINBERG: What we'd always do--Fermi and Wattenberg and Wally Zinn and Anderson and Leona Woods [Marshall] and Phil[ip] Morrison, actually was involved--would do the experiments, and they'd get a multiplication constant of, I don't know, .995. And then Fermi, on his side, and we on our side would look at the data, and we'd say, "Well, the graphite isn't all that pure. So if we purify the graphite, we'll get an extra 2%." The uranium was oxide; it wasn't metal. If we convert to metal, we know we get 2% from Ed's experiments. The uranium wasn't all that pure. When we purified we always were able to see that you could get a multiplication constant of about 104 or 105, which is what the Hanford reactors were.

CREUTZ: You say "always." It took a long time to get to that point.

WATTENBERG: But actually, another one of those one-percents was that we had built a big can and put an exponential pile

in a can at Columbia and had then pulled the air out of it, which contains nitrogen, which absorbs neutrons, and put in carbon dioxide. We knew there was another 1% we could get from substituting carbon dioxide for nitrogen.

02:04:39:00 [Weinberg indicates balloon cloth on the floor next to Goldberg's chair]

WEINBERG: You see that balloon cloth there. You see?

GOLDBERG: Let's show that.

BORST: Before we go that far, I would like to make some comments about the graphite. Because one of the decisive questions was to get the graphite pure enough. Now, the graphite that was being produced at that time was electrolytic graphite that went into the steel industry for electrodes in the steel industry. It was made of coke from. . .

WEINBERG: Petroleum.

BORST: No, no. That was the improved stuff. The original stuff was coke from Pittsburgh, you know, from coal, and a tar, which was also a by-product of the coke industry. Now, the great breakthrough was to go to petroleum coke and to petroleum tar, so you got rid of the vanadium and a lot of the boron that was present in the by-product coke from the steel industry. And after that, why, it was just a matter of cleaning up the edges, and we were in business.

02:05:55:00

WATTENBERG: Can I say one other thing that we've talked about privately? Not enough stress is put on the fact that the physicists don't make graphite; the physicists don't make uranium oxide, and we don't make the metal. It really was American industry that provided us with this material. I'm afraid a lot of people get lost along

the way who made a major contribution and made the whole thing possible.

GOLDBERG: I was going to mention some of them. One name I've run across is Alexander at the Metal Hydridos Company.

WEINBERG: Yes.

WATTENBERG: [Frank H.] Spedding.

WEINBERG: Spedding deserves tremendous credit.

BORST: Spedding, Frank Spedding.

GOLDBERG: At Iowa State.

WATTENBERG: Yes. I never was really sure who deserves the credit for the graphite.

BORST: Szilard.

WATTENBERG: No. The AGOT graphite.

BORST: There's a perfectly lovely story which may even be true, but it ought to be told anyway, is that Szilard came up to Niagara Falls, where the Atcheson Graphite Division was, and talked to them about ordering 500 tons of graphite, you know, their very best quality. But he said, "Unfortunately, graphite has one part per million of boron." And the executive said, "Well, is that good or bad?" [Laughter] And Szilard assured him that it was bad. Then the executive said, "Well, you go back to your laboratory, because we can sell all the graphite we can produce to the steel industry. You just go home and play with your apparatus."

So the next day, or a few days later, he got a letter from the White House in Washington, and thereafter there was good cooperation.

[Laughter]

GOLDBERG: I want to show this picture now. This is the famous painting. You have all seen this, I take it. Let me put it on the stand so that we can get a picture of it.

WATTENBERG: It reminds me of pictures of [George] Washington crossing the Delaware River. [Laughter]

02:08:06:00 [Goldberg hands Wigner a painting depicting the first testing of the pile]

GOLDBERG: And don't you think that somebody would have said, "Sit down, you damn fool, you're rocking the boat."

WATTENBERG: No, the point is that some of us wore lab coats. More likely, we had wool sweaters and wool shirts on. We were not dressed that formally with ties, except perhaps for Greenewalt and Compton.

GOLDBERG: So Greenewalt was from DuPont, and DuPont was already on the scene.

WATTENBERG: That day. We adjourned that afternoon.

BORST: He came out from Wilmington for the function.

WATTENBERG: No, he came out for a meeting, and the meeting conflicted with this test. And Compton brought him over after.

GOLDBERG: You were at the test?

WATTENBERG: Yes.

GOLDBERG: Were you at the test?

02:08:50:00 [Close-up of painting while Creutz helps Wigner identify the people present at the testing; Wigner hands the painting to Weinberg]

WEINBERG: No, no. What happened with me, they gave blue badges to the people to get in, and you got a number, and the number depended upon where you were in the hierarchy. I think I had blue badge number 55, and they chose the first 50. Which was kind of a mistake, because they probably should have had only the youngest people there so there would be more people. But these three guys I think were there.

CREUTZ: No, I was in Detroit.

GOLDBERG: Were you there, Mr. Babcock?

BABCOCK: No, no. No.

WATTENBERG: But he did operate that pile. He came about two weeks later.

BABCOCK: I think I was probably operator number ten of the reactors in the world.

CREUTZ: I remember feeling rather sorry that I wasn't there, and why couldn't they wait two days 'til I got back. No, that's not the way the thing went.

02:09:54:00 [Weinberg passes painting to Wattenberg]
WATTENBERG: There were several people who really had helped us build it even who weren't there, like Bernie Feld and John Marshall.

BORST: You don't hold up progress.

GOLDBERG: Who did the painting?

BORST: Who knows?

WATTENBERG: It says Gary Deeman or Deerman. Anyway, his vision of people in coats and jackets is not right.

GOLDBERG: But it's a good rendering of the pile?

02;10:15:00 [Goldberg places painting on easel]
WATTENBERG: The opening was at the other end, but you have to do it that way to see the effect. Yes. And it shows the step-back layers and the wood underneath and the thing down below. There actually was more. . .

02:10:30:00 [Goldberg indicates a figure in an elevator at the top of the painting]
BORST: That's [Horace Van Norman] Hilberry.

WATTENBERG: That's an elevator with three of us standing on there. In the morning, there was another group over there. I don't know who's on there. In the afternoon, I think Lorenhyer and myself and Bob Nobles were over there.

GOLDBERG: You were on top of the pile?

WATTENBERG: No, we were off to one side. Sam Allison had a fear that there might just be something that hadn't been thought of. And he would like to be able to pour cadmium sulfate all over this thing and kill it. One control rod would do as much as his cadmium sulfate. Well, we were dead-set against having cadmium sulfate in bottles there, because if there was an accident, it would have delayed the thing until we could have gotten another thing. Sam Allison was a nice guy. So we did it. [Laughter]

GOLDBERG: Or maybe you did it because he wasn't a nice guy.

WATTENBERG: No, no. Sam's a lovely guy, and we did take the cadmium sulfate bottles up on that elevator, which is off to one side. And we were to throw it over.

GOLDBERG: Cadmium was a poison for a reactor.

WATTENBERG: Cadmium eats up neutrons. That's right. And it would stop the reactor.

GOLDBERG: Wasn't there somebody up there with an ax?

WATTENBERG: That was tied to the railing. There was a rope that had been tied to another control rod. So they don't really show all of the safety rods. There was a weight on the other end of it, and it was just tied there. If anything happened, that ax could have just severed the thing. There was a solenoid on another one, a little electromagnetically held out rod, and that solenoid was operated by a relay that was attached to an ionization chamber. Before lunch, that actually did operate. It tripped, as it should have.

GOLDBERG: Why didn't they use a shroud?

WATTENBERG: They didn't use the shroud because we got in more good graphite, high-quality graphite, than was needed. And also at the last few weeks, six tons of uranium metal arrived. By putting that in the center, we had much less need for building the thing all the way up, and we had a better reproduction constant.

02:13:15:00 [Golderg displays a copy of a letter written by Creutz]
GOLDBERG: Let's back up now, because there are several strands we've drawn out here. Before this reactor came on line, the project was turned over to the Army. DuPont was selected, and we got some good graphite. I've got a letter here. I don't know if the camera can pick this up. Let's see. This is a letter dated May 27, 1942. It's from E. Creutz, and it's to Vannevar Bush in Washington. Vannevar Bush was, at that time, head of the OSRD and of what became the Manhattan Project--that is, the Uranium S-1, I guess it was called then. Let me just read a little part of this.

WEINBERG: May 27, '42?

GOLDBERG: May 27, '42. The grammar is not quite right, but we'll forgive Ed that. "If we really are in a hurry to produce the chain reaction and make it useful before the German government does, and I am not kidding when I say that most of us physicists are working as though we believed that to be in the kind of hurry we are in, then something must be done to speed up the procurement of uranium metal. I believe that the way to do this is to give the authority to negotiate for and conclude contracts with the manufacturers into the hands of the scientific group who are going to decide how these materials are to be put together. This means that Dr. A.H. Compton would be given your permission to short-circuit the longer procedure of obtaining essential materials through another division, which is not and cannot be in constant close contact with the experiments."

Is that photographable? Well, in any event. . .

WATTENBERG: What kind of division were you referring to there, Ed?

GOLDBERG: Yes, that's what I wanted to know.

CREUTZ: Everything had to go through New York, through the Manhattan Project office.

02:15:12:00 [Close-up of letter]

WIGNER: I'd like to tell a funny story, or at least I think it is funny. That is that somebody congratulated me that when we came to Chicago in February, we already foresaw that a chain reaction will be established, and so we took along some Chianti to celebrate Fermi. And that was very good, he said, that was fine foreseeing this. I said, "Well, no, that wasn't difficult to foresee. Difficult to foresee was that Chianti would be already in short supply." [Laughter]

BORST: There's one incident that I think is worth retelling, which is folklore, of one of Groves' first meetings with the scientists in Chicago--that is, [Brigadier] General [Leslie R.] Groves. He was the head of the Corps of Engineers who were responsible for the development of the Manhattan Project. He is reputed to have said, "Gentlemen, I know nothing about nuclear processes; I know nothing about fission. But what I do know about is procurement. If you tell me what you want, I will get it."

And according to the story, Eugene Wigner pulled out his slide rule, did this for a few seconds, and he said, "General Groves, if you can get us 500 tons of diamonds, this would make a much better reactor." [Laughter]

GOLDBERG: You see? You're in history.

- WIGNER: I don't remember this.
- BORST: I'm sure it was true.
- WIGNER: Five hundred tons of diamonds?
- BORST: It has a higher density of graphite.
- WIGNER: Yes.
- WEINBERG: Do you remember when Fermi said, "Well, let's knock off for lunch"? Where did he eat lunch that day? Do you remember?
- WATTENBERG: We went over to the Commons.
- WEINBERG: That's what I thought. I thought that you guys all were there, and we always used to eat together. My recollection was--and I've been telling this story for 45 years--that we just tried to act as though nothing much was happening.
- WATTENBERG: That's right. We were really used to not discussing our work.
- WEINBERG: Right.
- WATTENBERG: I mean, that was built into us. We always had other topics of conversation, and Fermi was always playing games with us of asking us things to calculate. He always had a student-teacher relationship, you know, like, "How thick can the dirt be on the windows?" or things like this.
- WEINBERG: I remember, I think Al[vin Cushman] Graves was at lunch with us then.

WATTENBERG: Yes. But we actually broke up, there were so many of us. We did not all eat together. We were broken up into small groups. I think I went over with three younger people.

02:18:06:00

GOLDBERG: I'd like to go back, if we can, to this problem of the shortage of uranium or the difficulties that you were having with uranium. By this time, it was clear that you were going to have to get uranium metal. Like some of the other metals we've been talking about, uranium wasn't your ordinary garden variety metal at that time. And you did a lot of work on that.

CREUTZ: I got interested in the problem when Eugene pointed out that there was a constant known as a thermal conductivity that was important. Most of the physicists didn't think about that, I think. [Laughter] From his engineering hat he thought of it. And so we measured the thermal conductivity of uranium and some other properties. But it was very hard to get pieces. There were two men at Westinghouse Lamp Division in Bloomfield, New Jersey, who had made metallic uranium. That's D.W. Mardin and a man named John Renssler.

