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Ceremonial address presented on the occasion of the Twenty-fifth Anniversary of the founding of the European Institute for Transuranium Elements, Karlsruhe, FRG, October 28, 1988

Pioneering the Nuclear Age

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G.T. Seaborg

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PIONEERING THE NUCLEAR AGE

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Pre-fission and fission

As a first year graduate student at Berkeley in 1934 nearly five years before the discovery of nuclear fission, I began to read the papers coming out of Italy and Germany describing the synthesis and identification of several elements thought to be transuranium elements. In their original work in 1934, E. Fermi, E. Amaldi, O. D'Agostino, F. Rasetti and E. Segre bombarded uranium with neutrons and obtained a series of beta-particle-emitting radio activities. On the basis of the periodic table of that day (Figure 1) they were led to believe that the first transuranium element, with atomic number 93, should be chemically like rhenium (i.e., be eka-rhenium, Eka-Re), element 94 like osmium (Eka-Os) and so forth. Therefore they assigned a 13-minute activity to element 93. I quote from a classical paper written by Fermi [1], entitled "Possible Production of Elements of Atomic Number Higher than 92", which I remember reading at that time:

> "This negative evidence about the identity of the 13 min.-activity from a large number of heavy elements suggests the possibility that the atomic number of the element may be greater than 92. If it were an element 93, it would be chemically homologous with manganese and rhenium. This hypothesis is supported to some extent also by the observed fact that the 13 min.-activity is carried down by a precipitate of rhenium sulphide insoluble in hydrochloric acid. However, as several elements are easily precipitated in this form, this evidence cannot be considered as very strong."

I recall reading soon thereafter a paper by Ida Noddack [2], entitled "Uber das Element 93," which took issue with this interpretation, suggesting that the radioactivities observed by Fermi et al. might be due to elements of medium atomic numbers:

> "Es wäre denkbar, dass bei der Beschiessung schwerer Kerne mit Neutronen diese Kerne in mehrere grossere Bruchstucke zerfallen, die zwar Isotope bekannter Elemente, aber nicht Nachbarn der bestrahlten Elemente sind."

However this paper, which intimated the possibility of the nuclear fission reaction, was not taken seriously.

Experiments in Germany during the following years by O. Hahn, L. Meitner and F. Strassmann (Figure 2) appeared to confirm the Italian interpretation and for several years the "transuranium elements" were the subject of much experimental work and discussion. In a typical paper by Hahn, Meitner and Strassmann [3], which I read, part of a series they published during 1935-1938, they reported a 16 minute $_{93}$Eka-Re^{237}, 2.2 minute $_{93}$Eka^{239}, 12-hour $_{94}$Eka-Os^{237}, 59-minute $_{94}$Eka-Os^{239}, 3-day $_{95}$Eka-Ir^{239}, 12-hour $_{96}$Eka-Pt^{239}.

In 1938 I. Curie and P. Savitch [4] found a product of 3.5 hours half-life that seemed to have the chemical products of a rare earth, but they failed to give an interpretation of this astonishing discovery. Their paper, which I also read at the time, had the title, "Sur La Nature Du Radioelement De Période 3,5 Heures Forme Dans L'Uranium Irradie Par Les Neutrons," and included the following:

> "Nous avons montré qu'il se forme dans l'uranium irradie par les neutrons un radioélément de période 3,5 heures dont les proprietes chimiques sont semblables a celles des terres rares. Nous la designerons ci-dessous par la notation $R_{3,5h}$...
>
> $R_{3,5h}$ se sépare nettement de Ac, allant en tête de fractionnement, alors que Ac va en queue. Il semble donc que ce corps ne puisse être qu'un élement transuranien possedant des proprietes tres différentes de celles des autres élements transuraniens connus, hypothese qui souleve des difficultés d'interpretation."

