Then, Sachs quoted from a series of lectures prepared by F. W. Aston in honor of Lord Rutherford on the theory of atomic research. The book, *Forty Years of Atomic Theory* reviewed the progress of atomic research in England and other countries. Sachs read the last paragraph of that work to Roosevelt:

> There are those about us who say that such research should be stopped by law, alleging that man's destructive powers are already large enough. So, no doubt, the more elderly and ape-like of our prehistoric ancestors objected to the innovation of cooked food and pointed out the grave dangers attending the use of the newly discovered agency, fire. Personally, I think there is no doubt that subatomic energy is available all around us and that one day man will release and control its almost infinite power. He cannot use it exclusively in blowing up his next door neighbor.[34]

Roosevelt evidently got the point of Sachs' presentation.

> The President remarked, "Alex, what you are after is to see that the Nazis don't blow us up."
> I said, "Precisely," and he then called in General Watson . . . . and he said, "This requires action."[35]

Early government support—The uranium committees

Watson organized an informal committee, first selecting the two military men most concerned with science—Lt. Colonel Keith Anderson for the Army and Commander Gilbert Hoover for the Navy—to serve on the committee. He appointed Dr. Lyman J. Briggs, director of the Bureau of Standards, the nations' government physics laboratory as the chairman of the committee.[36] Sachs sent a letter to Eugene Wigner, a respected physicist to help

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[34]Sachs, 9.

[35]Sachs, 9.

[36]Sachs, 9; Hewlett and Anderson, 19-20.

him contact interested scientists. The first Advisory Committee on Uranium met on October 21 with nine in attendance.[37]

Leo Szilard began the session with an overview of the possibilities of a chain reaction using uranium and graphite layered together. Edward Teller then addressed the group, and the issue of money for funding the project was raised. Commander Hoover insisted upon a precise amount, and Teller told him $6,000 would work for the first year so that the scientists could buy graphite. The amount was agreed upon, and a report of the meeting was sent to the President on November 1, 1939, claiming that a chain reaction was a possibility but still unproved. The group suggested that the government support a thorough investigation though and concentrate it in the universities of the country. The President noted the report, according to a memo from Watson on November 17, and decided to keep it on file for future reference.[38]

The government could not be pushed into any other action for several months mainly because there seemed to be so much debate on the feasibility of an atomic weapon. The scientists also tended to avoid military applications and concentrated their studies instead upon a chain reaction to develop nuclear power. The one fear—world domination by Germany—waxed and waned throughout those early months, and with it saw the rise and fall of interest in the uranium research problem. Just when government interest would seemingly die, Germany would cause a renewed interest in the weapon

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[37]Sachs, 9-11; Hewlett and Anderson, 20. Briggs, Fred L. Mohler a physicist of the Bureau of Standards, Richard Roberts a physicist of the Carnegie Institution, Sachs, Szilard, Wigner, Edward Teller, Anderson, and Hoover were the attendees.

[38]Hewlett and Anderson, 20.

by conquering another area of Europe. The money for graphite calculations was not transferred from the Army and Navy until February 1940.[39] Events in March renewed interest when *The Physical Review* began reporting that in order for a chain reaction to occur, U_{235}, the lighter isotope of uranium, must be used.[40] Several experiments helped close the gap on the possibility of a nuclear chain reaction, but no earthshaking discoveries came from the scientific community. Einstein, in the spring of 1940, sent yet another letter relating the experiments at the Kaiser Wilhelm Institute in Germany. At a meeting of the Uranium Committee in April, the émigrés still could not shake the Army, Navy, or the government into decisive action; the bureaucrats decided to wait until calculations of the uranium graphite system at Columbia had been completed before taking decisive action. At a meeting in June 1940, a request of $40,000 was made for continued research on the Szilard-Fermi experiments at Columbia. Before the money could be awarded though, other administrative events affected the future of the Uranium Committee.[41]

The organization of the National Defense Research Committee (NDRC) on June 27, 1940, placed the Uranium Committee within a new organizational structure[42] under Vannevar Bush.[43] The first contract awarded was the $40,000

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[39]Smyth, 47-48.

[40]Hewlett and Anderson, 22.

[41]Smyth, 49.

[42]Irvin Stewart, *Organizing Scientific Research for War* (Boston: Little, Brown and Company, 1948), 8-9. NDRC was to direct and coordinated scientific weapons research by issuing contracts to individuals, educational institutions, and industry. It was not intended to replace the Army or Navy and its laboratories but was supposed to expand the scientific role in national defense.

[43]Rhodes, 356; Kevles, 293-94. Vannevar Bush was a technical genius, an engineer who received a doctorate jointly from MIT and Harvard in 1916. He went on to conduct war

to Columbia University for continuation of the Fermi-Szilard experiments for a chain reaction.[44] The committee continued to operate in the same form until the summer of 1941, when it was enlarged, renamed the Uranium Section or S-1 Committee, and placed under the Office of Scientific Research and Development with subcommittees formed on uranium isotope separation, theory, power production, and heavy water.

After a year, NDRC showed some definite weaknesses since it acted primarily as a research organization, with little power in engineering aspects of the uranium problem. As a solution, Bush founded an umbrella organization to coordinate all scientific research related to national defense. On June 28, 1941, an Executive Order was signed instituting the Office of Scientific Research and Development (OSRD) within the Office for Emergency Management directly under the President of the United States. Bush became director of this new organization and took with him the Committee on Uranium. James Conant replaced Bush as head of NDRC, which became a division of OSRD delegated to make recommendations on research and development. Bush understood well the rearrangement of authority because he stated in an interview later in his life: "I knew you couldn't get anything done in that damn town unless you organized under the wing of the president."[45]

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research creating a successful submarine detector and in the 1920s experimented with analog calculating machines. He rose to the Vice Presidency of MIT and moved to the Carnegie Institution as president in 1939 to become closer to the hub of government work.

[44]Weart and Szilard, 117.

[45]Smyth, 51; Hewlett and Anderson, 41; Kevles, 299-301; Kevles, 301.

Science and National Security

In 1941, several events cemented the link between science and national security. Many American scientists became convinced about the merits of the uranium research program, partly with help from their British colleagues. When the British MAUD Committee[46] sent a report on its scientific progress to the U.S. scientific community, several American physicists became convinced about the possibilities of building a bomb. This committee was much like the U.S. uranium committee, except that it was made up of active working physicists instead of government bureaucrats. MAUD, in the spring of 1940, came to the conclusion that a bomb could be built.[47]

Americans were somewhat more convinced about the feasibility of a bomb after Glenn Seaborg and his research group identified plutonium in E. O. Lawrence's laboratory in California in early 1941.[48] In the spring of 1941, Lyman Briggs persuaded Vannevar Bush to initiate an independent review of the entire uranium project; Bush, in turn, asked F. B. Jewett, president of the National Academy of Sciences, to establish a committee. Jewett appointed a

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[46]Rhodes, 340. The committee was code named MAUD after the mysterious message Lise Meitner had cabled to an English friend: "MET NIELS AND MARGRETHE RECENTLY BOTH WELL BUT UNHAPPY ABOUT EVENTS PLEASE INFORM COCKCROFT AND MAUD RAY KENT. The message was believed to be an anagram for "radium taken." Later in the war, the committee found out that Maud Ray was a governess for the Bohr children who lived in Kent.

[47]Moore, 276-79; Rhodes, 329-330, 340-41. The committee reported that fast neutrons as well as slow neutrons could cause nuclear fission. The report included an estimation that U_{235} could be reproduced in a sphere small enough to make a bomb. The best method to produce the bomb—gaseous diffusion—would turn uranium into a gas thus allowing for the collection of the U_{235} isotope. The committee also severely criticized American scientists because they were doing nothing about the German menace.

[48]Rhodes, 352-355. In March 1941, Glenn T. Seaborg, a young chemist in Lawrence's laboratory, discovered a new element called 94, later named "plutonium" in 1942.

committee chaired by Arthur H. Compton of the University of Chicago, charging it to study the military importance of uranium and decide upon the level of expenditure needed for a concentrated government-supported effort in uranium research.[49]

Shortly after a meeting in May 1941, the committee presented its first report recommending an intensification of the research effort for at least six months.[50] Later that summer, an appropriation of $267,000 was made for uranium research, partly because of that first report and partly because of the reports on British research.[51] A second report produced recommendations on the engineering aspects of uranium research in the summer of 1941.[52]

A third report was commissioned and delivered in the fall of 1941, but not before Bush, at a high-level presidential meeting on October 9, 1941, was given a free hand to investigate the possibilities of making an atomic bomb.

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[49]Smyth, 51. Arthur Compton was a physicist already of some renown and the younger brother of Karl Compton, President of MIT, who had already had some interest in this general field.

[50]Rhodes, 365. The committee detailed three military applications of research: production of radioactive materials to spread on enemy territory, a power source for submarines and other ships, and explosive bombs. The committee also developed a timetable: a year was needed to produce and test the radioactive materials, the power source would require three years after a chain reaction was created, and bombs could not be ready before 1945. A priority was placed on obtaining a sustaining chain reaction.

[51]Smyth, 51. Briggs was still in charge of the budget recommendations for the Uranium Committee. He made pleas not just from the first report but based also on work that had been undertaken in England by MAUD. Briggs argued for continuing the first objective of a chain reaction. But he claimed that isotope separation was crucial for any military applications of uranium production and suggested that a chain reaction could occur in an airplane-carried bomb device.

[52]Hewlett and Anderson, 39. Compton traveled to South America that summer so William D. Coolidge, a physical chemist recently retired from General Electric, was given the charge to add some engineers to the committee and review the first report. Their review was sent to Bush on July 11, 1941, and supported the first report's recommendations. The emphasis was still on creating a chain reaction rather than a bomb.

Bush conferred with Roosevelt and Vice President Henry Wallace whom he had already briefed on uranium progress earlier in the summer. He outlined British research achievements, the costs of building a production plant, what little was known of German research, and the time needed to produce a weapon. Bush was told to expedite the work in every way but not to proceed on building a production plant since that would need more discussion and a different organization to carry out Policy considerations on these matters were to be restricted to Roosevelt, Wallace, Bush, Conant, Secretary of War Henry Stimson, and Army Chief of Staff George C. Marshall, transferring responsibilities from the Uranium Committee to a new, more secretive group around the president. The specter of German domination played a major role in the actions on the part of Bush and the president. By the fall of 1941, both men had seen the results of Hitler's campaign and coupled with reports from British research and reports from those escaping from Germany who insisted the country was progressing on atomic research, their inclinations were much stronger to support an ultimate weapon. Bush actually gained permission to finalize the bond between science and national security in that meeting.[53]

Bush received the third report on November 6, 1941, and the President was sent his copy on November 27. Compton had traveled extensively in October of that year to gather the information needed for this third and most important report.[54] In that third report, he issued a clear call to build a bomb,

[53]Hewlett and Anderson, 45-46.