WEINBERG: Why did they do that?

CREUTZ: Because they were interested in lamp filaments. They said they tried practically every metal in the periodic table to see if there would be a better lamp filament than tungsten, because tungsten was so expensive. They concluded that uranium was too hard to fabricate. So they looked at uranium as a possible material for false teeth because it was so hard. The way they made uranium was kind of interesting. They took uranium nitrate, where uranium is hexavalent, and they had to reduce it to the quadravalent state. They did this by light, a photochemical reaction. They'd make a solution, put

it on the roof of their plant in Bloomfield, New Jersey, and the light would act on it and reduce it to tetravalent uranium. Then they'd electrolyze that in a molten salt bath, sodium chloride bath.

So we went up there. I guess I went alone the first time. They had a little piece this big. They called it a cashew nut. I said, "Do you think you could produce a ton of this material?" Of course, they threw up their hands, hadn't thought of that. But in the first place, of course, there wasn't enough sunlight available to do that in a finite time, practically. But then the New York office took over the procurement process, and got Westinghouse to use Westinghouse ultraviolet lamps for a light source, and they did produce the first several--how many tons for the chain reaction?

WATTENBERG: Was that Westinghouse?

CREUTZ: That was Westinghouse. That was the only source.

WEINBERG: I never knew why the sun came in there.

CREUTZ: Yes, because it was a photochemical reaction. It was about that same time that Szilard got this idea of reducing uranium salts with magnesium, and then Spedding picked up that process.

GOLDBERG: So it was Szilard who suggested that?

CREUTZ: As far as I know, Szilard certainly suggested it to me. He said, "Let's go to Brush and see if they can make uranium." And Spedding picked up the process. Probably some of that six tons was Spedding's metal.

WATTENBERG: We had a mix of stuff. The first things we got were such a heartbreak. They were made by sintering the metal. They were little cubes about so big, and we opened up the box

and started looking at them, putting them out, and they burst into flames. There was so much air surface.

CREUTZ: This was the metal hydrides process, wasn't it?

WATTENBERG: Yes. There was so much air surface thing that the oxidation, like rusting, heated up the metal, and then it burst into flames.

GOLDBERG: Uranium is pyrophoric.

WATTENBERG: Yes, very much so. In fact, what isn't known is that Zinn had a serious accident in, I think 1940, in which their supply of uranium powder. . .

CREUTZ: This was thorium, actually.

WATTENBERG: It was thorium that he got burned on?

CREUTZ: He was working with thorium powder for metal hydrides.

WATTENBERG: Anyway, it exploded, and he had his arms badly burned.

CREUTZ: I'm told the first thing that Fermi did after taking care of Zinn was to go in with a Geiger counter to see if there had been a chain reaction. Well, Fermi thought we ought to check, so he checked for radioactivity, to see if the thorium had chain reacted.

WIGNER: I see. [Laughter]

CREUTZ: I'm sure he knew it hadn't, but he thought he should check.

WATTENBERG: Anyway, so there is difficulty working with it, and the sintering process didn't work.

GOLDBERG: The stuff from metal hydrides, that was. . .

CREUTZ: That was the sintered powder that Al was talking about.

GOLDBERG: This was P.P. [Peter Popow] Alexander?

CREUTZ: That's right. Peter Popow.

GOLDBERG: Did they ever get a process? Did they ever produce anything?

CREUTZ: They made some of these sintered pieces which are not useable.

GOLDBERG: So later on, all of the uranium came from Westinghouse?

CREUTZ: No, no. Probably they produced about a ton, and then Spedding in Ames, Iowa, took over and made large quantities.

GOLDBERG: So it was Spedding. It was at Iowa State University that the. . .

CREUTZ: Several tons were made.

02:23:05:00

WEINBERG: And who made the Hanford uranium? Do you know?

- GOLDBERG: That's what I was getting to.
- CREUTZ: That was in the hands of DuPont.
- BABCOCK: I don't know.
- GOLDBERG: But it would have been then DuPont who decided that?
- WIGNER: I think that all of it was arranged by the DuPont Company.
- WEINBERG: It wasn't Westinghouse that made that.
- CREUTZ: I don't believe so. I don't know.
- WATTENBERG: You know, somewhere along the line, you should work in the story of where these companies got the uranium ore from. There is this man who had stockpiled it. You probably know. You have that in your archives.
- WEINBERG: [Edgar] Sengier, wasn't it?
- WATTENBERG: Sengier. And he turned it over to them.
- GOLDBERG: It was sitting in a warehouse on the docks of New York.
- WATTENBERG: But I think that doesn't happen 'til after Colonel [Kenneth David] Nichols is into the act, which is 1943, I guess.
- GOLDBERG: No, Nichols was earlier. In fact, Nichols was part of the Army team before Groves. It was Nichols and Marshall before Groves. Groves was brought in, actually, in September.

WATTENBERG: I'm not sure, what is the date of that Belgian uranium.

. . .

GOLDBERG: We can look it up. I don't remember what it is now. But it was ironic that they were hunting around for sources of uranium, and the Belgians had stored tons of it in a warehouse in New York.

02:24:30:00 [Begin VHS Tape 2 of 2]
[Begin U-Matic Tape 3 of 4]

02:26:50:00 [Interview resumes]

BORST: The President's office in the White House.

WIGNER: I see.

BORST: And FDR said, "General Groves, this is the secret of the war. You keep it." Now a general with that kind of injunction from the Commander in Chief, how could he do anything else but try to. . .

WEINBERG: I never heard that story. Where did you hear that?

BORST: I'll give you the names later. But this is absolutely antithetical to the scientists, because knowledge is power, and if you get compartmentalized, who is to say what information is required for that person to do his job? How do you decide what Wigner should know or Fermi should know? And yet we got into real trouble because the information was not transmitted from Los Alamos [Scientific Laboratory], and Y-12. Goodness knows how close they came, but they were handling critical quantities of enriched uranium, without knowing anything about criticality. So these are the offshoots of security. But with that injunction by FDR, of course, I can understand now Groves' adamant position on security.

02:28:06:00

GOLDBERG: But there already had been some problem. I have a letter from the archives in which Bush wrote to Compton about some of the meetings you were talking about earlier in the year, which a number of people were at who didn't have clearance. And yet they were discussing the whole project. That is, either they didn't have clearance, or they [Bush and Conant] didn't know who they were. In fact, I have that letter right here: "Frankly, both Dr. Bush and I are extremely disturbed about one aspect of this meeting, namely the fact that full disclosure was made."

WEINBERG: Which meeting is that?

GOLDBERG: This is a meeting of January 18, 1942, in which there was--I don't have the minutes of that meeting here, but the minutes show that there was a full discussion of all aspects of the program--that is, the critical mass problem, the bomb problem itself, but also the various techniques for separating U-235 from U-238, and the plutonium.

WEINBERG: It must have been a fairly high-level meeting.

GOLDBERG: Well, I don't remember.

WEINBERG: Do you remember it, Ed?

CREUTZ: That may have been the time when they were saying, "If we only had uranium metal," and I reached in my pocket and pulled out some and said, "Well, here's a two-inch sphere of it." [Laughter] That Westinghouse had made.

02:29:33:00 [Close-up of letter from Bush to Compton]
GOLDBERG: Well, in any event, the people they named that they didn't know, I'll just read you the list: Anderson, Marshall, Snyder, Jones, Zinn, Feld, Doan, and Teller. They don't know who these people are. That's according to Bush. So already, even before Groves came, maybe it was because of Gregory Breit's insistence on compartmentalization, there was already this tension about keeping things secret.

WIGNER: I have to tell a story in which we physicists were wrong, and the engineers of the DuPont Company, surprisingly enough, were right. This is a story that we wanted a reactor which has circular--well, cylindrical form. And they decided, "Well, it has a basis. Let's put it in all the way into this form."

And we thought, "Well, if you do it, it's alright, but it won't do any good." But that was wrong, because one of the fission products had a tremendous absorption, so that when the reactor was started, it soon enough stopped because absorption of this fission product was too high. But I could fix it, because I put uranium into those extra pieces, which were, in our opinion, unnecessary. [Laughter] And it made it chain reacting again. In other words, there was something that we did not foresee--a fission product with a neutron absorption--which is entirely outside the range which was known before, and which stopped the chain reaction, but which could be eliminated over countless difficulty by the extra amount of reactor that was put in by them, and which we thought is entirely unnecessary.

CREUTZ: Let me just add to this. This had been anticipated in a sense, because there was a meeting probably around June, where Compton asked each of us to think of what were the things that we hadn't thought of that couldn't stop the process. And you said, "Well, there could be a fission product, but the cross-section would have to be so large, it'd have to be λ^2 over π ," which is,

according to your formula, your and Gregory's formula, as large as it could be.

He said, "That's too improbable. Let's not worry about that."

[Laughter]

WEINBERG: Johnny Wheeler worried about it, though.

GOLDBERG: We're going to get to that.

WIGNER: John Wheeler had then identified the problem, and he told them to put in extra uranium.

GOLDBERG: We're going to get back to that.

WIGNER: I shut up.

02:32:36:00

WATTENBERG: I'd like to go to that statement that you read, and just point out that in 1941, the people who were involved-- and there really isn't that much fast neutron calculational work going on--but Szilard and Marshall actually were carrying out calculations on a possibility of an implosion technique for making a bomb.

WEINBERG: Really?

WATTENBERG: Yes.

WEINBERG: You mean that Seth [Henry] Neddermeyer was not the inventor of the implosion technique?

WATTENBERG: Well, I think that due to the fact of this compartmentalization, that that thing of Szilard and Marshall's was not known, because Szilard never goes out there, Marshall

doesn't come out 'til 1944. The one man who might have tied it together was John [Heny] Manley, but I don't know that he ever saw the report.

WIGNER: Who was that?

WATTENBERG: John Manley could have tied it together, but I don't know that they were getting the CF reports.

WEINBERG: That's the first time I ever heard this.

WATTENBERG: This is in Szilard's collected works.

WEINBERG: Well, I've read Szilard's collected works. I guess I've never seen that.

WATTENBERG: They're talking of compacting the uranium to make a bomb.

GOLDBERG: But in any event, the point, I guess, is that it's not just the Army.

WATTENBERG: So that really there may have been things that didn't get transmitted because of the secrecy that should have been, and yet you also had possibly things that did lead to the leaks of Alan Nunn May and others later on, because it wasn't that well implemented.

GOLDBERG: Look, I think maybe we should take a break now and come back again after lunch.

02:34:30:00 [Pause in interview]

20:06:44:00 [Interview resumes]

WATTENBERG: Al, we were discussing privately different reactors that people had built. We haven't heard from you. What reactors did you build, or design, rather?

WEINBERG: Well, you're talking about the wartime period?

WATTENBERG: Yes.

WEINBERG: You have to realize I was Eugene's assistant, and so what I was involved in was basically what he was involved in. The main job that we had was the Hanford reactors, and my job was to figure out the multiplication constant and the dimensions, the lattice dimensions. I remember as though it were yesterday that I finally came up with the calculations which showed how the multiplication constant, therefore the size of the reactor, varied with the lattice dimensions. And then I went in to Eugene Wigner's office, and we looked at the curves. Then we were going to decide what would the lattice dimensions be. And the question was whether the spacing of the slugs should be--I think it was eight inches or eight and three-eighths inches, and made a little bit of difference. I think it was Eugene who said, "Well, let's make it eight and three-eighths inches," because the blocks of graphite that were available came so that eight and three-eighths was more convenient than I think was eight. And that's how the Hanford dimension was chosen, and it was eight and three-eighths inches.

Now, the configuration of the water cooling, "the ordinary thing, well, that's kind of obvious," not at all obvious in those days. And the way the Hanford slugs were made, you had solid slugs, and you had the coolant channels on the outside. We called that external cooling as opposed to internal cooling, where you had holes drilled down the tubes. Now, our good friend Ed Creutz here may not recall, but he was the one who proposed external cooling. I remember that.