Then came the breakthrough. Early in 1939, Hahn and Strassmann [5], on the basis of experiments performed in December 1938, and with interpretive help from Meitner who had been forced to leave Germany, described experiments in which they had observed barium isotopes as the result of bombardment of uranium with neutrons. This historic paper, which I also read at the time, had the title, "Uber den Nachweis und das Verhalten der bei der Bestrahlung des Urans mittels Neutronen entstehenden Erdalkalimetalle" and contained the following conclusion:

> "Als Chemiker mussten wir aus den kurz dargelegten Versuchen das oben gebrachte Schema eigentlich umbenennen und statt Ra, Ac, Th die Symbole Ba, La, Ce einsetzen. Als der Physik in gewisser Weise nahestehende 'Kernchemiker' konnen wir uns zu diesem, allen bisherigen Erfahrungen der Kernphysik widersprechenden, Sprung noch nicht entschkliessen. Es konnten doch noch vielleicht eine Reihe seltsamer Zufälle unsere Ergebnisse vorgetauscht haben."

Subsequent work showed that the radioactivities previously ascribed to transuranium elements are actually due to uranium fission products, and hundreds of radioactive fission products of uranium have since been identified.

Thus in early 1939 there were again, as five years earlier, no known transuranium elements. During these five years I developed an increasing interest in the transuranium situation. When as a graduate student I gave my required annual talk at the College of Chemistry weekly Research Conference in 1936, I chose the transuranium elements as my topic, describing the work of Hahn, Meitner and Strassmann referred to above.

During the two years following my seminar talk in 1936 and before the discovery of fission, my interest in the neutron-induced radioactivities in uranium continued unabated and, in fact, increased. I read and reread every article published on the subject. I was puzzled by the situation, both intrigued by the concept of the transuranium interpretation of the experimental results and disturbed by the apparent inconsistencies in this interpretation. I remember discussing the problem with Joe Kennedy, a colleague in research, by the hour, often in the postmidnight hours of the morning at the old Varsity Coffee Shop on the corner of Telegraph and Bancroft Avenues near the Berkeley campus where we often went for a cup of coffee and a bite to eat after an evening spent in the laboratory.

I first learned of the correct interpretation of these experiments, that neutrons split uranium into two large pieces in the fission reaction, at the weekly Monday night seminar in nuclear physics conducted by Professor Ernest O. Lawrence in Le Conte Hall. On this exciting night in January 1939, we heard the news from Germany of Hahn and Strassmann's beautiful chemical experiments. I recall that at first the fission interpretation was greeted with some skepticism by a number of those present, but, as a chemist with a particular appreciation for Hahn and Strassmann's experiments, I felt that this interpretation just had to be accepted. I remember walking the streets of Berkeley for hours after this seminar in a combined state of exhilaration in appreciation of the beauty of the work and of disgust at my inability to arrive at this interpretation despite my years of contemplation on the subject.

During the years (1934- 1941) before the United States entered World War II Berkeley was a leading center of nuclear research (Figure 3). Lawrence, who had invented the cyclotron a few years earlier, designed and built, successively, the 27-Inch, 37-Inch, and 60-Inch cyclotrons and began construction of the 184-Inch Cyclotron. These were powerful instruments with which to conduct our research. J. Robert Oppenheimer was the leader of an extraordinary program of theoretical investigators. Other nuclear pioneers included Edwin M. McMillan, Luis W. Alvarez, Emilio G. Segre, and Willard F. Libby. The research staff of Lawrence's Radiation Laboratory included many other luminaries. Some of these nuclear pioneers served as my mentors, colleagues or collaborators in research. Importantly, graduate students played an important role in the program.

During this time I conducted research, with my collaborators, on the inelastic scattering of fast neutrons, the synthesis and identification of numerous radioactive isotopes (some of which later became important agents for the diagnosis and treatment of disease [cobalt 60, iodine 131, technetium-99m]), the chemical separation of nuclear isomers, the identification of the products of symmetrical fission induced at the higher energies, etc.

Perhaps the most important result of my research program was the synthesis and identification of the element with atomic number 94 (plutonium), following soon after the discovery of element 93 (neptunium) in 1940.

Neptunium and plutonium

The first transuranium element, with the atomic number 93, was synthesized and identified (i.e., discovered) at Berkeley in the spring of 1940 by McMillan (Figure 4) and Philip H. Abelson [6]. Using neutrons produced at the 60 inch Cyclotron, they bombarded uranium to produce the 2.3-day beta-emitter that, on the basis of their chemical work they were able to assign definitely to 93 239. They showed that this element is chemically similar to uranium and not like rhenium, as suggested in the periodic table of that time. They suggested the name neptunium (symbol Np) after the planet Neptune because it is just beyond uranium, as the planet Neptune is beyond Uranus, for which uranium is named.