[54]Smyth, 46-49; Rhodes, 373-376, 386-387; Compton, 53. It was probably the Mark Oliphant visit that helped Compton and several other Americans including Conant, Bush's right-hand man issue a clear call for a bomb. Oliphant from Britain toured the United States in the late summer and early fall of 1941 as an strong advocate for building a bomb. He relayed news of British research at a meeting of the Uranium Committee in August, using the

thus moving scientific research into practical, military applications tied to national security. The objective of the program in scientific research would simply be: "'to consider the possibilities of an explosive fission reaction with U_{235}.'"[55] A bomb with enough destructive power could be created by gathering together a mass of U_{235}. How much destructive power was still unknown, but something on the order of a few hundred tons of TNT was possible. The report evaluated methods of isotope separation including gaseous diffusion which the British were committed to and the centrifuge system in development at Columbia. It estimated that bombs could be ready in three to four years. Costs were difficult to estimate, but the most expensive part of the process, the separation of the isotopes, could cost $50 to $100 million and the production of bombs could cost as much as $30 million. The report was just what Bush wanted and could have been the impetus to push America headlong into the uranium research project except that Bush had received approval to proceed a full month before this report. It did verify, after the fact, what Bush had convinced Roosevelt of in the October 9 meeting: science could

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word "bomb" very clearly. He solicited assistance from the United States because Britain, according to him, did not have the resources, an estimated $25 million, to build a bomb. Since he was not immediately effective with the Uranium committee, he decided to attempt to convince the most enthusiastic scientist he knew in America, Ernest O. Lawrence. And convince him he did. Lawrence contacted Arthur Compton at Chicago shortly after Oliphant's visit and repeated his conversation with Oliphant. A special meeting was set in Chicago on September 25, 1941, because Lawrence was to be in town to speak on the occasion of an honorary degree to be bestowed upon James Conant, the chairman of the NDRC. The three men met and after a heated discussion agreed to push uranium research in the United States. Conant went back to report the meeting to Bush, and shortly thereafter the third report was commissioned. Compton described that meeting as the start of the wartime atomic research program. Conant, in his secret history of the Manhattan Project, indicated that men like Oliphant helped turned the tide in the American uranium research enterprise.

[55]Rhodes, 386.

be used in a practical, military way to protect the country's security. Roosevelt returned his copy to Bush two months later on January 19, 1942, with a note attached that said:

> V. B. OK—returned—I think you had best keep this in your safe. FDR.[56]

Bush took action long before that returned note. Lyman Briggs, at Bush's request, called members of the Uranium Committee to come to Washington on December 6, 1941, to talk about reorganization of the uranium work. Bush sent more detailed letters, dated December 13, 1941, to each of the primary administrative men to be involved in the expanded government-supported project. A planning board with Eger V. Murphree, a young chemical engineer as head, would oversee engineering planning studies and supervise any pilot plant efforts. Bush appointed three program chiefs in charge of physics and chemistry research: Harold Urey to handle separation by diffusion and centrifuge methods as well as heavy water studies; Ernest Lawrence in charge of small-sample preparation, electromagnetic separations, and plutonium (element 94); and Arthur Compton with responsibility for the chain reaction to produce plutonium and oversee weapons theory. Two weeks after Pearl Harbor, the first large contract was awarded to Lawrence for $400,000 to study electromagnetic separation techniques.[57] Arthur Compton went back to the University of Chicago to create the Metallurgical Laboratory. The uranium research project had begun in earnest.

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[56]Rhodes, 388.

[57]Hewlett and Anderson, 49-52.

The Metallurgical Project and Laboratory

The period from December 1941 to February 1942 revealed more effort devoted to the administration of uranium research than to research itself, as evidenced by the creation and organization of the Metallurgical Project under Arthur Compton at the University of Chicago. Compton, author of the three National Academy of Science reports, was already a committed and distinguished physicist in the field of atomic research when called upon to head a program coordinating theoretical studies on plutonium and building a reactor to confirm a sustaining chain reaction.[58] In 1939, he heard about the discovery of uranium fission but thought his research was far afield from nuclear physics. When he became active in the field of cancer research though, his friend at the University of California in Berkeley, Ernest O. Lawrence, convinced him to take another look at atomic research.[59] That involvement began when Jewett called upon him to head a review of recent atomic research and its military applications, a task Compton took enthusiastically.

On the afternoon of December 6, 1941, Arthur Compton first had lunch with his new bosses Bush and Conant and then proceeded to his hotel room to make arrangements for his new assignment. According to his recollections, he spent that whole afternoon and evening on the telephone making contacts to

[58]Riedman, 188-190. Compton, the son of a Presbyterian minister, was born on September 10, 1892. He received his doctorate from Princeton in 1916 and studied under J. J. Thomson, the famous English physicist, at Cambridge where he also attended lectures by Ernest Rutherford, a Nobel-winning physicist. Upon his return to the United States in 1920, he became a professor at Washington University in St. Louis where he concentrated his research on x-rays. Three years later, the University of Chicago appointed him professor of physics. In 1927, he received the Nobel Prize for work on waves and light.

[59]Riedman, 190.

coordinate his new enterprise. He called Fermi at New York and George G. Pegram, a physicist friend at Princeton, to get advice and talk of plans for the future. On December 7, 1941, he remembered going to New York to talk to Fermi and his colleagues about the direction of research and to get their assurances that the group would work with him. Out of great respect for Compton as scientist, they readily agreed to support the project.[60]

The S-1 Committee met in Washington on December 18, 1941, and Bush announced the official revamped uranium research organization. Compton sent Bush a memorandum on December 20, 1941, detailing his plans and proposing a preliminary budget for the project. For the time being, the project research would be centered in Columbia, Chicago, Princeton, and Berkeley. His budget of several million was approved when it would have been unfathomable only a short time ago.[61]

From December to February, Compton was immersed in organizational plans and problems. In January, he began a series of meetings to discuss centralizing the efforts at one site rather than have research sites at Columbia, Princeton, Berkeley, and Chicago.[62] The various discussions, often heated,

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[60]Compton, 72-73. Laura Fermi, 185. Laura Fermi summed up the respect for Compton in the following way: "Compton was a thoughtful and considerate person, who took no step without weighing its effects upon others. Perhaps because of this, whenever he expressed an opinion, it was interpreted as an order and accepted without much comment."

[61]Rhodes, 398; Hewlett and Anderson, 53-54. At Columbia and Princeton the building of a pile and corresponding physical measurements would require 80 men and $340,000 for six months; Chicago needed 150 people and $278,000; and Berkeley wanted 150 men and $650,000 to prepare U_{235} and plutonium. Compton asked for another $500,000 for pile materials.

[62]Hewlett and Anderson, 53-55. The first meeting occurred at Chicago on January 3, 1942, and the best he could accomplish was a promise to continue existing work at the various sites. The second meeting at Columbia on January 18 at least developed a preliminary program timetable: by July 1, 1942 to determine whether a chain reaction was possible, by January 1943 to achieve the first chain reaction, by January 1944 to extract plutonium (element 94) from uranium, and by January 1945 to have a bomb. In the afternoon of that meeting, the

culminated in the now-famous and often repeated sickroom episode. Compton called his main scientists to Chicago and when they arrived on January 24, 1942, they were ushered into the bedroom where Compton was battling the flu and a fever. He argued to move the site to Chicago to concentrate all the research from the various laboratories. He tried to convince the others at the meeting—Leo Szilard from Columbia, Ernest Lawrence from Berkeley, Luis Alvarez from the M. I. T. Radiation Laboratory, Richard Doan from Phillips Petroleum Research Laboratory, and Norman Hilberry from New York University—the merits of a Chicago location. Compton had already received enthusiastic support from the president and vice president of the University of Chicago; there were more men to draw from in the central part of the country since there were fewer involved in war-related work, and Chicago was centrally located between the east and west coasts. After endless discussion, the physically and mentally exhausted Compton, the noted consensus builder, made an arbitrary decision: the site would be Chicago and those at the meeting should join him in the research effort there.[63] The only objection came from Lawrence, and Compton later recounted that objection:

> "You'll never get the chain reaction going here. The whole tempo of the University of Chicago is too slow."
> "We'll have the chain reaction going here by the end of the year,"

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topic turned again to centralizing the research at one site. Compton and Lawrence dominated the discussion and both agreed that at least the chain reaction research should be in the same place, if not the whole project. They had agreed that the large cyclotrons at Berkeley could not be moved elsewhere, and Lawrence would remain in Berkeley to oversee that part of the research. However, Columbia and Princeton scientists did not want to move their operations either.

[63]Compton, 80-81; Rhodes, 399.

I predicted.
"I'll bet you a thousand dollars you won't," he challenged.
"I'll take you on that," I answered, "and these men are the witnesses."
"I'll cut the stakes to a five-cent cigar," countered Lawrence.
"Agreed."
I won the bet, but I haven't yet received the cigar. Maybe the five-cent variety is no longer made.[64]

After the meeting, Compton called Fermi who somewhat reluctantly agreed to come to Chicago as soon as possible, and sent his assistant, Herbert Anderson, ahead to prepare for Fermi's arrival. Eugene Wigner at Princeton was also called and agreed to come. Leo Szilard, who had immediately left Chicago after the meeting, was telegraphed in New York to join Fermi.[65]

Compton had previously discussed the wartime research project with President Robert M. Hutchins and Vice President E. T. Filbey. They had already talked about the location of a nuclear reactor and what adjustments might need to be made on campus to accommodate the research. On the morning of January 25, Compton went to see Vice President Filbey to get clearance for the moves of the scientists to Chicago and to discuss their locations on campus. Shortly, thereafter, the mathematics department, which shared quarters with the physicists in Eckhart Hall, was contacted and volunteered to move its entire operation to the library in order to make room for the expanded research project and the anticipated new personnel.[66]

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[64]Compton, 81.

[65]Rhodes, 400.

[66]Compton, 80, 82.

By February, the centralized project had taken its code name as the Metallurgical Project, so called, according to Compton, because

> we had been considering for some time setting up at the University an institute of metals, which indeed became a reality after the war was over. Thus our Chicago colleagues saw nothing surprising in a wartime metals research program.[67]

Compton was director of the Metallurgical Project and Norman Hilberry was appointed Associate Director. But now the project needed a centralized research facility or laboratory. Called the Metallurgical Laboratory, it was also organized and staffed during this period. Richard Doan, because of his experience in industrial organizations, was made director and scientists from around the country were brought to Chicago, including one from Iowa State College.[68]

The Ames Project

Frank H. Spedding: Chemistry division leader and Ames Project head

Sometime between December 1941 and February 1942,[69] Frank H. Spedding, a professor in charge of physical chemistry at Iowa State College in

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[67]Compton, 82.

[68]Compton, 83.

[69]In several interviews later in his life, Spedding claimed he was contacted by Compton on December 6, 1941, and asked to take over the Chemistry division. Though that is possible, it is somewhat unlikely if Compton needed to contact others to get Spedding's name. Also, the Metallurgical Laboratory was not organized until sometime in January and this would have been a more logical time to contact Spedding. There is also evidence that Spedding was in Chicago in January for several weeks according to some of his later interviews and interviews with Wilhelm making this a more likely time to be asked to head up chemistry research. In other accounts, particularly from the early Ames Laboratory publications, Spedding claimed he was contacted sometime in February. In any event, Spedding was officially hired on the project on February 21, 1942. Referenced in J. C. Sterns, "Letter to Frank H. Spedding on Hiring," March 19, 1942, the Frank H. Spedding Papers, Record Group 17/1/11, the Robert W. Parks and Ellen Sorge Parks Library, Ames, Iowa (hereafter cited as Spedding Papers). A memorandum was sent to President Charles Friley on

Ames, Iowa, was asked to take over the Chemistry Division of the Metallurgical Laboratory in Chicago. How a young professor of chemistry, who had not been directly involved in uranium research before 1942, could be asked to direct some of the most famous atomic chemists is puzzling at best, unless considerable information is provided about Spedding's background and the situation at Iowa State College in 1942.