CREUTZ: External? I always thought it was Eugene.

WATTENBERG: No, it was Ed who proposed it.

WIGNER: External what?

WEINBERG: Cooling.

WIGNER: Cooling. Excuse me.

WEINBERG: And then the argument was, "Well, how would you make the tubes?" And I think it was Ed who suggested that you could have little ribs on the tubes, but Eugene probably was involved in that also. You see, the way the Hanford design actually was done--and it was all really done within a period of six weeks--it was really a tour de force, but largely because it was controlled and detailed by him. But the way it was done was, each of us had part of the job. Mine was the multiplication constant; Gale Young's was to calculate the heat transfer and some of the other engineering aspects; Miles Leverett was to do some of the engineering, process design. There was Leo [Arthur] Ohlinger, who helped with the detail design, but he also drew the pictures. All the pictures in the original CE-407 were executed by Leo Ohlinger. And then there was this fellow Bosivon from. . .

WIGNER: Who?

WEINBERG: You may not remember him. His name was Bosivan. He was from Belgium, who happened to be there at the time. I don't recall, Ed, whether your name appeared on CE-407.

CREUTZ: I don't remember.

GOLDBERG: CE-407 is a Met[allurgical] Lab report?

WEINBERG: That was the process design report for the 500-megawatt water-cooled reactor.

GOLDBERG: And when was that report?

WEINBERG: That came out in January of 1943. It was finished, actually, a little bit before then, and that really was the basic design that DuPont, including Crawford Greenewalt and Dale Babcock, took over and proceeded with it.

GOLDBERG: Who designed the water cooling system? Was that part of the lattice design?

WEINBERG: No. The external system was all done by the DuPont Company, although the parameters and so on had been worked out likely by Miles Leverett and the engineers who worked with Eugene. The configuration control rods was part of the nuclear design and we did that, and John Wheeler was involved in that also. But the remarkable thing with the Hanford reactors was that Eugene Wigner always retained an enormous sense of intellectual responsibility for the Hanford reactors, even though it was DuPont's job. So when the design drawings would come in to Chicago, this same person who won the Nobel Prize for his introducing symmetry into physics would look at every single design drawing, the detailed design drawings.

WIGNER: That was terribly important.

WEINBERG: That's right. That's exactly the point. And he would pore over them, and if he saw anything that he didn't like--he seems like a mild man--he wasn't very mild then. [Laughter] He would insist it was wrong.

WIGNER: You see, I think you understate this bad situation. They really did not understand what should be done.

My principal job was, to begin with, before the DuPont came in, to think what could go wrong. And there is a very important thing that I did not realize and that was luckily avoided, is that one of the fission products had such a . . .

WEINBERG: Right. And Dale Babcock was the one who proposed that we fill out, which gave about another quarter of percent to the reactor.

GOLDBERG: We're going to get to that.

WIGNER: There was great difficulty with the designs, because they really did not, in the beginning, understand what will happen. The first design was, for instance, instead of the shield, you know, the outside shield, they had a very strong containment grid. And, of course, the neutron is smaller than even this amount of the shield. We had to correct that. There were several things which we had to correct, because, of course, members of the DuPont Company did not think of microscopic substances that can come out.

WEINBERG: They learned, looking back on it, quite fast, Eugene.

WIGNER: Yes, they learned.

20:13:41:00

GOLDBERG: Let's pull back a minute. There are several places that I'd like to stop here. First of all, Crawford Greenewalt, and go back to Groves. You have the same problem with Groves, in a sense, that you had with the DuPont Company, starting out; DuPont had no experience with radiation.

WEINBERG: None of us had very much more experience, I must say, you know. Chain reactors were all of six months old at the time! [Laughs]

GOLDBERG: But here was Crawford Greenewalt and the DuPont Company, who now had the contract, and that was a cost. . .

WEINBERG: One dollar, I think.

BABCOCK: I would rather say we did it for a fee of \$1.00. That's important.

GOLDBERG: It was Oswald [Herman] Greager who said that the contract was to build and operate the Hanford facilities and take them down afterwards. He also made a strong point in the interview we did with him that DuPont did not want to have responsibility for choosing the design or the process. I'm not sure why.

WEINBERG: That's surprising because, you see, when the DuPont Company was brought in, one of the first things they did was to examine the different possibilities. And what was on the books, more or less, was the helium-cooled design, the Leverett design; there was Szilard's idea for bismuth cooling; there was heavy water, which was taken quite seriously, the heavy water reactor. I think Harold Urey was promoting that. And then there was the Wigner water-cooled graphite. The DuPont Company decided, among those four, that the water-cooled graphite was the most feasible. I don't know if that agrees with your understanding of the situation.

WIGNER: It was a cage instead of a shield.

WEINBERG: Right.

WIGNER: And of course, many neutrons can come into that. . .

- WEINBERG: And Crawford Greenewalt, who was the head, then, of what was called the TNX Division. . .
- BORST: He was the head of the technical portion.
- WIGNER: But he became president of DuPont.
- WEINBERG: Greenewalt's job was to oscillate between Wilmington and Chicago.
- WIGNER: Between?
- WEINBERG: Between Wilmington and Chicago and convey the information from Chicago to Wilmington, and make sure that that went back and forth. So he worked very closely with us in those early days, the very earliest days. I remember we would argue with him about design details, and he made many good suggestions. I think, for example, the actual design of the shield, the actual materials of the shield, that was a DuPont proposal, where they had this plastic.
- WATTENBERG: That's somewhat later, because that was in the summer.
- WIGNER: Because the first design was a cage.
- GOLDBERG: Let's see if I understand this now. You were responsible for the lattice design.
- WEINBERG: The lattice design and the size of the reactor.
- BABCOCK: Can I interrupt a minute? We always said the basic design went to you people. The nuts and bolts design was a DuPont project.

GOLDBERG: What were you working on at the time?

BORST: Well, when DuPont sent its first contingent out to Chicago, under Charlie Cooper, these were a bunch of eager beaver graduate students in chemical engineering, and I was asked to instruct them as to what a neutron is and what a nucleus is and what fission is and so on, and get them adjusted to this new kind of field.

GOLDBERG: Was this in Chicago?

BORST: Oh, yes, in Chicago. Then a little later, I was in Wilmington on the mixture of Hanford and Oak Ridge design, and then when Oak Ridge started, I came down here.

GOLDBERG: You came out to Oak Ridge.

BORST: Yes. Before we go too far, I would suggest that we discuss the siting problem, which is not trivial, the question of a site.

WATTENBERG: Can I just interpose one thing? That I was involved, or they showed up also at the reactor that we had in Chicago, and we did help them to learn how to run the reactor, give them the experience of running the reactor, benefiting from other people having taught them basic things. They were exceptionally bright, and I had the feeling that they weren't just graduate students; they were much more experienced people than the people we had had previously from industrial companies.

GOLDBERG: When you say "the reactor," you're talking about the reactor that originally was in. . .

WATTENBERG: CP-1, the first reactor. Two weeks after we had it built, two weeks after the first operation, we were teaching other people how to operate it, how it reacted.

GOLDBERG: But you also moved it, did you not?

WATTENBERG: Just the control room. No, no. It was still the original reactor. We didn't move it 'til February. And they were there, some of them were there during that period.

GOLDBERG: So when did you actually move the reactor?

WATTENBERG: We moved it in February, and it was reconstituted completely in April. We started taking it down in February.

GOLDBERG: February of '43.

WATTENBERG: Yes.

GOLDBERG: But you'd already been training DuPont people on it before then.

WATTENBERG: Yes, we all learned to operate it very quickly, and Dale was one of the people who operated it.

BABCOCK: That's right.

WEINBERG: It was in 1942, wasn't it?

WATTENBERG: It was easier than teaching people to drive a car. [Laughter] You could walk away from it for five minutes and nothing would happen. You can't do that from a running automobile.

WIGNER: Instead of a shield, a cage--that something would
 terrible happen. There were several other things that
I was worried about, and some were unnecessary. You see, I considered
my job to think of the dangers, and one of the dangers that I was afraid
of is: that as the neutrons collide with the carbon atoms, the carbon
atoms will be displaced and a good deal of energy will be accumulated.
And I was afraid that if these energies accumulated, and then suddenly
released because it's heated up, there may be almost an explosion. But
that, fortunately, did not realize.

WEINBERG: In a sense, it did happen at Harwell, though.

WIGNER: It did happen?

WEINBERG: Oh, yes.

WATTENBERG: In Great Britain.

WIGNER: In Great Britain, yes. But all that was strong enough
so that the carbon atoms. . .

GOLDBERG: Would actually start to swell.

WEINBERG; Well, that's a later story.

WIGNER: So it's not such a new thing. It's not really easy in
 every way to establish. One has to think what could
go wrong, and that I considered my principal job. These gentlemen
thought of the details very well, and it worked really well for a long,
long time. Isn't that so?

WEINBERG: Yes, those reactors ran for over 20 years.

GOLDBERG: 1967, I think. I don't want to forget this: how do you move a reactor?

BORST: At that time it wasn't very hot.

WATTENBERG: They had avoided bringing it up to a very--they had avoided running over 200 watts for any extended period of time, so the radioactivity was minor.

20:21:35:00

WEINBERG: Excuse me, Al. These's a rumor that I've heard that Herb Anderson actually made the reactor critical the night before. Is there any evidence to support that?

WATTENBERG: Everything I ever heard said he didn't, that he did not do it.

WEINBERG: Fermi got him to promise not to.

WATTENBERG: And we obeyed the rule.

WIGNER: What did you say about Herb Anderson?

WEINBERG: That the night before December 2, they just made it critical, just to see if it would work.

WIGNER: What did he do?

WEINBERG: He didn't do it. It's a false rumor.

WATTENBERG: People were quite responsible. They gave me the keys to the reactor. I did not run it all by myself.

[Laughter] But anyway, we tore it down by using four young men and myself. Alright, I was also a young man--five young men. And we at

first were picking up the individual bricks, putting them on a thing, then carrying them to a place where a truck would pull up, and putting them on. They were transported out. And it was done with everything, uranium oxide in place, and we just moved it piece by piece. We did put on gloves to avoid the contact of possible beta emitters on our fingers, the radiological effect, and that did cause a few accidents of fingers getting smashed, but nothing serious.

BABCOCK: I remember seeing you people do that. I stood there and watched for maybe ten minutes. You turned black, didn't you?

WATTENBERG: Yes, we did. We not only turned black, we breathed black, and after we washed, we oozed black.
[Laughter]

GOLDBERG: It just oozed out of your pores.

WATTENBERG: So anyway, it got moved out, and then there were some other people my own age and things reassembled out there into a three-walled shield at that time, and a fourth wall we put up by hand later, alright.

GOLDBERG: At that time, you were a graduate student. You were between a master's and a doctorate, is that right?

WATTENBERG: I had passed all my exams. I hadn't submitted a thesis. That's right.

GOLDBERG: But you were also designing elements of the reactor.

WATTENBERG: Later on, yes. Designing elements of the reactor. Later on, I did calculations on reactors, yes, and I

also analyzed reactors. I tried a type of reactor that did not work, namely a homogeneous mixture of uranium salts in heavy water.

GOLDBERG: But at Chicago. . .

WATTENBERG: No, that I did outside, in the Chicago area.

20:24:32:00

WEINBERG: Well, let me say that after the Hanford reactor was, so to speak--the cake was put in the oven, as far as Chicago was concerned. Then although we still reviewed the blueprints as they came in, on the whole, our main job had been finished, so we moved on to the next sort of thing. We had a committee that was formed. I think it met at the end of 1945. No, it was the beginning of 1944, because Fermi was still at Chicago then. That we called the New Piles Committee, which met for about--we called these things "piles" at that time--met every two weeks for about, oh, there may have been about ten or twelve meetings. I had recently occasion to peruse the minutes of those meetings. They're still very interesting. People would present ideas for new reactors for various purposes, and there would be Fermi, and Compton sometimes would be there, Szilard would be there, Wigner would be there, occasionally when Teller was in town, he'd be in there, and all the younger people would sit in the back rows. Everybody at that time--well, it was very easy to invent a new reactor, because it was a whole new business then.