Immediately thereafter, during the summer and fall of 1940, McMillan started looking for the daughter product of the 2.3-day activity, which obviously would be the isotope of element 94 with mass number 239 (94-239). Not finding anything he could positively identify as such, he began to bombard uranium with deuterons in the 60 inch cyclotron in the hope that he might find a shorter-lived isotope--one of a higher intensity of radioactivity that would be easier to identify as an isotope of element 94. Before he could finish this project, he was called away to work on radar at M.I.T.

When I learned that McMillan had gone, I wrote to him asking whether it might not be a good idea if we carried on the work he had started, especially the deuteron bombardment of uranium. He readily assented.

Our first deuteron bombardment of uranium was conducted on December 14, 1940. What we bombarded was a form of uranium oxide, U_3O_8, which was literally plastered onto a copper backing plate. From this bombarded material we isolated a chemical fraction of element 93. The radioactivity of this fraction was measured and studied. We observed that it had different characteristics than the radiation from a sample of pure 93 239. The beta-particles, which in this case were due to a mixture of 93-239 and the new isotope of element 93 with mass number 238 (93-238), had a somewhat higher energy than the radiation from pure 93 239 and there was more gamma radiation. But the composite half-life was about the same, namely, 2 days. However, the sample also differed in another very important way from a sample of pure 93-239. Into this sample there grew an alpha particle-emitting radioactivity. A proportional counter was used to count the alpha particles to the exclusion of the beta particles. This work led us to the conclusion that we had a daughter of the new isotope 93-238--a daughter with a half-life of about 50 years and with the atomic number 94. This is much shorter-lived than the now known half-life of 94-239, which is 24,000 years. The shorter half-life means a higher intensity of alpha-particle emission, which explains why it was so much easier to identify what proved to be the isotope of element 94 with the mass number 238 (94-238). (Later it was proved that the true half-life of what we had, i.e., 94-238, is about 90 years.)

On January 28, 1941, we sent a short note to Washington describing our initial studies on element 94; this also served for later publication in The Physical Review under the names of Seaborg, McMillan, Kennedy, and Wahl [7]. We did not consider, however, that we had sufficient proof at that time to say we had discovered a new element and felt that we had to have chemical proof to be positive. So, during the rest of January and into February, we attempted to identify this alpha-activity chemically.

Our attempts proved unsuccessful for some time. We did not find it possible to oxidize the isotope responsible for this alpha radioactivity. Then I recall that we asked Professor Wendell Latimer, whose office was on the first floor of Gilman Hall, to suggest the strongest oxidizing agent he knew for use in aqueous solution. At his suggestion we used peroxydisulphate with argentic ion as catalyst.

On the stormy night of February 23, 1941, in an experiment that ran well into the next morning, Art Wahl performed the oxidation which gave us proof that what we had made was chemically different from all other known elements. That experiment, and hence the first chemical identification of element 94, took place in Room 307 of Gilman Hall, the room that was dedicated as a National Historic Landmark, 25 years later (Figure 5). Thus we showed that the chemical properties of element 94 resembled those of uranium and not those of osmium.

The communication to Washington describing this oxidation experiment, which was critical to the discovery of element 94, was sent on March 7, 1941, and this served for later publication in The Physical Review under the authorship of Seaborg, Wahl, and Kennedy [8]. Later, in a publication after the war Wahl and I [9] suggested the name plutonium (symbol Pu) after the planet Pluto, the second and last known planet beyond Uranus.

Almost concurrent with this work was the search for, and the demonstration of the fission of, the isotope of major importance--94-239, the radioactive daughter of 93-239. Emilio Segre played a major role in this work together with Kennedy, Wahl and me. The importance of element 94 stems from its fission properties and its capability of production in large quantities. The 0.5-microgram sample on which the fission of 94-239 was first demonstrated was produced by transmutation of uranium with neutrons from the 60 inch cyclotron;

it was chemically isolated in rooms in Old Chemistry Building and Crocker Laboratory and in Room 307 Gilman; and the fission counting was done using the neutrons from the 37-inch cyclotron. A fission cross section for plutonium-239, some 50 per cent greater than that for uranium-235, was found, agreeing remarkably with the accurate values that were determined later. This result was communicated to Washington on May 29, 1941, and this served as the basis for the later publication of an expurgated version by Kennedy, Seaborg, Segrè, and Wahl [10].