Frank Howard Spedding was actually uniquely qualified to take over the Chemistry Division. His academic preparation had been meticulous, and his breadth of chemical knowledge in rare earth chemistry exemplary. Spedding was born October 22, 1902, in Hamilton, Ontario, Canada to American parents. The family moved to Ann Arbor, Michigan shortly after Spedding's birth where his father became a well-known, successful photographer. Spedding graduated from the University of Michigan in 1925 with a B.S. in Chemical Engineering and an M.S. in analytical chemistry the following year. Spedding worked under H. H. Willard and though he liked the man, he did not like the scientific area. Early in his studies, he received a taste of the hard work of experimentation. Given a 50-pound rock of pollucite or cesium ore by Professor Willard, Spedding was told to extract cesium from it and prepare it, preferably as a pure salt or chloride. By 1926, he gave his professor several kilograms of the salt.[70] Spedding almost had his moment of glory when he

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February 24, asking for the half-time release of Spedding to work on the Metallurgical Project (Referenced in Charles E. Friley, "Letter to Arthur H. Compton on Frank Spedding's Appointment," February 28, 1942, Spedding Papers).

[70]Frank H. Spedding, interview 1 with Elizabeth Calciano, Ames, Iowa, April 21, 1971, revised October, 1979 by Frank Spedding, transcript in possession of Edith Landin, Ames, Iowa, 5-6 (hereafter cited as Spedding, interview 1 with Calciano). This series of interviews

thought he had discovered a new element. He was set to call it Michiganium, but discovered that what he thought was a new element were actually impurity traces in the materials with which he was working.[71] As he was about to finish his Master's degree, Spedding met one day with a professor he had in his undergraduate days, Moses Gomberg, who suggested that he go on to Berkeley and work under G. N. Lewis, head of physical chemistry at the best school in that area in the country.[72] Gomberg wrote a recommendation and despite the fact that Spedding applied late, he was offered a teaching assistantship and placed in Lewis' prestigious research group.[73]

The advantages to a young scientist coming to Berkeley were obvious. Almost every nationally known and internationally known figure in chemistry and physics found his/her way to lecture or present seminars at Berkeley Equipment was readily available to conduct theoretical or experimental studies. G. N. Lewis was also the consummate scientist, and Spedding later estimated that clearly one-third of all physical chemistry department heads in the country had studied under this master chemist.

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was conducted in the late seventies and early eighties by the daughter of Frank H. Spedding. The author was given permission to view the transcripts of interviews that were revised by Spedding, those in possession of Edith Landin who served as Spedding's assistant at the Ames Laboratory.

[71]Spedding, interview 1 with Calciano, 7-8.

[72]Harry J. Svec, "Prologue: F. H. Spedding (Oct. 22, 1902-Dec. 15, 1984)" in *Handbook on the Physics and Chemistry of Rare Earths*, edited by K. A. Gschneidner, Jr. and L. Eyring, Vol. 2 (London: Elsevier Science Publishers, 1988), 1-2. Some of the material for this book was taken from a Svec interview with Spedding in 1984, that was reprinted in various forms for several Ames Laboratory publications for Spedding's eightieth birthday and for a narrative to nominate Spedding for the J. B. Priestly medal.

[73]Spedding, interview 1 with Calciano, 1, 14.

Lewis encouraged discovery and experimentation in any field of interest When asked to define physical chemistry once, G. N. Lewis was reported to have said it was anything that interested him.[74]

Frank Spedding was put to work proving and disproving several of Lewis' far out theories [75] In addition to learning about experimentation, Spedding also developed an approach to science that would be used later in the organization of the Ames Project. Lewis did not teach chemical theory. Instead, his students were taught that scientists had to experiment and stand up to scrutiny by their peers through an ongoing examination that took place under the seminar system. Spedding later described that seminar experience vividly. It generally met on Tuesday night from 7:30 to 9:00 or 10:00 p. m. in a special room with one long table in the center where faculty sat and a eight-inch platform surrounding the table where the students sat two rows deep. All senior faculty attended. Professor Lewis with his smoky cigars continuously filling the room often made the main speech, or a student would sometimes have to present a paper and stand for criticism. After about an hour, the moment arrived that the graduate students dreaded. Lewis would call on some unsuspecting student and ask him or her to present to the group a summary of his research in progress. Once Lewis called upon a student and she froze and ran from the room. Because that event somewhat disturbed him, Lewis began to stop students on Tuesday afternoon and tell them to be ready. Later, he called major professors and told them to have their students

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[74]"Frank Harold Spedding Turns 80," *Ames Lab Changing Scene* 8, 10 (October 1982): 2.
[75]Spedding, interview 1 with Calciano. 24.

ready for that night.[76] Spedding, cigar and all, would repeat those very scenes in his own organization during the war and long after the war with his infamous Sunday and later Thursday Speddinars (as they came to be known).

Spedding inherited from Lewis another scientific approach that would be crucial to the development of the Ames Project: good scientists tackled tough problems whether or not they knew anything about them. Once a question could be formulated or what the scientists were looking for could be identified, the problem was practically solved. That philosophy was sorely tested during the war years at Ames.[77]

Frank Spedding spent two years in research for Lewis without finding a problem to publish as a thesis topic. Finally, Simon Freed, another graduate student, just finishing his Ph.D. in the band spectra of rare earths, invited Spedding to work with him evaluating the magnetic properties of the rare earths at low temperatures. Lewis encouraged the experimentation but did not place his name on any of the publications since he was not directly involved in the research work.[78] Spedding finished his Ph.D. in May 1929, after writing his dissertation. There were numerous jobs for young Ph.D.s at that time, but Lewis offered Spedding an instructorship to remain at Berkeley, a common way for obtaining a long-term academic job at this particular institution.[79]

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[76]Spedding, interview 1 with Calciano, 39-41. In that interview Spedding admitted to modelling his own seminars after those of Lewis.

[77]Frank H. Spedding, interview 2 with Elizabeth Calciano, transcript in possession of Edith Landin, Ames, Iowa, n.d., 76.

[78]Spedding, interview 1 with Calciano, 30.

[79]Calciano interview 1, 30-33. Generally, a bright student would be offered an instructorship whereupon he would work one year at a time under the instructorship. If reappointment occurred four times, usually an assistant professorship with a three-year contract was offered. After one or two terms of those assistant professorships, the department

Spedding took the offer and, of course, expected to remain at Berkeley permanently. Unfortunately, the Depression intervened in October 1929, and Spedding had to settle for a series of one and two year temporary appointments, each adding to his knowledge about his specialties—spectroscopy and rare earths—but none adding to his job security or to his financial situation. In 1931, he received the prestigious but not lucrative National Research Fellowship, awarded by the Rockefeller Foundation, for two years of full-time research at Berkeley. In 1932, he received his old job back from G. N. Lewis for two more years at the salary of $1,000 per year.[80] By this time, it had become common for the younger men to take associateships for about one-half of an instructors pay.[81]

The year 1933 was an auspicious year for the young chemist, even though he still had no permanent academic appointment. Early that year, Spedding had obtained some samples of rare earths from a professor in Illinois and set about to prove a theory: the fine structures of the rare earth bands depended upon the adjacent atoms in the crystal in which the rare earths were placed. For this work he was awarded the Langmuir Prize for Chemistry in 1933.[82] Spedding was invited to receive the award and make a speech at the

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examined the person's credentials and if it wanted that person as a permanent faculty member, an associate professorship with tenure was offered.

[80]Svec, "Prologue," 1988, 4.

[81]Spedding, interview 1 with Calciano, 35.

[82]"Frank Harold Spedding Turns 80," 3; Svec, 1988, 4-5. Spedding was only the third chemist to win the award, following Oscar K. Rice and Linus Pauling. He was the last under 31 to win it. The next year the award name was changed to the Award in Pure Chemistry of the American Chemical Society and awarded to young chemists under 35.

Chicago World's Fair. When he finished his speech and received his award, he noticed an older man approaching the podium. In his later years, Spedding remembered the old man:

> [He] was short, had a long white beard and was bald. . . . He blurted out "How would you like to have a pound of europium and two or three pounds of samarium?"[83]

Of course, Spedding thought the man was crazy. As far as he knew, those rare earths were available only in milligram quantities. He answered the man politely and told him that would be fine; it would certainly help his work to receive europium and samarium. After he arrived back in California, a box containing fruit jars of europium and samarium oxides arrived from this odd man in Chicago. As it turned out, his patron was Herbert McCoy, a professor of chemistry from the University of Chicago. Spedding started a correspondence with the man that lasted until McCoy's death in 1945. McCoy befriended young chemists like Spedding and provided them with quantities of rare earths for their research, only charging them cost or nothing at all in some cases.[84]

In 1934-35, Spedding won the Guggenheim to work abroad in Germany with two physicists, James Franck, a Nobel Prize winner, and Francis Simon, an expert in low temperature physics. Unfortunately, before Spedding could finalize his trip plans, Hitler came to power and both men fled the country.

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[83]Svec, "Prologue," 1988, 5.

[84] Svec, "Prologue," 1988, 5-6. It was probably Herbert McCoy who later got Spedding the job as Chemistry Division chief and even Compton in *Atomic Quest* confirmed that it was McCoy who recommended Spedding to him (p. 93). See also Norman Hilberry, interview with George Tressel, 1967, Transcript of Reel 2, In possession of author, Ames, IA, 4. Hilberry also indicated that it was McCoy who suggested Spedding to the physicists when they met in early February 1942.

Spedding went instead to England to work at the famous Cavendish Laboratory with Ralph Fowler, a noted theoretical physicist. He also traveled to other laboratories in France and Germany and spoke in Leningrad. The Speddings, Frank and Ethel, also went to Copenhagen for a month where he worked with the eminent Niels Bohr. After this time abroad, Spedding and his wife returned to the United States. Still with no permanent job, Spedding took another temporary two year position, the George Fisher Baker Assistant Professorship at Cornell University for 1935-37.[85]

Spedding, working with Hans Bethe a colleague at Cornell, resumed his previous rare earth research and continued to depend upon Herbert McCoy for a supply of rare earths.[86] When the two year stint was over, Spedding was in the same situation, with no promise of a permanent job. He decided to try his luck again in Berkeley, so he and Ethel packed their old Chevrolet and turned west. Spedding had heard of an opening at Ohio State, but when he arrived, William Lloyd Evans, the chairman of the department, had just hired a chemist the day before. Evans told Spedding that his friend Buck Coover had an opening at Iowa State College for a physical chemist, and Evans even

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[85]Svec, "Prologue," 1988, 5-7; Frank H. Spedding, interview with Harry A. Svec, Ames, Iowa, September 1984, transcript in Spedding Papers, 3-4. For a very detailed account of the trip abroad see one of the chapters in Edith Landin's possession called "Year in Europe—1934-35." This manuscript of several chapters was dictated to Ms. Landin, Spedding's assistant, hired to help him prepare a book he wanted to publish on his life. Unfortunately, Spedding died before he could publish the work. Dr. Spedding's daughter has given this author permission to use material from this book in this dissertation. It was dictated in the late 70s and early 80s and much of the material is duplicated in the Calciano interviews. The "book" is a collection of chapters with some paged, but none arranged in a definite order (hereafter cited as Spedding Manuscript).