Now, the specific job that the laboratory turned to right after Hanford was the design and construction of the X-10 reactor, which Lyle Borst was very much involved in. My job there was essentially the same as it was for Hanford, namely, to design the lattice. It was really quite a very straightforward thing, and guys like Lyle and his associates did the hard work, which was to see that the slugs would not leak. You remember you had problems with the leaks in the slugs? So the X-10 reactor then went critical in November of 1943, and, of course, that's a separate story which Lyle will tell.

Beyond that, Wigner's group investigated all sorts of different reactor configurations with every conceivable mixture or combination of fuel, moderator and coolant, and we looked at heavy water reactors. In fact, you may recall that about that time, Harold Urey was banished from the project in Manhattan--I mean at Columbia. He went into a sort of Babylonian captivity for about three months or so at Chicago, and he took along with him two very bright young fellows, Karl [Paley] Cohen, who then became the chief scientist for the General Electric Company, and Professor Kaplan at M[assachusetts] I[nstitute of] T[echnology].

GOLDBERG: Irving Kaplan?

WEINBERG: Irving Kaplan, yes. They were, of course, in the diffusion business, and they learned the reactor business. I always have felt that in one sense, Karl Cohen was unique, and it was for this reason they played such a central role in establishing the main line of reactor development--because he was almost the only person in the United States at that time who understood completely both the technology of isotope separation and the technology of reactors. And therefore, the idea of using slightly enriched uranium, which all of us thought was a terrible thing because it was so expensive, for him it was a very natural kind of thing.

WATTENBERG: That's our competitor's product.

WEINBERG: Yes. Yes. So these people were at the Metallurgical Laboratory again under a DuPont chemical engineer by the name of Ace [Harcourt C.] Vernon. Remember Ace?

CREUTZ: Oh, sure.

WEINBERG: And Harry [Henry DeWolf] Smyth was the head of that group. They were the ones who were supposed to look

at the use of heavy water. Because at that time, we weren't all that certain that Hanford would work.

WIGNER: Oh, we knew it. [Laughter]

WEINBERG: Yeah, we were kind of certain, but we always felt that maybe you should have a second line.

WIGNER: Yes.

WEINBERG: So people looked at the use of heavy water and, of course, Wally Zinn then built the CP-3, which was the first heavy water reactor.

GOLDBERG: Zinn's name we haven't mentioned very much, but we were talking about this at lunch.

WATTENBERG: I wanted to say that, going back to the '40 -'43 period, that we should mention that the people who played a major role at Columbia and in building the first reactor at Chicago, really were Wally Zinn and Herb Anderson, George Wyle was with him, John Marshall, Harold [V.] Lichtenberger, and Bob Nobles. But Zinn was really. . .

WEINBERG: And Leona.

WATTENBERG: And Leona Marshall, I'm sorry, yes. Leona Woods Marshall. I really feel that Wally Zinn was an exceptionally talented engineer and manager. He was very good at making people work for him and making sure that things worked well.

WEINBERG: A tough guy. Tough guy.

WATTENBERG: And also he was cautious and had backups. A very exceptional person. I think things really worked out on schedule due to a great extent to Wally Zinn.

WEINBERG: And then he became the vice president in charge of nuclear engineering for Combustion Engineering Company. It's interesting that of the American reactors, the Combustion Engineering reactors are the best.

20:30:38:00

GOLDBERG: Well, let's talk about two things now. I mean, just to nail this down. One is the siting of Hanford and the criteria for that, and then the relationship between the Hanford reactor and the reactor X-10, the reactor here.

WEINBERG: Let me say that the siting problem occupied Professor Wigner's attention.

GOLDBERG: In his spare time.

WEINBERG: No. It was part of it. And we used to have little meetings, and we'd have maps. This was probably the spring of 1942, when we'd say, "Well, we're going to build this. Where should we do it?" The place that we liked best we called Site S for Lake Superior, and it was up on the Houghton Peninsula. I happened to be up there not long ago, by the way, and that's where Manson Benedict comes from, you know, and that's where the Calumet Heckler Company used to be. They're out of business now. The reason was because Lake Superior was cold and the water was pure. We studied it, and that was our great hope for where to site it. Now, by that time, well a little later, the Army had chosen Oak Ridge, and originally and for a very short time they thought they'd put the reactors down here. But then when the DuPont Company came in, they recognized the scale of the thing

and said, "No way." It was then that they chose Site W. You probably weren't involved in Site W.

BABCOCK: Very casually.

GOLDBERG: Were any of you involved in those siting meetings?
Because [Franklin T.] Matthias was then an observer for Groves, and went to a meeting in Wilmington, I guess.

BORST: I don't think Chicago was involved in that siting. You see, Oak Ridge was chosen for the diffusion plant, and it was decided to keep it away from the coast, put it behind a couple of ranges of mountains so that if there were an invasion, it would be a defensible site. So they found that the Clinch River was excellent water, and arranged to build the K-25 plant here.

Then, of course, Chicago proposed that we build the production plants here, but DuPont first asked, "How big a bang will come out under the maximum circumstances?" And somebody came up with that calculation--I suppose Eugene was in on it--and decided that this site was hopelessly inadequate. And then they looked around for a big water supply, a big power supply. Cooley Dam was just about to come in, so the Cooley Dam never provided power for the northeast. The electricity went from the dam to Hanford and pumped the Columbia River through the reactors.

The DuPont Company again said, "Don't build a pilot plant where you're building the big plant." Because inevitably the construction people will see the ribbons in the big plant and the pilot plant will be starved. So they placed the X-10 air-cooled graphite reactor at Oak Ridge, and then, of course, the water-cooled reactors out at Hanford.

20:34:02:00

GOLDBERG: If the big plant was going to be water-cooled, why wasn't the little plant water-cooled?

BORST: They didn't know that much.

WEINBERG: Well, that was a sore point, and there were big arguments between Wigner's group and the DuPont Company. I remember very distinctly that Professor Wigner would point out exactly that point: that if it was going to be water-cooled, then the little plant should be water-cooled. But the DuPont position was that the main reason for building it was not so much to get experience on the reactor as it was to get the plutonium to try out the chemical plant, and that you had to have something that you knew would work.

BABCOCK: And in a hurry.

BORST: And in a hurry, yes.

WIGNER: Perhaps I should mention that we discussed very much the reactor with them. We did not discuss the full chemical processes which separated the uranium and the plutonium from each other. And also the other things, which was also very important, and many people whom we don't really know well or at least I don't know really well, though I know some of them, contributed very significantly. It was not at all a superficial and easy thing to do in the second part of the afternoon. But it was a serious problem, and it was not easy to build and to put into operation the chemical plant which separated the plutonium.

WEINBERG: I guess you expect to interview Glenn Seaborg, do you?

GOLDBERG: Yes, I do, but we've already begun, and we have an interview with some of the DuPont people who ran the canyons at Hanford. Again, you know, there's so much time in an afternoon. I don't think we'll be able to do that and talk about the reactors and talk about Chicago in 1944 and 1945.

WATTENBERG: Do you want to just put in, though, that Oppie was asking Seaborg whether they could purify plutonium to one part in ten to the eighth, because of this problem that if you have light element impurities in there, the neutrons will be emitted, and the bomb, instead of going off, would just fizzle. So the purity was a nuclear physics problem, basically.

GOLDBERG: There was, of course, a crisis late in 1943.

WEINBERG: Right.

20:36:36:00 [Goldberg displays an enlarged photo of the face of the X-10 reactor]

GOLDBERG: They were really worried that you couldn't get it pure enough. This is the face of the X-10 reactor.

WIGNER: Which one?

GOLDBERG: X-10.

WIGNER: X-10, yes.

GOLDBERG: Why don't you say something about it.

20:36:50:00

BORST: Well, it was the simplest possible extension of CP-1. CP-1, of course, had no cooling system. What we did at X-10 was make the first gas-cooled system. Actually, we cut a diamond shape into the graphite and put the slug sitting on the bottom of the diamond, which is not optimum by any means, but it was something you could do with woodworking machinery. So it was done that way. It was built about 25-foot-cubed and surrounded by a shield with a buffer zone between the shield and the reactor.

- GOLDBERG: So what we're looking at here. . .
- BORST: This is the charging face.
- GOLDBERG: There's many feet of concrete between here and the. . .
- BORST: About seven feet of concrete.
- WEINBERG: You have to realize that the original CP-1 was cubic symmetry; that is, you had lumps. But here it was cylindrical. These were cylindrical.
- BORST: Which lost some K, but it made engineering much easier.
- 20:37:52:00 [Goldberg places a photo on the easel of a worker inserting slugs into the reactor's charging face]
- GOLDBERG: Here's a close-up of the charging face, with somebody actually putting the slugs in. Are these the slugs used at Hanford?
- BORST: No, quite different.
- GOLDBERG: What is the difference?
- CREUTZ: These were 4 inches long, 1.1 inches in diameter, and Hanford's were about twice that long and about 10 inches longer.
- WEINBERG: I think there's another essential difference, that these were just canned, whereas the others were more metallurgically bonded.

- BORST: These are just slid into an aluminum can, and the end of the can was closed.
- WEINBERG: Lyle, I think you were in charge of figuring out whether they leaked or not, and it drove you crazy.
- BORST: Yes, I was. We were ready to charge the reactor. The reactor was completed, and we discovered that two-thirds of the uranium cans were defective.
- WIGNER: Two-thirds of the uranium cans were defective? Which cans?
- BORST: The aluminum cans around the uranium.
- WIGNER: Around the uranium.
- BORST: Right.
- WIGNER: They were defective?
- BORST: Yes.
- WIGNER: That would have meant that the uranium dissolved.
- BORST: That's right. That was the problem.
- WIGNER: Two-thirds of the cans were defective?
- BORST: Yes.
- GOLDBERG: Was it the same defect in all of them? They were just leaks?

BORST: Just leaks, that's all.

CREUTZ: Leaks in the welds, primarily.

BORST: But you couldn't send them back to the manufacturer and load the reactor. So the first criterion was to get the chain reaction going. Six months earlier, I had, just as a matter of backup, ordered an assay balance, a balance about so long and about so high, which would measure masses to one part in ten to the eighth up to a kilogram. Well, we had to do something pronto, and so I went to the head of our procurement section and said, "Well, now, we need this balance." Well, two days later, it was flown in to the Knoxville airport on a B-18, because it was too big to go into an ordinary commercial aircraft.

WEINBERG: I never heard this story, Lyle.

BORST: And we put it in the bomb bay. Then when they got to Knoxville, why, they took it out of the bomb bay and got it over to the laboratory. Of course I immediately got it going and had people weighing slugs, because the only thing I could think to do is take the slug which was defective, weigh it precisely, put it in an oven, expose it to high temperatures, take it out and weigh it again. And see how fast these things go bad. There was no point in talking about perfect things; there just wasn't any. So we had to charge defective material, and we just wanted to know how hot we could run the uranium without having this stuff oxidate too rapidly.

Well, the next day, the Corps of Engineers' representative turned up at the laboratory, wanting to know why we had to have a chemical analytical balance flown in by a bomber to Tennessee. Fortunately, at the time I had a gal weighing slugs there, and he saw that it was already in operation, and didn't worry about it any further, any longer, otherwise, I would have been. . .

- GOLDBERG: He said if you needed it, he could get it for you.
- BORST: Well, they did. I don't know who they took it away from.
- WIGNER: How soon was it noticed that many of the uranium pieces were damaged?
- BORST: The slugs were canned, I don't remember where, and then they used what was called a wheel arc closure. They spot-welded all around, and then folded that thing in, and the result was that there were defects. Now, as far as I know, there were no production tests. At the production facility, they just thought that they were perfect, so they shipped them. And they got to Oak Ridge, and we had the required amount of uranium. But it was no good.
- WEINBERG: Did you actually use those in the first building?
- BORST: Yes. There was no alternative.
- WIGNER: They leaked?
- BORST: Yes!
- WIGNER: What did they leak of, principally?
- BORST: Well, we never analyzed it. We were too busy trying to get the damn thing going.
- WIGNER: But how soon was this discovered?

- BORST: Well, as soon as we were thinking about charging the reactor, we figured we'd better test it ourselves and see how good they were, and we found they weren't.
- WIGNER: But how soon did this come?
- CREUTZ: You had run the reactor?
- BORST: No, no, this is before charging the reactor.
- GOLDBERG: Before it was charged.
- BORST: Probably two or three weeks before criticality. Something like that.
- WIGNER: Then I didn't understand, foolishly enough, what the first phenomenon was. You had those cans, and they were out of order?
- WEINBERG: They had microscopic, small leaks in them.
- CREUTZ: Pinhole leaks in the weld.
- WIGNER: In the weld. And how did you know this?
- WEINBERG: Originally, how did you notice them?
- BORST: What they did was, they put a couple of tons of this stuff into an autoclave and filled it with hydrogen and then heated it up, and everything blew up.
- CREUTZ: Made uranium hydride.
- BORST: Yes. So we knew something was not right.

WIGNER: And you went in?

CREUTZ: You went and made uranium hydride, which expands and would swell up the can.

WIGNER: And then those cans you didn't use.

BORST: Oh, no, they used them anyhow.

WIGNER: You said something, that two-thirds of the cans. . .

BORST: Were defective.

WIGNER: And then you could use only one-third?

BORST: No, they used them anyhow, Eugene. We charged everything. That was the only way to get the reactor going. And then we operated the reactor at such a level, at such a temperature, that the oxidation was not excessive. Even though it was oxidized, it was not excessive.

WIGNER: I didn't know about this.

GOLDBERG: This canning operation was different than the canning operation at Hanford.

BORST: Oh, yes.

WIGNER: The canning operation you were intimately involved with, the problem was related to the canning operation.

20:44:50:00

CREUTZ: We worked on some of these problems when we came down to Oak Ridge, some of us from Chicago, and used a different test. We put a number of these slugs in an autoclave and pressurized it with helium at about 500 pounds per square inch. First we measured the length very carefully to a thousandth of an inch. We left them overnight in the pressure. We reduced the pressure. Those that had leaked, the high pressure helium inside would now swell and make the can longer, swell the ends. We remeasured them with a dial indicator, and we found about a third of them had leaked. But that was another method of testing.

WIGNER: What did you do with those?

WATTENBERG: The technology of arc welding we uncanned--in its early stages.

CREUTZ: We took the aluminum can off, put them in new cans, we welded more carefully and tested more carefully.

WIGNER: I see. That I did not know. So that was a very serious thing, that the aluminum cover was not good.

BORST: . . . produced enough additional material and was better canned and then we put that in. . .

[Note: Cross-talk on tape]

20:46:04:00

WEINBERG: The reactor was operated a long time at only about 300 kilowatts.

GOLDBERG: That caused a problem later on, because when the Hanford reactor first encountered the xenon poisoning, you hadn't had that experience in Oak Ridge.

BORST: Well, there's blood on the saddle there. [Laughter]
The claim was that we should have found the xenon poisoning at Oak Ridge. After we got this telegram from Hilberry at Hanford, we knew that there was something wrong. And we simply shut the reactor down and watched its criticality, and we found that we were observing xenon poisoning. But at the level the reactor was operating at that time, the time required for the reactor to heat up from the nuclear reaction was about the same time as for the xenon to decay. And this reactor has a very high temperature coefficient, because it's got a lot of nitrogen in the graphite and so on. So it was not at all easy to miss the xenon.

Now, organizationally, the DuPont company, which was responsible for the reactor, for safeguarding the reactor, Henry [Winston] Newson, who was my boss, and I were very close to the DuPont people and close to the reactor. But we had no official status. I have never operated a reactor, never removed a control rod or inserted an emergency rod. But the operators--well, some of them hadn't much more than a high school education--told me one time, "You know, after a long shutdown, gee, this reactor is hot." Well, what do you make from a statement like that? I knew the temperature of the reactor, and I didn't take the bait. And they were saying that the reactor was extremely reactive and that it became critical with many more rods inserted. Of course, if I had caught that, then we could have dug it out. But since it was the operators and DuPont, why, this one went between chairs. And we did not detect xenon until Hanford. There, of course, it was not a matter of incremental or small effect; it was disastrous. The reactor became critical, and then it. . .

WIGNER: Is this published?

BORST: No.

WIGNER: This is a secret? Because I did not know what you just said. This was a secret?

BORST: Well, I wrote a report on the xenon poison at the time.

WIGNER: The xenon poison was not a secret.

BORST: Well, it certainly was at the time of the Smyth Report ["Atomic Energy for Military Purposes"], because it was in the first edition and it was excised.

GOLDBERG: It was taken out of the Smyth Report?

BORST: Yes. You read the bound copy and compare it with the mimeographed copy, and you'll find that the xenon is in one and not the other.

WIGNER: The xenon poisoning was discovered soon, but that wasn't what made the holes in the. . .

BORST: No, we're talking about a different process.

WIGNER: But how about the holes in the. . .

BORST: That's not been written up. I have not published that, no.

WIGNER: But about one-third of the plugs had this fault, and things were separated?

BORST: No.

WIGNER: They were used the same?

- BORST: They were all used.
- WIGNER: What was the effect of that?
- BORST: Well, we were limited in the level at which we could run the reactor. We could not run the reactor above a certain uranium temperature. Otherwise, the uranium would fail.
- CREUTZ: Did any fail, actually?
- BORST: Oh, sure, but it was an acceptable failure rate.
- GOLDBERG: Let's turn for a minute now. . .
- WIGNER: This is very interesting and really remarkable, because even though I was supposed to know everything, I certainly did not know this.
- BORST: I don't know why.
- WATTENBERG: Did your delays lead to the chemical plant wanting to get some volume uranium from Chicago? Because we shipped uranium from Chicago to CP-2.
- BORST: This did not delay start-up, it did not delay coming up to power, but it did reduce production.
- WATTENBERG: Well, that's probably why we sent down some.
- BORST: And of course, for the people in the chemical plant, they were interested in getting a radioactive material to try in the plant to see how it goes.

WATTENBERG: We pulled things out with a toilet plunger stick and dumped them into a truck and shipped them down. Al is going to leave us soon. Are there other things we should be. . .

20:51:21:00

WEINBERG: I'll just mention one other thing, and that is that the ideas for using pressurized water for running power reactors really originated very early, during the war, actually. As was mentioned earlier, at Los Alamos in 1944, there was this crisis when they realized that they could not make a gun weapon out of plutonium, actually because of the spontaneous fission in the 240. So there were all sorts of questions that we raised. What could we do if all this plutonium at Hanford was really not very useful? So there was a meeting that was held, I remember. Again, Compton called the meeting, various people attended, and people gave various suggestions. Wigner's suggestion was, well, we could convert all of this plutonium into Uranium-233, and you would convert the Uranium 233--well, let me go back a way.

One of the things that Wigner's group did was make a rather systematic investigation of the possibility of making chain reactions with a variety of moderators. Of course, water was one of the moderators, and we realized very quickly that you could not make a chain reaction with water as a moderator, that you would need a certain amount of enrichment. So the idea of a reactor with plain water moderation was not anything all that new. But Wigner suggested that you dispose the plutonium in plates, aluminum plates, and then surround that with thorium, and then you would be making then what we would call today a U-233 converter. So that was the first suggestion that I know of, of a configuration in which you had highly enriched uranium with water moderation and actually water cooling.

About that time, the Navy approached us, and they were talking about submarine propulsion. Some of said, well, the simplest way to power a submarine would be with a reactor of this type and which you pressurized it. And that's really basically how the pressurized water reactor got started. I began making some calculations on the possibility

of establishing a chain reaction with natural uranium and ordinary water. The people at Oak Ridge had been doing experiments on trying to figure out whether ordinary uranium, if it were in a sufficiently large massive block, could explode. And Art Snell was the one who did that. So they had these experiments in which they took all of these slugs, which were destined to go into that reactor--about 35 tons of them--just piled them up and just tried to see whether the things would blow up. If they blew up, then--I'm exaggerating. But then having those slugs there with interstices, it was not very difficult then to put water in there and see what would happen. That's how the very first ideas for what later became pressurized water reactors, which were the basis for most of the industry now.

And, of course, at that time we realized that--well, to go back for a moment, by late in 1944, Fermi was still at the Metallurgical Laboratory, so it was the middle of 1944. They had done enough experiments. . .

20:55:30:00 [Pause in Tape]
 [Begin U-Matic Tape 4 of 4]

WATTENBERG: They essentially killed off all their nuclear physicists
 After the war, they all had disappeared. They never
 reappear. There was, in general, a complete anti-intellectualism among
 the (inaudible) and these neutron physicists in Russia. They're all gone.

BORST: They got drafted, though, to Siberia.

WATTENBERG: Could be. What is it--one in 20 or one in five
 Russians. . .

GOLDBERG: One in 22.

21:07:15:00 [Interview resumes; Weinberg has departed]

WATTENBERG: One in 22 Russians got killed. But also, they were all
 suspect. All of these nuclear physicists were suspect.

They were friends of people like [George] Placzek, and he played tricks on them like visiting them, and then they went to jail afterwards.

GOLDBERG: But you know, the really big secret was on the day that the bomb dropped, that it worked. Once you know it works, then there are only a limited number of. . .

CREUTZ: The problem we had was: we didn't know if it worked.

GOLDBERG: I've always felt that if the spies did anything, they saved them a little money.

WATTENBERG: But you know, that purity problem, you know, thank God it slows it down, all right. Because many places cannot build a separation plant, and if the pile worked, and you didn't need a good separation process, high purity, it's a problem. We should be grateful for us that it couldn't be done any easier.

GOLDBERG: But I guess if you have the will--that means enough money--and you're wanting to spend it. . .

CREUTZ: Well, it takes a few very smart guys like Eugene Wigner to think of these things.

BORST: Russia has smart people.

BABCOCK: Oh, yes, no doubt about it.

BORST: [Dmitri Ivanovich] Mendeleev was a Russian. Who could find a better chemist than Mendeleev? Chess players frequently are Russians. So they have intellects.

WATTENBERG: Well, Fermi will tell you how to do it without Wigner. Which is you build one that doesn't work, and then you

build another that doesn't work, and then you finally get one that works.

CREUTZ: Part of it was the industrial capacity and techniques. You've seen Russian accelerators, how massive they are. And the finesse of American industry was extremely important.

BORST: They reduced the costs, certainly.

BABCOCK: And the time.

BORST: And time, I suppose. But it didn't change the result. You can get by with a pretty cumbersome arrangement.

21:09:25:00

BABCOCK: Didn't the Russians have a hydrogen bomb very early? I mean, before we did?

BORST: That's right. Of course, that was not a hydrogen bomb; that was a U_{235} bomb.

WATTENBERG: Somebody at EPRIE has gotten hold of the Chinese records and has the history of their making--and is translating--the history of the problems they ran into in building reactors. I don't know who it is, but you can check out with EPRIE. It's one of their engineers who has gone and learned Chinese and has learned--I meant to follow it up. But I think it is one of the EPRIE engineers whose made a study of the Chinese history. But apparently it's available--in Chinese.

CREUTZ: The China Syndrome.

WATTENBERG: They apparently discussed their problems. It would be interesting to find out.

CREUTZ: It sure would.

BORST: The big secret was the fact that it worked, you know. On August 6th, [1945] I heard over the Oak Ridge radio a broadcast from Germany.

WIGNER: In German?

BORST: No, it was in English.

WIGNER: In English, but from Germany?

BORST: From Germany, in which they said it was Heisenberg, and Heisenberg, or the voice said it was impossible to build a bomb.

WIGNER: It was impossible?

BORST: It was impossible to build a bomb.

WIGNER: To build a bomb.