[86]Spedding, Letters to Harold McCoy, June 28, 1936 and January 20, 1937; Letter from Harold McCoy February 8, 1937, Spedding Papers. McCoy's only demand for giving Spedding rare earths was that he continue in the field of low temperature research, a research field of none of the other eight to ten receivers of McCoy's largess.

offered to write a letter of reference. Spedding traveled on to Ames, Iowa and talked to Coover who offered him the job on the spot. Spedding tells the rest of the story:

> [Coover] was a horse trade and he said, "Now I can give you an appointment if you take assistant professorship."
> And I had guts enough to say, "No, I've been seven years on temporary appointments and I'm looking for a job with tenure, so I won't take anything less than associate professor."
> So he said, "I'll have to go to the Board of Regents."
> I said, "Fine. I'll go on West and you can wire me."[87]

Spedding traveled to Yellowstone National Park and after a week of not hearing, he decided to move on to Berkeley. They started to leave, but first Ethel stopped at the bathroom. Spedding remembered:

> While she was in there, I was over looking at the bulletin board. There were a lot of telegraph messages on it. There was a note on there to Dr. Spinolza, and I read it. And it said: Regents granted you an appointment. And I thought it sounds just like what I'm looking for, but the wrong name. Anyway, I got on the phone and called Coover, and it was his message; they just got the name all wrong. It was just a minute's difference of whether I'd got it or not. . . . I wouldn't normally have chosen the place. I was desperate; I hadn't been able to get a job except fellowships for seven years, and I thought, "Well I can go there and build up Physical Chemistry and when things really open up, I can go to another school."[88]

So Spedding, the nationally known chemist from Berkeley, found his way to Iowa State College in 1937 to take over the physical chemistry section. In early 1942, Arthur Compton invited Frank Spedding to participate in the

[87] Spedding, interview with Svec 1984, 6.

[88] Spedding, interview with Svec 1984, 6. At the time, when Spedding corresponded with his friends or colleagues, he presented Iowa State in much better light. In a letter to McCoy on November 10, 1937 (Spedding Papers), he said: "So far, I have liked my new position very much as I am able to run things just to suit myself and the research opportunities are very good."

atomic bomb project. According to Spedding, Arthur Compton decided that he needed expertise in chemistry and metallurgy to complete his project satisfactorily. At this time very little about the chemical qualities of uranium and its byproducts was known. Spedding was chosen as head of the Chemistry Division, but because there was little room for the number of scientists needed, Spedding volunteered to start chemical and metallurgical research on the Ames campus. Spedding later related:

> They had vastly underestimated the amount of chemistry that had to be done. So that when I arrived at Chicago, they were allowing two rooms for the chemists to do all the chemical work and I informed [Compton] that two rooms would be woefully inadequate. . . . So I told Dr. Compton that they had to have a lot of chemical and metallurgical work done immediately, and we couldn't do it at Chicago until we built a building and till we got some staff together. . . . But it takes time for people to pick up and move, and I told him that we had a metallograph and we had a furnace here at Ames and that we could get some of this work going. And so after he deliberated a week or two they decided . . . that I would spend half a week in Ames . . . testing out various things that might be used in a reactor.[89]

The only other professor in the section was Harley A. Wilhelm, an instructor who had graduated from Iowa State in 1931. Wilhelm had held the area together and was the College's only spectrochemist. When Spedding came in, that was his area too, so he took an old spectrograph that had been ordered in the 1920s by Anson Hayes, a physical chemist of some renown that Iowa State had lost to industry. Spedding gave Wilhelm the area of metallurgy.[90] Spedding soon found his teaching load heavy; he served on a large number of

[89]Interview with Frank H. Spedding, Harley Wilhelm, and Adrian Daane, May 1967.

[90]Wilhelm, interview with author, 1990.

Ph D. committees; and the equipment to do the type of research he wanted to conduct could not be ordered because of the lack of funds.[91] He even had to temporarily change his emphasis from the rare earths to topics of an agricultural emphasis, an area of research in which he could find College funds.[92]

Iowa State University: The location of the science laboratory

It is surprising that Iowa State College became the center for this uranium research, considering the state of research and science at the institution at the time. Spedding recalled Iowa State as typical of many midwestern schools. The student body numbered around 5,000 and was dominated by agriculture, engineering, home economics, and other applied

[91]In several letters to McCoy, Spedding tells of the deteriorating situation. On February 27, 1939 he tells McCoy: "I have been extremely busy getting this division organization and getting my teaching under way so that my research has suffered. However, I have finally assembled my equipment and expect to be producing at the same old rate shortly." On January 28, 1941, he again tells McCoy, "It has taken longer than I anticipated to get my research program functioning here at Ames but I expect to have it go full blast from now on." He tells McCoy about building a spectrograph and wood grating, but he does not tell him the frustrations of writing over two hundred companies to get the pieces necessary to build that equipment (Spedding Papers notation on a miscellaneous file).

[92]"Industrial Science Research Institute Progress Reports on Projects," Iowa State College, Division of Industrial Science, Office of the Dean, May 8, 1939, Ames Laboratory Papers, 5; Iowa State College of Agriculture and Mechanic Arts, *Announcement of the Graduate Division*, Ames, Iowa, 1945-46, 126-130. Spedding's first funded project involved his area of absorption spectra at low temperatures, but was for vitamins and organic materials rather than rare earth metals. Spedding's department was part of the Division of Industrial Science, a service division that included most of the non-engineering and agricultural departments on campus. The funds came from the Industrial Science Research Institute, the administering unit that oversaw research for those same departments. Spedding also told McCoy about this new approach in a letter as early as February 27, 1939 (Spedding Papers): "I have become interested in the possibility of quantitatively determining the amount of vitamins, hormones, etc., present in complex organic mixtures by means of adsorption spectra at low temperatures." In 1940 with R. M. Hixon another faculty member in Chemistry, he examined spectra of sugars and starches (F. H. Spedding and R. M. Hixon, "Ramen spectra of sugars, dextrins and starches, *Iowa Corn Research Institute Report of Agricultural Research* 5 (1940): 62-63). Spedding also had five Ph.D. students before the war, all working with adsorption spectra.

subjects.[93] In fact, every Graduate Catalog from 1920-21 until 1946-47 made the following announcement about the purpose of graduate study at Iowa State:

> Iowa State is a technical institution. Its Graduate College offers to qualified students the opportunity to pursue advanced courses and to undertake research in technology and those branches of science that find their application in industry.[94]

In 1937 when Spedding was hired, some basic scientific research was being conducted, although it tended to be primarily in agricultural areas. As far as equipment and research facilities were concerned, Spedding later recounted his version of Iowa State's condition:

> When I arrived in 1937, only a fraction of the building was reasonably and adequately equipped, and many rooms did not have standard laboratory equipment. Instead they had sawhorses with planks on top and a shelf underneath to hold glassware. The glassware was protected by a chintz curtain hanging down from the planks. These rooms were under-wired and the lighting was one cord from the ceiling with a bare electric light bulb. There was [sic] almost no wall plugs. As far as equipment was concerned, there was little of it. As far as I could tell when I arrived in 1937, the building had never been repainted since 1912.[95]

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[93]Frank H. Spedding, interview 3 with Elizabeth Calciano, Ames, Iowa, July 1979, transcript in possession of Edith Landin, 1.

[94]Iowa State College of Agriculture and Mechanic Arts, *Announcement of the Graduate Division*, Ames, Iowa, 1920-21, 11. Each year the bulletin explained that no major advanced degree offerings were made in the liberal arts areas. According to a gentlemen's agreement, the University of Iowa in Iowa City was to handle those areas; Iowa State was supposed to offer only applied courses in its chemical and physical sciences also. The head of the Iowa State chemistry department though resisted that ruling and managed to attract men of the caliber of Henry Gilman, an internationally known organic chemist, as well as Spedding by disregarding that gentlemen's agreement.

[95]Spedding, interview 3 with Calciano, 3; Svec, interview with author, 1991. The chemistry building had burned in 1912, and Coover as chair of the department had built a beautiful new building, but he put all the funding into the building; there was not enough money to furnish it adequately.

Organization of the chemical division of the Metallurgical Laboratory

Spedding's expertise with the rare earths garnered him the division job under Compton. Because there was not enough room at Chicago to conduct the needed chemical and metallurgical research, Spedding volunteered Ames as an additonal laboratory site. Thus, he had two projects to begin—one in Chicago and one in Ames. He spent Monday, Tuesday, and Wednesday at the University of Chicago and Thursday, Friday, Saturday, and Sunday in Ames, making an arrangement with the station master at the Ames depot to reserve a sleeper car every Sunday night into Chicago and one on its return on Wednesday night.[96]

At Chicago, Spedding attempted to gather the best chemists he could find from around the country. He and Arthur Compton visited Glenn Seaborg at Berkeley and convinced him to head up plutonium studies at the University of Chicago. The young, ambitious chemist and some of his research group arrived on April 19, 1942.[97] While in California on the same visit in late

[96]Frank H. Spedding, interview 5 with Elizabeth Calciano, May 5, 1980, transcript in possession of Edith Landin, Ames, Iowa, 2. Train travel was the preferred mode of travel during the war years. There had been a regular daily train to and from Chicago for several years. Ames was a side station for Des Moines, and the Northwestern Railroad dropped off a sleeper car from Des Moines about 9 p.m. daily ; it sat on the side track in Ames until the *City of San Francisco* came about midnight on it way to Chicago and picked it up. The same train returned with a sleeper car the next morning leaving Chicago about 11 p.m., and arriving in Ames around 5 a.m. It sat until 8 a.m. when it was taken to Des Moines. Spedding always reserved lower berth 5, in car 194 each week.

[97]Spedding, interview 5 with Calciano, 16-17; Glenn Seaborg, "Letter to Frank Spedding," March 2, 1942, Spedding Papers; Frank Spedding, "Letter to Glenn Seaborg," March 11, 1942, Spedding Papers; Glenn Seaborg, "Letter to Frank Spedding," April 9, 1942, Spedding Papers; Hewlett and Anderson, 90. Seaborg, partly due to his youth and lack of experience in administration, had been one of the scientists overlooked when Spedding was chosen as the head of the Chemistry Division, and according to Spedding, he gave him several headaches during his tenure as division head. Spedding had to deal with complaints from those working under Seaborg that he did not give due credit for work. When Spedding stepped down from the job eighteen months later, Seaborg was again overlooked and gave the next director, James Franck, problems too. Eventually, his colleagues, including Spedding,

March, Spedding met an inorganic chemist at the University of California, Los Angeles, Charles Coryell, who specialized in radioactive fission products on cyclotrons and offered him the division of fission products. Milton Burton, from New York University took over the radiation damage section. The fourth group, analytical chemistry, was headed by George Boyd who was already at the University of Chicago. Later, Compton added John Chipman from MIT to head up the metallurgical studies at Chicago.[98]

Summary

From 1939 to February 1942, strides were made in scientific studies of uranium, even though there had been no chain reaction, uranium in only gram quantities was available for experimentation, and knowledge of the chemistry of uranium and its byproducts was virtually nonexistent. But some subtle changes in uranium research and funded research in general had occurred. Scientists arguing from 1939 until early 1941 could not convince the government to fund scientific research. By the end of 1941 though, Vannevar Bush, the engineer/scientist, had convinced President Roosevelt that it was

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came to respect him as a great scientist. They felt that it was his youth and ambition that caused the early problems of not crediting his staff with discoveries or not being the necessary team player (Hewlett and Anderson, 90; "James Franck," Spedding manuscript, 2-3).