BORST: Yes. Now, that was the first 24 hours, you see. After another 24 hours, he decided it was just barely possible.

CREUTZ: What was the occasion of such a speech?

BORST: Well, after the bomb was detonated, they went around interviewing all sorts of people, and they got Heisenberg.

GOLDBERG: He was then in England.

BORST: I don't know. That may be. I don't know
Heisenberg's voice. I can't certify, but he was
announced as Heisenberg, and he certainly talked like a physicist.

WATTENBERG: He speaks very good English.

BORST: Yes, that's right. So I had no reason for doubting the
fact.

WIGNER: When was this?

BORST: August 6th.

WIGNER: Of 19. . .

BORST: '45.

WIGNER: Oh, in '45.

BORST: The first 24 hours after the bomb was detonated.

WIGNER: And he said it was impossible?

BORST: Yes, he said it was impossible.

CREUTZ: Somebody suspicioned it was a bomb, and he said no?

BORST: Well, it was announced as a bomb, and he said it
couldn't be.

WATTENBERG: The Japanese didn't believe it either.

WIGNER: He changed his mind.

BORST: He changed his mind. Of course, he didn't have the knowledge of the fast reaction. He did not have the cross-section for fast neutrons.

WIGNER: I see.

BORST: And he was thinking only in terms of thermal reaction. You can't make a detonation with a thermal reaction.

WIGNER: But listen. He should have known that there is a cross-section.

BORST: I'm sure he knew there was a cross-section, but what it was, I don't know. Anyway, I'm stating what I heard, and I can't justify it.

21:12:44:00

GOLDBERG: Let's hike back a couple of years before 1945. I'd like to go over now something that we skipped across, when we were talking about the canning: the problem with canning at Hanford, which was different than the problem at Oak Ridge. In fact, it was so difficult that I think at one point, Professor Wigner was in favor of not cladding the uranium with solder or whatever it was you were using. You were the expert by this time.

CREUTZ: I was no expert, but we were doing experiments when I first went to Chicago. In June of '42, we carried out experiments on trying to hot dip. I went around to the metal companies and said, "How do you protect metals from corrosion?" Well, electroplate, you electrically hot dip or you add some second metal. The electroplating we looked at. In fact, DuPont chemists helped us on the electroplating. That did not seem to be satisfactory. For a while we tried a nickel-chromium-nickel sandwich, but it had pinholes and the uranium would oxidize and the plating would come off. So we looked at hot dipping, like

galvanizing, where you dip a piece of metal in a molten other metal, and it sticks to the surface. We could do that with zinc, but the thought then was that the cross-section of zinc was too high to make that very interesting, and furthermore, it was essentially impossible to keep some uranium from getting into the zinc. So you'd have fission products exposed to water. Then the third was putting some other can around it. It got to have the name of cladding, which is an awful word, because it's "clad" with something else, so it was "cladding." So the cladding with aluminum, as Eugene suggested, was a good idea in cross-section and workability and melting point and so forth. So the first large-scale work for cladding the Oak Ridge cans, I imagine that was done by DuPont. I don't know who did that cladding.

BABCOCK: Yes.

CREUTZ: It soon became apparent that that would not be good enough for the higher thermal fluxes in Hanford, because if the can weren't quite tight on there, if the air film of a fraction of 1,000th of an inch would be too big a temperature drop, the uranium would get too hot. So the problem was to do some bonding. A fellow named Mike Foster suggested using first tin, and Wigner worried about the tin disease, where tin changes its crystal form and expands after some time. So Foster suggested a tin-zinc alloy for the solder, if you want to call it that, between the uranium and the aluminum. That worked quite well except, again, the cross-section seemed a little too high. So we tried alloys of aluminum. Now, of course, there you get into melting point troubles, because aluminum melts at around 600 Centigrade.

WIGNER: Fahrenheit.

CREUTZ: Fahrenheit. Excuse me. And if you want to have a solder that contains aluminum, you can drop the melting point a few degrees, but that makes it very difficult. But about

that time, DuPont came in, and if you alloy aluminum with silicon, you do drop the melting point by a few degrees. A very difficult thing to work out. But this was the process that did prove successful. The DuPont engineers and chemists worked that out, so the aluminum-silicon solder was used to solder the uranium in the aluminum cans.

WIGNER: So, actually, you put it not into an aluminum can, but into an aluminum-silicon can?

CREUTZ: No, you do put it into an aluminum can, but inside the can is a layer of aluminum and silicon alloy.

WIGNER: Where is that inside the can? Is it attached to the aluminum can?

21:16:58:00 [Creutz demonstrates with a drawing]

CREUTZ: Aluminum can. A layer of aluminum silicon. Initially, that was a separate can.

WIGNER: This was melted on it?

CREUTZ: Not yet. Then the uranium was placed in, and then this whole thing was heated up.

WIGNER: So the solder melted.

CREUTZ: The solder would melt. I don't know how it's done now.

BABCOCK: Actually, they put the uranium into the aluminum-silicon alloy. You coated the whole. . .

WIGNER: Aluminum-silicon alloy. Which was solid?

- CREUTZ: Yes.
- WIGNER: And how did you get the heat contact well established?
- BABCOCK: Molten.
- WIGNER: You melted the aluminum silicon.
- BABCOCK: The way you did it is you put the can inside a steel container. You took the slug, and you dipped it into aluminum-silicon alloy. Then you poured.
- WIGNER: That was molten alloy.
- BABCOCK: Molten alloy, and it was wet, and it coated all over. Then you poured the can full of aluminum-silicon alloy. Then you shoved a slug in there, and the aluminum-silicon alloy sprayed around, but it sticks.
- WIGNER: Good. I didn't know that. What is the melting point?
- BORST: How did you put the cap on?
- BABCOCK: You put the cap on by trimming it.
- BORST: At the same time?
- BABCOCK: As a final thing, yes.
- WIGNER: What is the melting point of aluminum silicon alloy?
- BABCOCK: I don't know, but it's a few degrees below aluminum. Aluminum is about 650 degrees, and I think this was about 630 degrees.

GOLDBERG: So it was just enough to do it.

BABCOCK: Just enough.

WIGNER: I see. And how thick was the aluminum silicon alloy at the end?

BABCOCK: Well, at the end, the can had a cap on the end, about a quarter of an inch thick.

WIGNER: A quarter of an inch? Four millimeters?

BABCOCK: It may have been a little less than that, but not much.

WIGNER: But aluminum silicon alloy was four millimeters thick?

CREUTZ: No, no. The aluminum cap.

BABCOCK: The aluminum can had an aluminum bottom in it.

WIGNER: Yes, that I know. But the aluminum silicon alloy, how thick was that?

BABCOCK: Oh, it was a few mils--I'm going to say 20 millimeters?

WIGNER: A few mils. In other words, it was essentially nothing. I mean, very thin.

BABCOCK: Just enough so you could push the can, put the slug in, and the solder would spray out.

WIGNER: More than a few mils, but very thin.

21:19:36:00

BORST: To quote more folklore, I had heard that this was such an impossible job that it couldn't be put into production. But the DuPont engineers went to work, and they set up a production line which had stoplights. When the stoplight changed from red to green, you took it out of this and put it in there. And when it changed from green to red, you took it out of there and put it from there. The line just went right down the line, and they managed to control the time and temperature to the point where they could produce a very, very good product, and probably not more than 1 percent defects.

WIGNER: Fantastic!

BABCOCK: Oh, you're the wrong order of magnitude. A hundredth of 1 percent.

WIGNER: I thought I was informed of everything, and I wasn't, evidently. This was with the first few reactors?

CREUTZ: I think this is a good example, though, of the many things a project faced, where--"What about the uranium?" Well, you put aluminum around it. "Well, how do you put aluminum?" It took many months to develop the ideas and more months to even. . .

WATTENBERG: . . . make ten-mil thick aluminum cans.

BABCOCK: They were near 20. I don't remember exactly, but about 20.

21:20:56:00

WATTENBERG: Because we didn't have the die-cutting tools we now have for aluminum cans. I wanted to ask a question. We earlier heard from Dr. Wigner, Professor Wigner, about the graphite

swelling and the displaced atoms. Did people worry at all about the uranium having displaced atoms and swelling?

CREUTZ: Very much. Very much.

WATTENBERG: That it would block up the cooling tubes? But we didn't have data on that 'til '44, almost.

CREUTZ: Actually, it's a higher temperature phenomenon. You're up several hundred degrees centigrade.

WATTENBERG: But they started the tests on--but where were they irradiating them? Because I remember in '44. . .

CREUTZ: You don't have to even irradiate. Well, thermally cycle in uranium.

WATTENBERG: That's the change in lattice. But when you irradiate it, you can also get into a mess, and those experiments were after Hanford was started up.

CREUTZ: Some of those were, I believe, carried out at Oak Ridge, actually, some of the radiation effects on uranium.

BORST: There was a dummy tube through the reactor, in which you pushed Hanford slugs in one end, and you pushed them out at the other end. That was used over years.

WIGNER: I must admit I have no thought about this melting adjustment of aluminum to the uranium by aluminum silicon alloy. But that worked out well, apparently.

CREUTZ: DuPont really solved the problem on that, yes.

- GOLDBERG: And it was a real roadblock at the time.
- CREUTZ: Oh yes, it was terribly worrying.
- WIGNER: It was close to the people.
- CREUTZ: That was a welding job.
- WATTENBERG: The Chicago pile Number Two really had, as its mission, to work on problems associated with or the construction of Hanford. A couple of mornings a week we devoted the pile to just testing every batch of every slug of uranium that was being made. We did tests of the shielding that was going to be used out at Hanford, for Zinn and Marshall, I think. We were very preoccupied with the problems for Hanford. So we were doing testing there of everything that could possibly be tested. I know we had a little discussion, but we couldn't do an irradiation test at our place.
- BORST: That's right.
- WIGNER: That's very interesting, what I just heard. What was the number of tubes in the reactor? About 400?
- CREUTZ: I don't remember.
- BABCOCK: Are you talking about Hanford, or are you talking about. . .
- WIGNER: Yes.
- BABCOCK: 2,004.
- WIGNER: 2,004 tubes.

BABCOCK: Plus or minus. [Laughs]

WIGNER: Yes. And in every tube were how many slugs?

WATTENBERG: They were 8 inches long.

BABCOCK: They were 8 inches long, plus about 1/2 an inch for end pieces. And how many that comes out to be, it's about 25 or thereabouts. I don't remember.

WIGNER: Ten thousand slugs in these?

BORST: Fifty thousand.

WIGNER: How many?

BORST: Sixty thousand.

WIGNER: And then we had to hope that this system works on all the 60,000. And did it work?

BABCOCK: It worked. We had I don't know how many failures, but it wouldn't use up all my fingers.

WIGNER: That's phenomenal. Sometimes I counted on a little bit of failure, and the radioactivity is too bad, but we forgot it.

CREUTZ: The real worry was not so much the--of course, it was the radioactivity, but also if it leaked, the slug would swell and might get caught in the tubes.

WIGNER: Yes.

BORST: Wasn't there a worry first about warping?

CREUTZ: Yes.

BORST: The uranium did grow on thermal cycling.

21:25:24:00

GOLDBERG: Let's talk a little bit about the xenon problem. You were involved in that right from the beginning.

BABCOCK: Yes, that's right.

GOLDBERG: Now, in many of the accounts that you now read, the claim is made that there was an argument between the Chicago scientists and the DuPont engineers about whether or not you should actually include those tubes. It was fortunate that DuPont wanted to, as it were, complete the square. And it was fortunate that they did because of the xenon poisoning problem. Was it just a matter of chance that this happened?

BABCOCK: Let me go back just a little bit in time. John Wheeler, which, of course, is a very famous name. He was on loan to the DuPont Company for this period that we're speaking about. I headed the so-called Physics and Control section, and to have a guy like John Wheeler in my group was an anachronism. That's just all there is to it. He actually was the boss, and I was his assistant, but there was no problem about that. One of John's problems was he had assigned to him looking after K.

WIGNER: After what?

BABCOCK: K, the multiplication constant.

WIGNER: Oh. K.

21:26:48:00

BABCOCK: And to see how close we were skating to the limit. It turns out that Compton, after we had made some changes in the design, sent an official letter to the DuPont Company saying this is his estimate of what the multiplication constant would be under these circumstances. Well, John Wheeler saw that and was very much concerned about it, because it was just barely enough, particularly when you took his pluses and minuses. Say they're all minus. Then there was no allowance in there at all for having fission product poisoning. So he made a very, well, reasonable estimate of what it might be. He didn't try to be too pessimistic, nor did he try to be too optimistic, and decided that we had real problems, and we ought to do something about it to get some more reactivity. Well, now then, we knew where more reactivity could come from, and the main thing was to take the slugs and lengthen the slug chain out, not have so much reflector. That was item one. The second one was to fill in the corners of the pile. It was a square pile, and you put a circular cylinder inside of it. So that leaves something like 25 percent of the space wasn't used up. We then said, "We will fix this so that we can use that space."

Now then, you must remember that at this time, the Manufacturing division was very, very anxious about their schedule. They had that on this day, we were going to start the reactor, and on this day we're going to move some slugs out and whatnot. They were very concerned about that schedule slipping. John and I wrote the memorandum to our management. And what we said was, "These are the things that should be done." Well, now then, the Technical Division approved it; that's Crawford Greenewalt. The Manufacturing Division had to be sold on it. I mean, they had to agree to it or we wouldn't do it. This thing came to a head on a Saturday afternoon. The general manager of the division was not there; Greenie wasn't there, who was the technical director. But the Manufacturing Division had their head and their assistant head. Now then, in the Technical Division, George Graves was the assistant

manager, and I was one of the people down the line. So George and I made a date with the manufacturing people to discuss this. I gave the technical basis for where we were, largely, of course, based on John Wheeler's calculations. John wasn't there either, or he would have been there, but I was the boss technical physicist in the DuPont Company for a full half a day.

Well, Manufacturing, of course, didn't say yes right away. They were concerned about their schedule. And what they said was that they would not make their decision until Monday or Tuesday, when they could get the information back from their suppliers to see what this would do. Now, I haven't mentioned other things that we asked to have done. We asked that all the holes be increased in diameter so we could increase the volume of metal in the reactor, and a few other things such as that.

Well, I'll leave it there and come on until Monday or Tuesday, and they found that the shield blocks that the slugs had to go through were all ready for shipment. But they would be willing to stop shipment of any that were ready to go, bore all the holes, which was 2,004, out to a full diameter, and the shield blocks, which weren't bored at all, they would be bored out. And there were estimates that that would delay the project two weeks. Manufacturing decided that that was an acceptable delay, particularly in view of the fact that the delay might have been a great deal more than that if we had needed to do that.

After things got tooled up and whatnot, the delay was not two weeks, but something over one week, and they started shipment out. Now, that doesn't mean they had them all done, but they were able, then, to get into the production line and ship out 200 or so shield blocks a week, or whatever it was they had.

Now then, that is really all the story about K. We actually got a large amount of extra K out of that, but we did not pipe in the 500-and-some extra tubes that went in the corners. I blame myself for that. I am sure that if I had stood up on my hind legs and said, "We have to do this," I'd either got it or I'd been fired. Just as simple as that. I didn't do it.

WATTENBERG: I think there's something that isn't perhaps appreciated in building a reactor. You can't build it oversized.

WIGNER: You can't. . .

WATTENBERG: You should not build it oversized. Because the control of the reactor takes place with a half a percent of the neutrons. So if you build it oversized, so that you have 1 percent extra instead of a half-percent at the half-percent level, you have seconds or minutes to control a reactor. If you go over by a full 1 percent, you have a millisecond, one-thousandth of a second, and you lose control. So that's why we physicists always worried about having too much excess reactivity or too large a reactor. You folks must have compromised in a very nice way, because you didn't get into that problem.

21:33:34:00

BABCOCK: I want to go into that subject just a little, if I may. We in DuPont were very anxious to get as much excess K in as we possibly could, and we also recognized exactly what you're talking about, that we had to be able to control that excess K. We had said that we wanted nine main control rods, which we could work in and out. If you're only going to have a minimum of excess K, two or three is all you need. But we asked for nine. And in addition to that, we had up our sleeves other ways of getting control. Now then, I'm now jumping to the time when the reactor stopped due to xenon, and we saw that we were going to have to put excess K in there, and we eventually got up to 1,500 tubes. Our control system was just barely good enough. And if you say it was just barely not quite good enough, well. . .

WATTENBERG: You wouldn't argue. [Laughter]

BABCOCK: I wouldn't argue. So we then, before we loaded any more tubes, we made an auxiliary system. In other words, what we did is we took out a few fuel tubes and put in. . .

WATTENBERG: Empties.

BABCOCK: Slugs in there, the poisoned slugs. Then as it warmed up and we didn't need them anymore, we needed reactivity, we reversed it.

WATTENBERG: So you had dummy controls or dummy safety rods.

21:35:08:00

WIGNER: If I may say so, I am amazed by all this. I did not know. I thought that you took the danger of one or two tubes, the slugs going out of order into consideration, and well, a little radioactivity will go out, but we get the plutonium reasonably fast. But that is not the situation. Evidently, you learned a great deal from the time that you asked us to review it, and you made decisions which you did not communicate to us, because you didn't think it is desirable. Is that correct?

BABCOCK: I'm just plain not with you. I didn't quite understand what we did that we didn't communicate to you.

WIGNER: Wel, just what you said, this aluminum-silicon cover. At least I did not know.

BABCOCK: Well, I'm sure that that had been communicated to the Met Lab. I am sure of that. You knew about it.

WATTENBERG: No, I wasn't reviewing what you were doing. Ed was closer.

CREUTZ: Yes, I was closer. At that time, John Chipman was in charge of metallurgy at the Metallurgical Laboratory. And he was certainly well familiar with the fact that there was. . .

WIGNER: Evidently, we were not informed. It is evidently a very clever system. We thought you take the chance that something goes wrong and worse things have happened before. Because we thought that it was very urgent to have some plutonium. But you did not feel that?

BABCOCK: Well, we thought that this was a pretty good system.

WIGNER: Yes, that I see. I did not. . .

BABCOCK: I'm quite surprised that you didn't know about it. That surprises me.

CREUTZ: I'm surprised, too, because there were. . .

WIGNER: Maybe I am just too stupid.

CREUTZ: You're not stupid, but we all do forget things. There were, of course, monthly reports from the Metallurgy Division of the Metallurgical Laboratory.

WIGNER: Maybe I was. . .

CREUTZ: I can't imagine, although it can be checked, that this would not. . .

WIGNER: I think I will express this to my friends who were-- well, Alvin Weinberg is one of them.

BABCOCK: I'm talking about something that was done out at Hanford after I was out there, and I was out there three months before the startup. So the time span which this was developed in and accepted was pretty short. I'll say two months.

WATTENBERG: Was this '44?

WIGNER: A wonderful thing. I did not know it.

GOLDBERG: The reactor itself went critical. . .

BABCOCK: September of '44.

GOLDBERG: September of '44.

WIGNER: In the beginning, of course, we had some gross mistakes, and we corrected those. But evidently, they learned a lot by working on it and by thinking about it and introduced these tricks. We thought that it might happen that something goes wrong.

[Note: Cross-talk on tape]

WATTENBERG: I think what this illustrates is the number of technical problems that were solved by fairly clever people like Dale Babcock and the staff that worked with him and also at the Lab. There were enormous numbers of problems with this.

WIGNER: It would be very interesting to have it in writing.

GOLDBERG: It's something that can be checked from the monthly reports at the Metallurgical Laboratory.

CREUTZ: I would think so.

- WIGNER: I will go, as a matter of fact, tomorrow, to Oak Ridge National Laboratory and inquire from others whether they knew about this. I surely did not.
- 21:39:32:00
- GOLDBERG: When did xenon poisoning, when the reactor went dead after startup because of the production of--I guess the parent of this is iodine 135?
- BABCOCK: Yes.
- GOLDBERG: And that decays in a couple of minutes?
- WATTENBERG: No. Several hours.
- BORST: Eight hours.
- GOLDBERG: Eight hours, to xenon?
- BORST: 135.
- GOLDBERG: Which has about a nine-hour half-life. And once that decays, then the reactor would start up again.
- BORST: Oh, yes.
- GOLDBERG: How long did it take for you to figure that out?
- BORST: One night.
- WATTENBERG: The stories are two days.
- BABCOCK: No. I'll give you the story.

WATTENBERG: Good.

21:40:11:00

BABCOCK: This is one who was there and was sweating. The pile died. I was not there at the time. It died at night. I learned about it the next morning. Crawford Greenewalt had been out there. We were staying in the transient quarters, and he told me about it and said that this had happened, and that we were going to have to do something about it, and that John Wheeler had a theory as to what it might be. Well, as soon as I got out to work and got started, why, John said, "This is what it might be." What he wants to do is to get the piece of paper that recorded the shape of the curve of which the dying out of the reactor took place. He got that-- I believe it was over the phone-- and said, "It is a parent-daughter decay, and therefore it has to be something that is relatively small neutron poisoning decaying into something that is a very, very high neutron poisoning." He selected three pairs from the data that was known on cross-sections. I don't remember now what they are, but anyway, John usurped my position as boss and says, "Dale, you and Paul [Frederick] Gast work on this one." He would work on this one, and Fermi and Mrs. Woods--Leona--I was trying to figure her first name--Leona would do the other one. Well, if that isn't two giants and a grasshopper, I never saw one. But at any rate, I think Leona and Fermi got theirs finished first, and they said, "Ours isn't it." That was the end of it.

Then John and who he was working with--I think it was--well, it doesn't make any difference--theirs wasn't it. So it came over to ours, and it had to be ours. Well, John took over the calculating, and I found that all I was doing was writing the points down, which is a very important thing to do. A lot of people were watching it. But it was obvious that on that first trial that John made of his assignment of various constants to it, we fit the decay curve very, very rapidly. And it was good.

WATTENBERG: I had the impression that they were worried also of the possibility of a thermal effect that hadn't been anticipated, and that you actually tried to recycle thermally before you-- that isn't true?

BABCOCK: No. I think I take credit for thinking that the pile went down because of a leak of water in one of the tubes. Now then, from all my experience, that was the most logical thing, and let's try it.

WATTENBERG: That's what it was.

BABCOCK: So we then started to dry the reactor. We were circulating helium, and the helium was moderately hot. As I remember, we got about two quarts of water out. Well, two quarts of water in that whole thing was nothing. It would have had to have been ten gallons if we were going to worry about it at all. It was obvious that that wasn't it. But just about the time that we decided that that wasn't it, the reactor came to life--not as a result of getting the water out, but as a result of getting the helium out. So that's the whole story. Things took place very rapidly after that.

WATTENBERG: We, of course, got telephoned by Fermi or somehow or other informed by Fermi. I don't think we used the telephone, quite. But we were told, and so we started working at midnight that night to see if we could poison the heavy water reactor. Of course, we paid attention, and we were capable of creating that poison and observing the effect. Our problem which prevented us from doing it and seeing it all the time was a chemical acidity problem that we were producing, ammonia and things like that, due to the electrolytic reaction. We had to stabilize the pH of our system, and we were always doing that on a basis of daily "What does it need to fix it." So we had to really stick to another schedule to see the poisoning due to the xenon. But once we established that we could do it and held the pH in a steady way,

we then did one to establish that there was no other period that was going to show up that was shorter than 40 days. So again, we went to work for what was going to happen at Hanford.

BORST: It was about 48 hours before we got it tied down at Oak Ridge. We got a telegram from Hanford.

WIGNER: I did not know any of this, and we were supposed to review everything that the DuPont Company does. But evidently the DuPont Company learned an awful lot in the course of installing the reactor, because in the beginning, they made very many very obvious mistakes, as I mentioned once before. I think it would be interesting to learn more about this. Will you have another. . .