[98]Frank H. Spedding, "Charles D. Coryell," Spedding Manuscript, [1]; Charles Coryell, "Letter to Frank H. Spedding on Employment," April 7, 1942, Spedding Papers; Frank H. Spedding, "Letter to Charles Coryell on Employment," April 10, 1942, Spedding Papers; Charles Coryell, "Letter to Frank H. Spedding on Employment," April 24, 1942, Spedding Papers; Spedding, interview 8 with Calciano, 10-11; Spedding, interview 5 with Calciano, 16; Spedding, interview 8 with Calciano, 11; Milton Burton, "Letter from to Frank Spedding," May 13, 1942, Spedding Papers; Spedding, interview 5 with Calciano, pp. 16-17; Milton Burton, "Letter to Frank Spedding on Employment," May 30, 1942, Spedding Papers; Spedding, interview 8 with Calciano, 11.; Spedding, interview 5 with Calciano, 16-17; Compton, 185.

imperative to the survival of the free world to invest in building atomic bombs. The original requests by the immigrants were ignored, partly because they were not U.S. citizens, but partly because the government was not ready to accept the idea that scientific research was necessary to protect national security. The turning point came partly because of the British who convinced many American scientists that science could be used in the development of a weapon, and, in turn, those American scientists convinced the American government bureaucrats that science had a practical goal, in this instance at least. However, in February 1942, many other problems awaited the scientists at Chicago and the newly organized Ames Project, problems challenging both scientific research and the administration of that scientific research.

SCIENCE AND TECHNOLOGY IN THE AMES PROJECT, 1942–45

Organizing Research and Technology Development

When Frank Spedding indicated to Arthur Compton that he had personnel in Ames who could examine chemical and metallurgical problems for Chicago, he must have been thinking of the only other faculty member in physical chemistry—Harley A. Wilhelm. Spedding's job as chemistry division leader was dated February 21,[99] and by February 24, 1942, he had signed up his colleague as the associate director on the Ames Project.[100] Wilhelm was not an internationally known scholar like Spedding, and his academic credentials, though sound, were not as impressive as those of his more famous colleague. Harley A. Wilhelm, whose parents were tenant farmers, was co-valedictorian of his small Iowa high school, but it was athletics that allowed him to financially afford college at Drake University in Des Moines, Iowa. A basketball scholarship paid his tuition, but in order to earn enough money to remain in school, in the summers he worked construction gangs and played semi-pro baseball, another passion. He graduated from Drake in 1923 in mathematics, having taken only two courses in chemistry. He applied for a

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[99] J. C. Sterns, "Letter to F. H. Spedding on Chicago Metallurgical Laboratory Employment," March 19, 1942, Ames Laboratory Papers. On March 19, 1942, Spedding received this letter from the University of Chicago acknowledging that he had been on their payroll since February 21, 1942.

[100] Oath of Secrecy signed by Harley A. Wilhelm, Spedding Papers; Spedding, interview with Svec, 1984, 14.

fellowship at Iowa State University but lacked the number of courses in chemistry to enroll. Turning to his other love, athletics, and to support a new wife, he took consecutive high school positions teaching science and coaching. Later, he held a less than successful college coaching position in Helena, Montana. He returned to Drake in the summer of 1927 and took enough chemistry courses to qualify for an assistantship at Iowa State. The family stayed in Des Moines while he went to Ames alone, since the assistantship could not support a wife and baby daughter. In 1928, he was awarded an instructorship that enabled the entire family to move to Ames.[101]

Wilhelm initially worked for Anson Hayes, the head of physical chemistry and a well-known metallurgist in iron and steel technologies. Hayes left Iowa State College in January 1928, for industry, leaving one of his former graduate students, W. H. Jennings, in charge of physical chemistry. Because Wilhelm showed an interest in spectrochemistry, he inherited the newly ordered spectrograph that was to have gone to Hayes. He earned his Ph.D. degree in December 1931, after writing his thesis on band spectra of magnesium sulfide and lead sulfide.[102] Wilhelm remained at Iowa State as an instructor, turning down a job in Nebraska because it left him no time for research.[103] Wilhelm remained as an instructor for several years because as a graduate of Iowa State, the president of the College refused to promote him or give him

[101]"Wilhelm Recalls the Early Days," *Ames Laboratory Changing Times* (August 1980): 4-5.

[102]Wilhelm, interview with author, 1990; R. M. Hughes, *Graduates with the Doctorate*, Studies of the Graduate College, No. 1 (Ames, IA.: Iowa State College, 1939), 20.

[103]Wilhelm, interview with author, 1990, 5; Harley A. Wilhelm, interview with Laura Kline, 1987, transcript in Robert W. Parks and Ellen Sorge Parks Library, Ames, Iowa, 5.

tenure.[104] Finally, in 1940, Wilhelm was offered a higher salaried job in industry and to keep him, Coover obtained for him an assistant professorship and a salary of $3,200.[105] In 1937, Spedding, replacing Jennings as head of physical chemistry, took the spectrograph for his own research work and left the metallurgical area of the department completely to Wilhelm, a fortunate circumstance in the long run for Wilhelm since he became Spedding's expert in that area for the Ames Project.[106]

After hiring Wilhelm for metallurgical studies, Spedding attempted to find other personnel to staff his Ames operation. He appointed I. B. Johns, a researcher with a physical chemistry background, to oversee plutonium research, even though at the time he was a faculty member in plant chemistry.[107] Graduate students who were working with or had previously worked under Spedding, Wilhelm, and Johns were the next most obvious people to work into the project. Spedding and Wilhelm contributed one student each in February—Adrian Daane for metallography and casting studies

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[104]Wilhelm and Spedding both recounted in various interviews that Hughes as president of Iowa State wanted to bring in outside talent for positions rather than hire inbred faculty members. If Coover raised Wilhelm to an assistant professor, he would receive tenure in three years and remain as a permanent faculty member. So it seemed to be common that people in Wilhelm's position would remain instructors or leave. (Wilhelm, interview with Kline, 1987, 12; Wilhelm, interview with author, 1990, 5; Spedding, interview with Svec, 1984, 7).

[105]Wilhelm, interview with Kline, 1987, 12; Wilhelm, interview with the author, 1990, 14; "Wilhelm Recalls the Early Days," 6. According to Wilhelm, Friley who had replaced Hughes as president kept the same rule on inbreeding, so Wilhelm must have been an important asset in the department for Coover to get the professorship for him. He had graduate students working under him, had taught all the metallurgical courses for engineering students as well as a ceramic engineering course. He was also the only other faculty member in physical chemistry.

[106]Wilhelm, interview with author, 1990, 4-6; Spedding, interview with Svec, 1984, 7-8.

[107]Spedding, interview 5 with Calciano, 1-2; Spedding, interview with Svec, 1984, 15.

because he was already working with oxides and carbines and Ray Hoxeng for uranium coatings studies.[108] In April, C F Gray finished his Ph.D. under Wilhelm and joined the small group to work in castings. In June 1942, Wayne Keller, a former student of Spedding's at Cornell, joined the project to work with uranium metal reduction.[109] Rounding out the early group were Amos Newton from Eastman Kodak, W. H. Sullivan from the New England Zinc Company in Pennsylvania, and Adolph Voigt of Smith College, all men originally from a research group that had been using the cyclotron to produce radioactive materials at the University of Michigan At Ames, they contributed their expertise as group leaders in the various non-metallurgical chemical research areas, particularly in studies of plutonium and radiation.[110]

In early February 1942, Spedding contacted President Charles Friley at Iowa State College for clearance to establish the Ames Project. He had previously received permission from him to spend several weeks in Chicago in January. In late February, Friley released Spedding from half his duties at

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[108]Fulmer, Appendix C: List of Scientific Personnel of the Ames Project under the Manhattan District; Wilhelm, interview with author, 1990, 6-7.

[109]Fulmer, Appendix C; Wilhelm, interview with author, 1990, 6-7; Spedding, interview with Svec, 1984, 15; Wilhelm, interview with Kline, 1987, 15.

[110]These three men, all recent Ph.D.s, were students of Kasmir Fajans, a renown radiation chemist at the University of Michigan, whom Spedding was trying to get into the project, either on a subcontract at Michigan or at Chicago where he could set up a group there. His students came to Ames only temporarily while Fajans was in the process of getting security clearance. Unfortunately, because of his Polish descent, he was never cleared and Spedding kept his students, incorporating them as group leaders at Ames. (Adolph Voigt, interview with author, July 1990, Ames, Iowa, 1; "Adolph Voigt Looks Back," *Ames Laboratory Changing Scene* December 1981, 5; Correspondence between Kasmir Fajans and Frank Spedding, May 11, 1942, May 12, 1942, May 14, 1942, May 23, 1942, May 29, 1942, June 24, 1942, and August 10, 1942, Spedding Papers; Correspondence with Dr. Amos Newton, May 23, 1942, June 3, 1942, and June 10, 1942, Spedding Papers; Correspondence with Dr. William H. Sullivan, May 23, 1942, May 26, 1942, and June 3, 1942, Spedding Papers). Also see Frank H. Spedding, "Auditing," Spedding Manuscript, [5].

Iowa State in order to work on the secret project at the Chicago Metallurgical Laboratory.[111] The government gave Friley security clearance in late February or early March so that Spedding could release information on the nature of the research work at Chicago and Ames. The other top-level administrator allowed access to classified information was Dean Harold V. Gaskill, dean of the Industrial Sciences Division, under whose jurisdiction rested all war-time research projects at Iowa State College.[112]

The original agreement with Compton guaranteed that any personnel hired in Ames would work there for three months and move to Chicago when space was available. Since it took longer than anticipated to hire men, find space at Chicago, and build the buildings to house the Ames and Chicago chemical staffs and because the Ames group was progressing well at the end of the three months, Compton agreed to allow the supporting laboratory to continue at Ames under contract for six additional months.[113] Spedding also had difficulty convincing scientific staff to work on the project at Chicago because many of the chemical scientists were suspicious of atomic research. The project, locally called "Compton's Folly," did not immediately attract large

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[111]Charles E. Friley, "Letter to Arthur Compton on Releasing Spedding for Duties with the Metallurgical Laboratory," February 28, 1942, Ames Laboratory Papers.

[112]Frank H. Spedding, interview 6 with Elizabeth Calciano, May 7, 1980, transcript in possession of Edith Landin, Ames, Iowa, 3. Gaskill's title was Director of Special Research for Iowa State College. (H. V. Gaskill, Letter to Major A. V. Peterson Listing all Personnel who can Sign Forms," August 7, 1943, MED Files, Record Group 77, National Archives, Washington, DC.)

[113]Spedding, interview 5 with Calciano, 13; A. H. Compton, "Letter to S. K. Allison on Reorganization of the Metallurgical Chemistry Section," June 5, 1942, the Ames Laboratory Papers, 1.

numbers of chemists because they thought research on submarine detection, radar, and gas research were much more important to the war effort.[114]

Metallurgical work conducted at Ames began with three 1920-vintage pieces of metallurgical equipment: a small induction furnace that needed a few parts, a photo-micrograph that had been missing a mirror for several years, and the old Hayes-purchased Helger E-1 quartz spectrograph. Luckily, Ames had good analytical equipment available. Eventually, the Manhattan Engineer District replaced the reliable, but old, equipment with government purchased instrumentation from funding especially allocated for the project.[115]

Organizationally, Spedding thought he needed the scientists in Ames in order to supplement the Chicago laboratory in case that larger group failed in its primary tasks. Therefore, he instituted a parallel organization, assigning the scientists at Ames the same problems as those given to scientists in Chicago, but from different perspectives. For example, Johns and his group worked on plutonium chemistry, and Amos Newton and William Sullivan had small groups backing up the fission products research at Chicago. Harley Wilhelm and Wayne Keller each headed small groups dealing with metallurgical problems. James Warf took charge of the group trying to find analytical methods to detect trace elements in pile materials even though there was also an analytical group in Chicago.[116] According to Spedding, there was little

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[114]Spedding, interview 5 with Calciano, 14.

[115]Spedding, interview 5 with Calciano, 13; Wilhelm, interview with author, 1990, 7-8; Spedding, Wilhelm, Daane interview, 1967, 1-2.

[116]Spedding, interview 5 with Calciano, 15. This organizational concept was used repeatedly throughout the war. Success was so desperately needed that often there was this parallel effort. For example, four methods of producing a bomb (three separation techniques

duplication since, after all, he was in charge of both groups and kept each informed of the other's work.[117]

In late 1943, Spedding delivered a report on the organizational structure of the Ames Project He acknowledged that the main chemical research group resided in Chicago. Ames served as the "supplementing pioneer research group to the main chemical program."[118] The men in group leader positions even at this date—Spedding, Johns, Wilhelm, Rundle, Sullivan, Newton, and Keller—were all young men, most barely in their thirties; the younger men and the few women under them were equivalent to graduate students working on their doctorates.[119]

Because of the youth and scientific inexperience of the scientists at Ames, Spedding indicated that most of the research needed to be completed "as a result of group discussions and teamwork between the various groups."[120] Twice a week, his group leaders and section leaders met to discuss the previous week's work and plan for the next week's tasks. In addition, each group met once a week with its own section chief or group leader. Spedding later remembered the organization:

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with uranium and the plutonium process) were maintained throughout the war because no one knew which way was ultimately to be successful.

[117]Spedding, interview 5 with Calciano, 19.

[118]Frank H. Spedding, "Report of the Ames Chemical and Metallurgical Groups from February 20, 1942 to Dec. 22, 1943," the Ames Laboratory Papers, 1.

[119]Frank H. Spedding, "Report of the Ames Chemical and Metallurgical Groups from February 20, 1942 to Dec. 23, 1943," the Ames Laboratory Papers, 1.

[120]Frank H. Spedding, "Report of the Ames Chemical and Metallurgical Groups from February 20, 1942 to Dec. 23, 1943," 1-2.

> At these meetings there is a free-for-all discussion and it is very difficult to state just who has the various ideas as one man stimulates another. The net result is that most of our contributions have been the result of teamwork and should not be attributed to any one group or any one individual.[121]

This team approach seemed to be the key to the Ames organization. Spedding often pointed to its role in pressing the research forward. He believed in this approach so much that even the shop personnel gave advice on how pieces of equipment could be modified or built.[122] In this instance, Spedding was actually following the lead of the other academics at the national level in charge of the entire project (as will be noted in a later chapter on the academic organization) and his own experiences as a member of G. N. Lewis' academic laboratory at Berkeley. Spedding's methods—the seminars, research groups, and project-oriented research—may have been novel at Iowa State, but this academic style was already characteristic of research organization throughout the atomic bomb project.

Many of the administrative problems for Spedding throughout the war revolved around getting staff, both scientific and support personnel. To obtain his personnel, Spedding relied on his academic network of contacts, potential scientists and others who contacted him directly, and even the military to provide him with workers. For example, after Leslie Groves and the Manhattan District took over the project in late 1942 and early 1943, Spedding

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[121]Frank H. Spedding, "Report of the Ames Chemical and Metallurgical Groups from February 20, 1942 to Dec. 23, 1943," 2. Also, several of the people this author interviewed indicated they attended and directed these seminars and meetings. See the author's interviews with Voigt, 1990, 4; Carlson, 1990, 5; and Wilhelm, 1990, 15.

[122]Frank H. Spedding, interview 6 with Elizabeth Calciano, Ames, Iowa, dictated May 7, 1980, transcript in possession of Edith Landin, Ames, Iowa, 30.

was allowed to pick out any military men and women who had bachelor's degrees in chemistry, as long as they did not have orders to leave for the war's European front. At one time, he went through military records with Groves' permission and chose forty chemists from the wartime list—twenty to go to Chicago and twenty for Ames.[123]

Spedding also recruited juniors and seniors, primarily at Iowa State, who were chemistry majors and put them to work on production lines. Sometimes these men were drafted, but Spedding often managed to get them reassigned back at Iowa State or in Chicago.[124] Local area people were often hired on the project at Ames. One of the chief jewelers in Ames at the time made small instrumentation; a retired bank president became a store room clerk and later a security guard at Little Ankeny; a gas station owner was head of security; and a small tool shop along with its owner was moved to the campus.[125]

Spedding's support staff was meager at the start of the war. A business manager and two secretaries kept records, made out purchase orders, and handled whatever non-scientific duties were needed. As the red tape grew throughout the war, so did the staff. At the end of the war, the Ames Project was employing almost one non-scientist support person for every scientist. The laboratory had its own janitorial staff. There was a large contingent of security guards hired from the local Ames community that had replaced the

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[123]Spedding, interview 6 with Calciano, 6; Frank H. Spedding, "Security Foul Up," Spedding Manuscript, 1-2.

[124]Spedding, interview 6 with Calciano, 9-10.

[125]Spedding, interview 6 with Calciano, 13; Svec, interview with author, April 1992.

campus police who had handled security before the war. However, the College still provided some services, particularly in the area of purchasing. Since the guards and the support staff were not unionized at Iowa State during the war period, they also participated actively in Spedding's organizational team concept. Guards often doubled as chauffeurs to pick up visitors who came to examine the Ames Project facilities and sometimes even turned off the scientific equipment at night so the scientists did not have to go back to campus.[126]

The Raw Materials Crisis in 1942

Introduction

Shortly after the organizational structure was in place, the Ames Project became involved in both the metallurgical and chemical problems of initiating a chain reaction. Metallurgically, Ames investigated producing sizable amounts of uranium as well as casting it on a large-scale, particularly for the upcoming Chicago experiment to demonstrate the feasibility of a chain reaction Chemically, in the early years, the project was concerned with the basic chemistry of the relatively unknown uranium, its melting point, viscosity, and its reaction with other compounds The laboratory also experimented with protective coatings for uranium, preparation of special compounds, and reactivity of uranium and its by-products. After many of these early problems were solved and a nuclear chain reaction had been successfully demonstrated, Ames often engaged in consultant studies and

[126]Spedding, interview 6 with Calciano. 27-28.

services for other laboratories, producing thorium, cerium, and other rare elements. Ames discovered uses of and recast metallic uranium turnings from scrap pieces shipped from around the country. And probably the best known contribution of the Ames Project—the establishment of a pilot plant to produce metallic uranium, using two of its own methods to both reduce and cast the metal in an old remodeled one-story house near the current-day journalism building—continued until industry could take over the process by late July 1943.[127]

Uranium metal

In February 1942, several research objectives confronted the Metallurgical Laboratory before it could produce a bomb—first, how to find a way to produce a chain reaction using the U_{238} isotope of uranium; second, how to chemically separate plutonium from the uranium isotope in order to produce an explosive chain reaction; and, finally, how to establish a plant to move the processes to a large-scale production of materials necessary to create a bomb.[128] Spedding's chemical division was officially responsible for the second

[127] Fulmer, 12-13. See also various monthly and weekly reports produced from the Ames Project from February 1942 to December 1945 for technical details of research activities. A sampling of these include: CC-176, July 2, 1942 for a discussion of casting uranium in graphite; CC-177, July 9, 1942 for a report on reduction of oxides with aluminum and magnesium as well as the production of crucibles of different materials; CC-238, August 15, 1942 when coatings on uranium were studied; CC-298, October 15, 1942 a report that included studies of recovery of metal from casting wastes; CT-542, March 27, 1943 for a study and review of methods in casting of uranium ingots; CC-587, April 19, 1943 when a complete write-up of uranium hydride studies was included; CT-751, June 24, 1943 a study of the moisture in lime liner materials; CC-1524, March 10, 1944 a report on the rare gases; CT-1784, August 10, 1944 a report on the production of cerium; and CC-2398, March 17, 1945 a preliminary report on thorium nitrate extraction from uranyl nitrate, all in the Ames Laboratory Papers. For a complete listing of research projects, see Fulmer, Appendix I: List of Reports for the Ames Project.

[128] Smyth, 89; Compton, *Atomic Quest*, 86-87.

objective, but it soon became apparent that chemical concerns were imbedded in every aspect of the project.

Materials procurement became one of the most critical concerns of the Metallurgical Laboratory. Uranium in its metallic form or in a salt form of great purity as well as graphite, beryllium, deuterium, and calcium were crucial for the chain reaction.[129] Purity of the uranium presented a particularly difficult problem. Virtually no uranium metal in its most pure form, or even a pure enough salt or oxide, was available in early 1942. In late 1941, Leo Szilard, reported to Arthur Compton that three processes existed to make uranium metal, each producing only gram quantities: the photochemical process developed at Westinghouse Lamp Division, the uranium-chloride reduction method discovered by J. J. Rodden who was presently at the National Bureau of Standards, and the calcium hydride method developed by P. P. Alexander of Metal Hydrides at Beverly, Massachusetts.[130] Most of these methods had neither scaled up their processes to make enough uranium at a reasonable cost, nor had they eliminated the impurities that so plagued most early production of uranium.

Harvey C Rentschler, director of the research laboratory, and John W. Marden, a deputy researcher, of the lamp division of Westinghouse, located in

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[129]Smyth, 91; Hewlett and Anderson, 65.

[130]Leo Szilard, "Memorandum for Professor A. H. Compton Summarizing My Contacts with Firms in Connection with the Supply of Uranium Metal, Graphite, Calcium Metal, Uranium Oxide, Uranium Carbide and Beryllium," Report No. R-7 of the Chicago Metallurgical Reports, [1941], in Ames Laboratory Papers. For a summary of the technical characteristics of these early processes, see also J. C. Warner, "Early Methods for Producing Uranium Metal," Chapter 6 in *Uranium Technology: General Survey*, by J. E. Verne and J. C. Warner, National Nuclear Energy Series, Division VII, vol. 2A (Washington, DC: Atomic Energy Commission; Elmsford, NY: Microforms International, 1977, microfilm), 142-150.

Bloomfield, New Jersey, as early as 1919 had experimented with using metallic uranium as a substitute for tungsten in incandescent filaments. In 1927, they took out a patent on the process.[131] Knowing that uranium was close to tungsten on the periodic chart, it seemed natural that it could be used as filament material. Since its melting point was lower than tungsten though, it did not prove satisfactory. Nevertheless, the laboratory continued research on uranium and other rare earths and even produced very small quantities of the metal for college and university research experiments.[132]

In 1929, two other researchers at Westinghouse, Frank Driggs and William Lilliendahl, refined the process to obtain pure uranium metal by an electrolysis of the fused salts. Essentially, the electrolytic process involved producing a "green salt," potassium uranium fluoride, which had been photochemically created on the roof of one of the buildings at Bloomfield, using sunlight to initiate the photochemical reaction. The resulting product, KUF_5, was mixed with calcium chloride and sodium chloride and heated. When the salts melted, the uranium ions that had deposited on a molybdenum electrode were removed and crushed into particles. After being washed in barrels, the uranium was dried in vacuum ovens and pressed into

[131] "Westinghouse Lamp Division Marks 50 Years of Progress in Bloomfield," Press Release from Westinghouse in Harley A. Wilhelm Papers, Ames Laboratory, Ames, Iowa, 4 (hereafter Wilhelm Papers). For an in depth discussion of these three processes, see Harley A. Wilhelm, "Development of Uranium Metal Production in America," *Journal of Chemical Education* 37 (February 1960): 56-68. Most of the material the author has used in the descriptions above and those to follow comes from the letters and other written material sent to Wilhelm as he was preparing this article. Although the material is also summarized by Wilhelm, the author cites the background documents since they are often in more detail than Wilhelm's account.