GOLDBERG: We will have another interview, or hope to have another interview with more of the DuPont people, in fact. This is coming down to the end now. I was hoping we'd have time to talk about political activity in Chicago in '44 and '45, but we're clearly not going to have time for that today either.

21:46:53:00

WIGNER: What is the situation with Greenewalt? Is he still interested in this?

BABCOCK: I would say no. Of course, you know he's retired as Chairman of the Board of the DuPont Company. He has a number of other private things that he is interested in, which he's taking care of as much as he can. But he is trying to shed off as much of this kind of load as he possibly can.

WIGNER: I see. What was your location in the DuPont organization? Were you second to Greenewalt?

BABCOCK: Well, you're talking about at the Hanford time?

- WIGNER: Yes.
- BABCOCK: Greenie was the technical director; George Graves was the assistant technical director. Then there were three people that reported to them--Hood Worthington, Lom[bard] Squires, and I.
- WIGNER: And you.
- BABCOCK: And me.
- WIGNER: I see. But you know, this whole thing amazes me, because it is something entirely new to me, which I did not know. I would very much like to learn a little more about it. Evidently, at least apparently, you did not communicate these changes to us.
- BABCOCK: Wel, now, on this particular one, I was in my office. Greenie used my telephone and called up Compton to tell him about this thing.
- WIGNER: I see.
- BORST: About the xenon.
- BABCOCK: Xenon. Right.
- WIGNER: And what did he say?
- BABCOCK: Well, let's skip that question. [Laughter]
- GOLDBERG: Compton was upset.

- BABCOCK: Compton was. . .
- WIGNER: Compton was director of the organization, and a very nice director.
- BABCOCK: Yes.
- WIGNER: But I am simply amazed that we knew nothing about this. I don't remember exactly when I retired from this job, and maybe some of it happened after that.
- BABCOCK: No, we're talking about 1944 now.
- WIGNER: I see.
- GOLDBERG: Well, we've gone through an awful lot today.
- WIGNER: Yes.
- GOLDBERG: But I feel like we're just beginning.
- WIGNER: Yes, I am simply amazed.
- GOLDBERG: I hope we can bring some of you back together again some time in the future and just pick up where we left off and continue.
- WIGNER: I will try to find out from other members, particularly Alvin Weinberg and Gale Young, whether they remember being informed of these things. Perhaps I have been, but I don't remember having been.

21:50:00:00

WATTENBERG: You know, our experience was that very often, some technician-level person would have a good idea.

WIGNER: Yes.

WATTENBERG: And that isn't true in building a production plant. But I'm sure that things happened in your production facilities where some clever technician figured out, "Why don't you first drill with a small drill and then with a bigger bit?" And a lot of things go on that make things possible if you have good working staffs and good technicians. You were referring earlier, very much earlier, to our keeping things clean and using carpenter's tools. We were blessed with a fabulous millwright and a couple of other very good technicians. And they just solved a lot of our problems for getting things. We first started with a metal planer to shape our graphite, for instance, and then it turned out that by using a wood planer, we were much better off and it went ten times faster. That's due, essentially, again to unmentioned people who are of a much lower caliber than most people we'd run into. Gus Knuth, an excellent tool and die maker at the University of Chicago. He really was fabulous.

CREUTZ: I just wanted to mention the same thing, that the machinists in the shop in Chicago were incredible. There was practically nothing you could ask them to do with any precision that they--first of all, they'd say, "I can't do that." But then they'd say, "Well, I can do this, though." They'd make suggestions, and I noticed that their hands were always clean. DiCostonzas and Wally Getzholtz. Of course, Tom O'Donnell was in charge of the shop there. A lot of these things couldn't have happened in the right way without machinists of that caliber.

WATTENBERG: I'd like to give an example of that. We were pressing this uranium oxide. When you use a press, you use lubricants usually.

WIGNER: What did you use?

WATTENBERG: We were using a press to press the uranium oxide thing. If you use a lubricant like stearates or something like that, you're putting hydrogen in, and that then absorbs neutrons. You're putting in a neutron absorber. It's because of DiCostonza that we essentially could get by with only a film of lubricant on our dies, he just made them so beautiful. Things like that. Those problems were solved, and they're not in the record, and they don't filter down necessarily.

GOLDBERG: It's sort of like after the war, when Percy [Williams] Bridgman won his Nobel Prize, he gave half of it to his machinist who made the high-pressure plugs for his research.

BORST: And he deserved it.

WATTENBERG: But when we had accidents of lack of cleanliness, if you want to know, and there were several times when we had to vacuum--I mean, some truck came in and dumped something, or we broke down through a floor and all of this dust settled all over the graphite, and we just had to vacuum all of our graphite to get rid of it, or somebody started washing the toilets with borax. [Laughter] Lots of things. I think we had awful good technicians and other people who just caught them and noticed what was going on, and we cleaned up.

GOLDBERG: I think with that, let's call a halt. Thank you very much.

WIGNER: Thank you. It was very interesting for me. And I am flabbergasted.

GOLDBERG: Your flabbergastedness will be recorded forever!

WIGNER: [Laughter]

BABCOCK: Are we off? I can now tell you what it was Dr. Conant said Compton said. He said, "What in the world has happened to Norman Hilberry out there? It must be that the DuPont scientists have chloroformed him!" [Laughter]

21:55:00:00 [End of interview]

[End of VHS Tape 2 of 2]

[End of U-Matic Tape 4 of 4]

SMITHSONIAN VIDEOHISTORY PROGRAM

Processing Log:
Procedures followed in the preparation of manuscript

RU 9531

Project Manhattan Project/Oak Ridge Session (s) Four - Eight
[C.D. 2]

X Transcriber completed verbatim transcript

X Transcript reviewed by staff for transcription errors:

X a) by comparing transcript with taped record in its entirety

X b) by comparing transcript with taped record where accuracy of transcription was in doubt

X Transcript reviewed by interviewer to resolve both editorial and content questions

X Cross-referenced with videotape by including time-code signals and visual cues (abstracts of visual information)

X Transcript printed and proofread

X Visual and oral record has been preserved

INDEX

- "Atomic Energy for Military Purposes" (Smyth Report) 106
Alexander, Peter Popow 73
All In Our Time 23
Allison, Samuel King 26, 48, 66
Anderson, Herbert Lawrence 23, 38
Argonne National Laboratory 21
Berkeley, University of California 49
Beryllium 6, 16, 26-29, 48
Bismuth 57, 58
Bismuth cooling 84
Boron 17, 18, 61
Bothe, Walter Wilhelm 7, 17
Bourke, Neils Henrick David 2
Breit, Gregory 11, 22-24, 77
Bridgman, Percy Williams 137
Brush Beryllium Company 28, 29
Bureau of Standards 17, 20
Bush, Vannevar 33, 67, 76
Cadmium sulfate 66
Calumet and Heckler Copper Company 28
Carbon 17, 20, 26
Carbon dioxide 61
Chain reaction 5, 7, 14, 20, 26, 53, 54, 68, 72, 77, 108
Cohen, Karl Paley 92
Columbia River 95
Columbia University 14-16, 22, 38, 49, 51, 59, 60, 93
Compton, Arthur Holly 25, 26, 34, 48, 50, 67, 76, 77, 91,
108, 124, 134, 135
Conant, James Bryant 33, 43
Cooper, Charles Burleigh 55
Copper 29
Cyclotron 12, 26, 46, 53
D2O (heavy water) 32, 33, 42, 43, 45, 46, 84, 92
DuPont Company 42-44, 56, 63, 67, 74, 77, 82-85, 87, 92,
94-96, 105, 114, 116, 119, 120, 123-126,
133
Eckert, Alfred Carl, Jr. 26, 47, 52
Einstein, Albert 19
Exponential piles 38, 60
Feld, Bernard Taub 38, 65
Fermi, Enrico 3, 8, 14, 15, 20, 24, 27, 34, 39, 40, 50, 53,
60, 68, 91, 109, 110, 131, 132
Fisk, James Brown 26
Fission 2, 10, 47, 48, 50
Foster, Michael 115
Geiger counter 7, 41, 72
General Electric Company 92
Graphite 15, 16, 18, 28, 29, 39, 53, 54, 60-62, 67, 80, 97,
105
 electrolytic 61
 purification 59

Graves, Alvin Cushman 69
Graves, George 124
Greager, Oswald Herman 84
Greenewalt, Crawford Hallock 43, 83, 84, 124, 131, 133
Grosse, Aristid Victor von 14
Groves, Leslie Richard 33, 55, 68, 74, 83, 95
Gurinski, David Harris 28
Hahn, Otto 2
Halban, Hans von 32
Hanford reactor 55, 57, 60, 80, 91, 94, 95, 104, 105, 121,
129, 133
Hanford reactor water cooling system design 82
Heisenberg, Werner 33
Helium 55
Helium-cooled reactor 55
Hilberry, Horace Van Norman 65
Hitler, Adolf 3, 34
Hutchins, Robert Maynard 51
Hydrogen 8, 15, 17
Implosion technique 78
Indium 41
Iowa State University 62, 73
Joliot-Curie, Frederic 5, 32
Kowalski, Lou 32
Leverett, Miles Corrington 55, 57, 81, 82, 84
MacPherson, H.G. 16, 17
Magnesium 28, 29
Manley, John Henry 79
Mardin, D.W. 70
Massachusetts Institute of Technology 92
Matthias, Franklin T. 95
May, Alan Nunn 79
Metal Hydridos Company 62
Metallurgical Laboratory 46, 55, 81, 92, 109, 127-129
Moore, Thomas 55
National Academy of Sciences 22
National Archives 17
National Bureau of Standards 23
National Carbon Company 16, 17, 59
Neddermeyer, Seth Henry 78
Newson, Henry Winston 105
Nichols, Kenneth David 74
Oak Ridge National Laboratory 16
Office of Scientific Research and Development 14, 67
Ohlinger, Leo Arthur 81
Oppenheimer, Robert 53, 96
Owing, Lee 57
Pearl Harbor 43, 50, 51
Petroleum 61
Plutonium 35, 76, 108
Princeton University 2, 4, 9-11, 15, 20, 22, 24, 36, 49, 59
Rashevsky, Nicholas 47
Renssler, John 70
Resonance neutrons 12

Rodium 41
Rommel, Erwin 35
Roosevelt, Franklin Delano 19, 20
Schenley Distilleries 18
Schockley, William 26
Seaborg, Glenn Theodore 52, 96
Sengier, Edgar 74
Smyth Report 106
Smyth, Henry DeWolf 92
Snell, Arthur Hawley 26, 53, 108
Spark spectroscopy 18
Spedding, Frank H. 62, 71, 73
Strassmann, Fritz 2
Szilard, Leo 2, 5, 6, 8, 9, 12, 14, 15, 17, 19, 20, 23, 26,
28, 29, 31, 53, 57, 58, 62, 71, 78, 84, 91
Teller, Edward 3, 19, 22, 91
The New York Times 10
Turner, Louis Alexander 36
U-233 converter 108
United States Army 67, 79, 94
 Corps of Engineers 100
University of Chicago 20, 26, 46, 49-51, 59, 68, 86, 107,
123
University of Wisconsin 11
Uranium 10-12, 20, 29, 47, 53, 60, 70, 78, 107, 120
 dioxide 11
 failure 107
 graphite 11
 metallurgy 57
 ore 74
 oxide 11, 13, 137
 radiation effects on 120
Uranium bomb 5
Uranium-235 7, 8
Uranium-238 10, 35, 36
Urey, Harold Clayton 42, 43, 49, 84, 91
Vernon, Harcourt C. 92
Weizacker, Carl F. von 37
Westinghouse Electric and Manufacturing 11, 73
Westinghouse Lamp Division 70
Wheeler, John 78, 123, 124, 131
Wilson, Robert Rathburn 8, 22
Woods, Leona 131
X-10 reactor 94, 95, 97
Xenon poisoning 104, 105, 123, 126, 130, 134
Young, Gale 47, 57, 81
Zinn, Henry 29, 72, 93, 121