[132] John Walsh, "A Manhattan Project Postscript," *Science* 212 (June 19, 1981): 1370.

small pieces called buttons.[133] Up until 1941, the process, according to Lilliendahl, had produced only a few kilograms of pure metal, hardly enough for commercial large-scale users. It was for sale in the open market for about $1,000 a pound.[134] In 1941, both the British and U.S. governments approached the company about the possibility of scaling up the process to produce ton quantities of the metal.

In December 1941, the Office of Scientific Research and Development (OSRD) signed a contract with Westinghouse to produce metallic uranium.[135] By the Spring of 1942, little metal had been produced, primarily because of the lack of sunshine in the New Jersey climate. Westinghouse investigated, with little success, the possibility of using ultraviolet lamps and even considered moving the operation to Arizona where the sun would shine more often. Though producing the required quantities of uranium metal remained a problem for the duration of the contract, the Westinghouse Process never encountered an impurity problem because of the high purity of its raw materials and because of its excellent analytical procedures to detect impurities all along the process. Later, after substituting uranium tetrafluoride, UF_6, instead of KUF_5, Westinghouse found that this process did not need the sun, and by November 1942, just a month before the Stagg Field chain reaction experiment, the company had sent 6,000 pounds of the metal to Chicago at a

[133]Patent No. 1,961,625 issued June 7, 1932; Walsh, 1370; W. C. Lilliendahl, "Letter to Harley A. Wilhelm on his Article on Uranium," August 5, 1958, Wilhelm Papers; Smyth, 92.

[134]Smyth, 93; Compton, *Atomic Quest*, 91 say the cost of producing uranium by this method was around $1,000 per pound while Lilliendahl in his letter to Wilhelm on August 5, 1958 quoted the amount at $500. This author finds no evidence to refute the figure given by Compton and Smyth whose accounts were much closer to the time period.

[135]Lilliendahl to Wilhelm, 2-3.

cost of approximately $22 a pound.[136] Its operations were mostly discontinued in late 1943, when the Ames Process supplanted all other methods because that process produced enough metal of the required purity at a much cheaper cost than others.[137]

In 1932, Peter P. Alexander, who was later president of Metal Hydrides, wrote a Ph.D. thesis on his process to reduce uranium. Assisted by L. W. Davis and Frederick Archibald, he published information about the process in *Metals and Alloys* in 1937. His method first reduced uranium oxide with calcium hydride. The resulting product was leached with a diluted acid, dried, pressed into cubes and sintered in a vacuum. The National Bureau of Standards first contracted with Metal Hydrides in 1941 for 7,000 pounds of the Alexander metal to be delivered to its headquarters. However, the delivery was stopped mid-stream when analytical analysis showed boron had contaminated the uranium. The culprit was the calcium used by Metal Hydrides, so the Bureau decided to establish a calcium distillation unit at Beverly, Massachusetts, where Metal Hydrides was located, a fortuitous coincidence for Alexander's company since it was essentially ready for large-scale production of uranium when three men from the Metallurgical Laboratory came to visit in early 1942.[138]

On January 14, 1942, Lyman Briggs from the National Bureau of Standards, Arthur Compton from the University of Chicago, and Ernest

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[136]Smyth, 93; Lilliendahl to Wilhelm, 3-4.

[137]Walsh, 1371; Wilhelm, "Development of Uranium Metal," 67.

[138]Peter P. Alexander, "Letter to Harley A. Wilhelm on Uranium Production," January 28, 1959, the Wilhelm Papers; Peter P. Alexander, "The Hydride Process—IV," Reprinted from *Metals and Alloys* (October 1938): [1]-[5]; Szilard, "Memorandum for Professor A. H. Compton," 1-2; C. J. Rodden, "Letter to Harley A. Wilhelm on Uranium Production," January 21, 1959, Wilhelm Papers.

Lawrence from the University of California personally contacted Alexander at his company. Shortly thereafter, they signed a contract and the company reorganized to produce large quantities of metals for the Metallurgical Project. Because the company had little equipment such as furnaces and also because its metal was extremely pyrophoric, no appreciable amount of metallic uranium was available from them until almost November 1942.[139]

C. J. Rodden at the National Bureau of Standards experimented with uranium reduction methods involving calcium. He had been working with the "James Process," a method reported in 1926 in a scientific journal, which had been developed at the University of New Hampshire while he was there. It used calcium to reduce uranium oxide and uranium tetrachloride. Late in August 1942, Rodden independently discovered the same process that the Ames Project scientists had developed earlier that month.[140]

Uranium oxide

Uranium generally came in the form of a oxide, and it was well known that a purer oxide would produce, in turn, an end product of greater purity. Uranium oxide had been difficult to obtain since 1939 when Alexander Sachs warned President Roosevelt that the German occupation of Belgium might ruin chances to procure Belgian uranium oxide from the Congo. By the time Sachs was authorized to approach Belgium by Dr. Brigg's Uranium Committee, Germany had invaded Belgium and taken over 500 tons of uranium into its

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[139]Smyth, 94; Peter P. Alexander to Harley A. Wilhelm, August 2, 1968, Wilhelm Papers.

[140]Wilhelm, "Development of Uranium Metal," 58-63.

possession. The shipment of ore from the Congo by then had ceased because of the war hostilities.[141]

By January 1942, the quantity of uranium oxide needed to produce a chain reaction was no problem. Over 1,200 tons of the oxide were stored in a port in New York; additional tonnage quantities were available at the Eldorado Gold Mine in Toronto and at a chemical plant in Colorado. A total of 2,000 tons was actually available, and predictions estimated that only 150 tons of the oxide would be necessary through 1944. Compton thought he needed only 45 tons for his early experiments in Chicago.[142]

However, the National Bureau of Standards had earlier ordered several tons of uranium oxide from Canada for experimental purposes and found that though quantity was not a problem, purity certainly was. J. J. Hoffman had earlier discovered an ether extraction method to remove all impurities from uranyl nitrate.[143] The Metallurgical Laboratory repeated those experiments successfully, but found that companies in North America had neither the necessary equipment nor the desire to purify the uranium oxide using the ether extraction method; ether was known to be very explosive and erratic.[144] When Herbert McCoy and Herbert Anderson visited the Port Hope Refineries of the Eldorado Mine in Canada in April 1942, they posed the question of ether extraction to them. The company claimed they could extract the metal but only

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[141]Szilard "Memorandum for Professor A. H. Compton," [6].

[142]Hewlett and Anderson, 65.

[143]Smyth, 93.

[144]Compton, *Atomic Quest*, 93.

if the proper equipment could be procured for them to scale up their present laboratory method.[145]

Compton, at this point, decided he would contact his old friend Edward Mallinckrodt who ran a chemical plant in St. Louis that specialized in the production of ether and other chemicals. In May 1942, Compton and Frank Spedding in his capacity as head of chemistry traveled to St. Louis. Compton explained the ether extraction project to Mallinckrodt while Spedding worked out the details with the engineers Henry Farr and John Ruhoff. Within two hours Mallinckrodt agreed to tackle the job. At best, Compton had no real idea how much the process would cost, so he approved a letter of intent from the OSRD to Mallinckrodt with a promise to negotiate a contract later for the actual costs. The first quantities were shipped in July 1942, and continued at the rate of 30 tons per month, accomplishing the remarkable feat of producing on a commercial scale pure uranium oxide that was attainable only on a laboratory scale mere months before. The actual contract was not signed until the day that the last of the 60 tons left the Mallinckrodt plant, an example of the flexibility of the government policies toward contracting management on the one hand and the remarkable faith in the project by the company on the other hand.[146] Making pure uranium oxide became crucial in several processes throughout the war, including adding to the pile at Chicago, making uranium

[145] Herbert Anderson and Herbert McCoy, "Memorandum to A. H. Compton on visit to Port Hope Refineries of the Eldorado Gold Mines, Ltd.," April 16, 1942. Ames Laboratory Papers, 4.

[146] Smyth, 93; Compton, *Atomic Quest*, 93-95; Spedding, interview with Calciano 5, 18; F. H. Spedding, "Patent Letter to Lt. Colonel H. E. Metcalf Describing the Mallinckrodt Process," May 11, 1945, 1-2.

compounds like uranium tetrafluoride, and using the material for research experiments to produce a purer metal.

The Discovery and Development of the Ames Process

The discovery of the Ames Process to develop metallic uranium gave credence to both the administrative apparatus of the Ames Project and the research and development expertise of the Ames scientists. The laboratory worked as a team on the many problems that Spedding brought from his meetings in Chicago. One of the interesting things about uranium at the time was the incorrect assumption that uranium could be reduced by the same processes as those used for the elements around it on the periodic chart. Early in 1942, the Ames Project as well as other laboratories thought that the oxides of uranium would reduce to form a salt slag and clean metal. Unfortunately, uranium did not behave in a predictable way. First, the Ames group experimented with the oxide derivatives of uranium in order to produce metallic uranium through a hydrogen reduction, but without tremendous success.[147] The oxides presented temperature-melting problems, casting difficulties, and tended to corrode the normal crucibles made from beryllium, magnesia, and graphite.[148] By June 1942, attempts to reduce the uranium oxide with carbon in a hydrogen atmosphere also only partially succeeded.[149] Other

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[147]F. H. Spedding, "Report on Chemical Project at Ames, March 6-12, 1942," Spedding Papers; Adrian Daane, "Research Notebook," March 31. 1942, Ames Laboratory Papers.

[148]F. H. Spedding, "Report of the Chemical Work Done at Ames up to April 13, 1942," Spedding Papers.

[149]Daane, "Research Notebook," June 2, 1942.

reduction experiments with aluminum, magnesium, and calcium, resulted in little success.[150] During the early weeks of July 1942, the problem of the crucibles was finally solved when several successful uranium castings were made with graphite crucibles, resulting in no uranium sticking to the graphite.[151] The biggest problem remaining then was the lack of uranium metal to cast in the new crucibles. No process had been developed to supplant the processes in existence, methods that were expensive, unpredictable, and still producing only gram quantities.

The situation was so bad that the idea began to circulate around Chicago that perhaps some pure metal could be used in the core of the experimental pile and compounds—oxides, chlorides or fluorides—could be placed on the perimeter. Coincidentally, in the summer of 1942, someone working on the calutron electromagnetic separation processes at Berkeley brought some uranium tetrafluoride (UF_4) to an administrative meeting at Chicago to discuss the possibilities of using this fluoride or an oxide of uranium on the outside of the pile core at Chicago. Spedding looked at the two-inch cube that probably had been produced at Harshaw Chemical Company in Cleveland and wondered if using a salt that produced no oxygen could produce metallic uranium. In the normal reduction experiments oxygen had been the greatest

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[150]Daane, "Research Notebook, "July 2, 1942, July 6, 1942, July 27 1942; Wayne Keller, "Research Notebook," July 8, 1942, July 10, 1942, July 12-18, 1942, Ames Laboratory Papers, 46-48.

[151]F. H. Spedding, "Report of the Ames Chemical and Metallurgical Group for the Week of July 2, 1942," Ames Laboratory Papers.

barrier in reducing the uranium to large pieces of pure metal. He took the block back to Ames in late July and gave it to the metallurgical group to test.[152]

Wayne Keller, one of the men under Wilhelm, took the block and began the historic experiments to reduce the tetrafluoride with calcium or some other salt to uranium metal. On August 3, 1942, he recounted the first reduction attempt in his research notebook:

> The fluoride and calcium were ground together in a mortar and placed in an iron pipe as a crucible. The crucible and charge were placed with proper packing in a quartz tube and the whole evacuated. A thermocouple was placed between the quartz tube and the furnace coil. The furnace was heated by 110 volts at 12 ampe.
>
> The temperature increased from 30°C at 4:00 p.m. to 370°C at 4:38 p.m. At that time the pressure rose to about one-half an atmosphere suddenly, then began to drop again in a few moments. . . . The temperature was read and was found to have risen from 370°C to 540°C in four minutes. . . . At 600°C heating was discontinued.
>
> When the furnace was almost at room temperature argon was introduced, the furnace opened, and the crucible removed.
>
> The material in the crucible was found to have fused, and a lump of quite compact, but low density metallic material was found in the bottom of the crucible. . . . The large block on the bottom was sawed in two, and inside was found one large button of very pure looking metallic uranium. . . . This button weighed about 20 grams.[153]

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[152] F. H. Spedding, "Interview with Barton C. Hacker," October 21, 1980, Ames, Iowa, transcript in possession of Edith Landin, Ames, Iowa, 10; Hewlett and Anderson, 87-88; Spedding, interview with Svec, 1984, 15; Spedding to Metcalf, May 11, 1945, 2.

[153] Wayne Keller, "Research Notebook," August 3, 1942, 58-60. On subsequent days more experiments were run aiming at a greater yield and more compact single ingot. By August 7, with several adjustments, an ingot of 82 grams was discovered on the bottom of the crucible, the largest single ingot to date. (Keller, "Research Notebook," August 7, 1942, 69). See also A.H. Compton, "Metallurgical Project Report for the Month ending August 15, 1942," Report No. CC-238, in Ames Laboratory Papers, 5-8. Spedding reviewed the work also in a report called "Metal Production," Metallurgical Laboratory Report No. R-414, November 25, 1942 in Ames Laboratory Papers. For a review of the experiments see Wilhelm, Keller,

From the initial success, it was a simple matter to run a series of experiments to refine the process and produce even larger ingots of pure uranium. Other compounds in combination with uranium tetrafluoride were tried in the reduction method, including sodium and uranium chloride, but the process for reduction with calcium improved so much that by the end of August, most of the attention turned to producing a large cast of uranium metal.[154] In September 1942, large quantities of the uranium-calcium charge were prepared in crucibles that were made of 4-inch black steel pipes 15-18 inches long, capped on one end and welded with a solid sheet on the other end. A spark plug for ignition was also welded on the bottom or placed internally. Lime was generally used as a liner to prevent the charge coming in contact with the steel sides.[155]

Several experiments with these new crucibles, or *bombs* as they became known, continued using up to 2,000 grams of the uranium tetrafluoride. After several modifications, a few large ingots weighing over 1,500 grams (3-4 pounds) were produced. On September 21, 1942, several more reduction experiments were tried, with close to 3,000 grams of uranium tetrafluoride and

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Butler, "Production of Uranium Metal by the Reduction of Uranium Tetrafluoride by Metallic Calcium," Report for August 5, 1942, in Report CC-238, "Report of the Metallurgical Project for the Month Ending August 15, 1942," in the Ames Laboratory Papers, 1-8. According to Harry A. Svec (interview, April 1992), Richard Thompson, a former undergraduate, actually conducted the first experiment under Keller's direction.

[154]Keller, "Research Notebook," entries for the rest of August, 91-103.

[155]Keller, "Research Notebook," September 2, 1942, 106. See also "Comparison of Refractories as Bomb Lining Materials in Production," and F. H. Spedding, "Summary of Work at Ames," March 10-April 10, 1944, the Ames Laboratory Papers, 8-9 for a description of the different materials used for liners.

calcium in each experiment. From these experiments, several ingots were cast and recast by C F Gray, producing a final billet of pure metallic uranium weighing approximately 4,980 grams (eleven pounds).[156]

On September 24, 1942, Harley Wilhelm took the 5-inch by 2-inch 11-pound ingot from the casting furnace, placed it carefully in a traveling bag some students had given him from his coaching stint in Helena, Montana, and caught the night train that traveled to Chicago from the Ames Depot.[157] Getting off the train in Chicago, Wilhelm had to catch the "L" to the University of Chicago campus. In transit, the handle of his case broke, so by the time he reached Spedding's office in Eckhart Hall, he was carrying the precious cargo in its case under his arm. Spedding and Wilhelm took the billet to Compton who had never seen one piece of uranium this big before. His immediate reaction was, "I bet there's a pipe [hole] inside." Wilhelm took the ingot to the basement of the biology building and instructed a shop man to cut it open. After a small fire in the cutting process, a cropping from the ingot finally appeared; there was no pipe.[158] Spedding evidently took a cropping to an administrative meeting soon thereafter. R. L. Doan the laboratory director later recalled that momentous day:

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[156]Keller, "Research Notebook," various September entries, including 9/21/42, 107-137. A. H. Compton, "Metallurgical Project Report for Month Ending October 15, 1942," Report No. CC-298, Ames Laboratory Papers. This latter report gives a summary of the metallurgical work for August and September 1942.

[157]Harley Wilhelm, "Interview with Laura Kline, Iowa State Archivist" November 14, 1988, Incomplete transcript in Parks Library, Ames, Iowa, 1. Harley Wilhelm, "Telephone Conversation with author," July 1989, Ames, Iowa; Wilhelm, interview with author, 1990, 12.

[158]Wilhelm, telephone conversation with author, 1989; Wilhelm, interview with Kline, 1988, 2. Wilhelm, interview with author 1990, 12.

> I don't believe anyone took the work there [at Ames] very seriously until Spedding came to a technical council meeting one fine autumn day and smugly laid an "egg,"—an almost perfect cylinder of uranium metal, on the table for inspection. Even then, while admiring the accomplishment, everyone I am sure felt that it would be futile to look to a couple of college professors for the production of any significant quantity of metal.[159]

The Building of a Pilot Plant

But futile it was not. Within a week, R. L. Doan, the Metallurgical Laboratory director, had arrived on the Iowa State campus to write an OSRD contract for the Ames Project to produce 100 pounds of uranium per day, using its simple and cheap process.[160] The intention was that Iowa State would demonstrate the process to companies like DuPont, Electromet, and Mallinckrodt but continue to make uranium until the companies could integrate the processes into their own plants.[161] The Ames Project at this point became two complementary projects—one aimed at conducting chemistry studies on uranium and plutonium and the other incorporating the Ames Process to produce uranium in a pilot plant

Most of the research and chemical studies to date had occurred in the Chemistry Building, but with the need to add a full-scale pilot plant more space was needed for furnaces and other machinery as well as for the increased staff to scale up the uranium-producing process. Wilhelm and Spedding started a

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[159] R. L. Doan, "Letter to Harley A. Wilhelm about Recollections for Paper on Uranium Production," August 21, 1958, in Wilhelm Papers, 2.

[160] Doan, 2.

[161] Spedding, interview with Barton C. Hacker, 11.

search for an adequate site or building. On the east side of Ames, there was an old gas generation plant made of brick. Though sturdy, that building would take too much work to renovate, so it was discarded as a practical possibility for the plant. After a lengthy search, Wilhelm and Spedding discovered an appropriate building on campus, a small World War I temporary wooden house behind the Dairy Industries building, near the power plant on the southeastern edge of campus. It had been used years before as a women's gymnasium. In 1942, it was used primarily for storage; there was evidently a popcorn laboratory in one part of the building, and in a kind of garage the psychology department had stored some trucks with educational, demonstration equipment.[162] The College gave the building to the project, and immediately the chemists had the dirt floor in the garage area replaced with concrete so that casting could take place in this area. The chemists set up the reduction laboratory in the original part of the building where the popcorn lab had been located. The building shortly began to expand in a most curious pattern. The porch was used for the especially dirty work, the least secret of the process. However, when it became too cold to work on the porch, a canvas would be added followed by a crude set of walls and finally a new roof. A new porch appeared and the process repeated itself. The odd expansion of the house took place as soon as more space became necessary to expand operations

[162]Originally called the Home Economics Annex, the building was built west of the Home Economics Building in 1920. In 1926, when the new Home Economics Building was constructed, it was physically moved to a new site south of the Press Building where it served as the girls' gymnasium, called the Field House. In 1941, it was no longer needed because the new Women's Gymnasium was constructed. It was being used as a storage facility when Spedding and Wilhelm discovered it. H. Summerfield Day, *The Iowa State University Campus and Its Buildings 1859-1979* (Ames, IA: Iowa State University, 1980), 254.

and continued much to the chagrin of the local university architect who had been trying to get this old building torn down for several years prior to its occupancy by the Ames Project people.[163]

After the building became available, a machine shop at the production site became the second necessity. Wilhelm heard of a small machine shop, owned and managed by Bill Maitland, for sale in Ames west of Grand Avenue near the railroad. Maitland made garden tools normally, but he could no longer obtain the metal he needed because of war-time restrictions on material priorities. Wilhelm examined the shop contents and discovered that Maitland would sell all his tools and equipment for $8,000. After consulting with Spedding, both men contacted Maitland and bought the entire shop, moving the equipment along with Bill Maitland to the campus production building, officially called the Physical Chemistry Annex, later nicknamed by the local workmen as "Little Ankeny," after a war munitions plant in Ankeny, Iowa.[164]

Production equipment, unlike lathes, motors, and small tools from Maitland, was much harder to procure. For example, reduction furnaces were especially hard to obtain. The small reduction furnace in the Chemistry Department used to produce most of the metal earlier was not big enough for a

[163]Wilhelm, interview with Kline 1988, 4.; Spedding, Wilhelm, and Daane, interview, May 1967, 10; Spedding, interview with Barton C. Hacker, 1980, 12. For photographs of the building and a floor plan of the operation see Appendix B, Figures 1-2.

[164]Wilhelm, interview with Kline, 1988, 5. It is not known how the building received its name, but it was quite apt as a name for the project (Esther Polito to Bert Merrill, Letter on the Name Little Ankeny, September 21, 1945 in the Ames Laboratory Papers). Adrian Daane, one of the scientists in the project thought that it was named by some of the local townspeople who worked on the project and the name just stuck. These people knew of the munitions factory in Ankeny and just named it after that factory since the work on the Ames Project was somewhat dangerous, particularly with the number of explosions occurring on a routine basis (Daane, telephone interview with author, March 18, 1992).

large-scale production plant. Luckily for the Ames operation, the Metallurgical Laboratory had ordered two 40,000 watt reduction furnaces for what they called "Site B," but when the Ames pilot plant needed to be established as a production facility, those furnaces were diverted to Ames. Mixers and grinders for processing metals like calcium and later magnesium and vacuum casting apparatus were also purchased from various producers using contract money from the Manhattan Engineer District, an Army Corps of Engineers operation, which took over this part of the Ames Project in late 1942.[165]

The Chicago Pile-1 (CP-1)—December 2, 1942

While these arrangements were still being worked out in the fall of 1942, the Ames group continued to reduce metal in the Chemistry Building, beginning a uranium shipping program to Chicago. The University of Chicago received two tons of the metal from Ames for the Stagg Field experiment that occurred on December 2, 1942. Westinghouse and Metal Hydrides also each shipped two tons to Chicago.

Most of the research and production work that Iowa State undertook to this point supported the critical chain reaction or pile experiment at the University of Chicago. Spedding, in his capacity as head of the chemistry section, was present as one of the few invited guests to witness the historic event. Enrico Fermi, a physicist at Chicago, designed the experiment originally, making all the necessary calculations including everything from

[165]Wilhelm, interview with Kline, 1988, 7; Wilhelm, telephone conversation with author, 1989.