First isolation of plutonium

The observation that plutonium-239 is fissionable with slow neutrons provided the information that formed the basis for the U.S. wartime Plutonium Project of the Manhattan Engineer District (led by General Leslie R. Groves with overall guidance by Vannevar Bush and James B. Conant) centered at the Metallurgical Laboratory (led by Arthur H. Compton) of the University of Chicago. Given impetus by the entry of the United States into the war in December 1941, I and some of my colleagues moved to Chicago in the spring of 1942. The mission of the Met Lab was to develop (1) a method for the production of plutonium in quantity, and (2) a method for its chemical separation on a large scale.

The key to solving the first problem was the demonstration by Enrico Fermi and his colleagues of the first sustained nuclear chain reaction on December 2, 1942.

Important to the solution of the second problem was the determination of the chemical properties of plutonium, an element so new that little was known of its characteristics, and the application of these to the design of a chemical separation process to separate the plutonium from the enormous quantity of fission products and the uranium. I served as leader of the large group of chemists who worked in collaboration with the chemical engineers to solve this problem.

The earlier tracer chemical investigations at Berkeley, continued at Chicago, served to outline the nature of the chemical separation process. The key was the oxidation-reduction cycle in which plutonium is carried in its lower oxidation state(s) by certain precipitates and not carried by these same precipitates when it is present in its higher oxidation state. Thus, it is separated from the fission products, which do not exhibit this difference in carrying behavior from oxidizing and reducing solutions. However, the carrying properties of plutonium at tracer (extremely small) concentrations might be different at the macroscopic concentrations that would exist under actual operating conditions in the chemical separation plant.

It occurred to me that central to the achievement of such a separation process would be chemical work on concentrations that would exist in the chemical separation plant. This seemed a very far out idea, and I can remember a number of people telling me that they thought it was essentially impossible because we had no large source of plutonium. But I thought we could irradiate large amounts of uranium with the neutrons from cyclotrons since the indications were that we probably could produce sufficient plutonium, if we could learn to work on the microgram or smaller-than-microgram scale. That way we could get concentrations as large as those that would exist in the chemical separation plant.

I knew rather vaguely about two schools of ultramicrochemistry -the School of Anton Benedetti-Pichler at Queens College in New York and the School of Paul Kirk in the Department of Biochemistry at the University of California at Berkeley.

I went to New York in May 1942, looked up Benedetti Pichler, and told him that I needed a good ultramicrochemist. He introduced me to Michael Cefola, and I offered him a job, which he accepted immediately. That he was on the job about three weeks later illustrates the pace at which things moved in those days.

Then, early in June, I took a trip to Berkeley, where I looked up my friend Paul Kirk and put the same problem to him. I could not tell any of these people why we wanted to work with microgram amounts or what the material was, but this did not seem to deter their willingness to accept. Paul Kirk introduced me to Burris Cunningham. When I asked him if he would come to Chicago, he accepted and was in town by the end of the month. He told me as soon as he arrived that he had a fine student, Louis Werner, he would like to invite, and I was, of course, delighted. Werner came along is a few weeks.

These, then, are the people who began the task of isolating plutonium from large amounts of uranium. We brought from Berkeley a little cyclotron-produced sample prepared by Wahl. It contained a microgram or so of plutonium mixed with several milligrams of rare earths. Using that sample, the ultramicrochemists Cunningham, Cefola, and Werner, isolated the first visible amount--about a microgram--of pure plutonium in the form of the fluoride. It was not weighed, but it could be seen! We were all very excited when we were the first to see a man-made element on August 20, 1942 (Figure 6).

In the meantime, hundreds of pounds of uranium were being bombarded with neutrons produced by the cyclotron at Washington University, under the leadership of Alex Langsdorf, and at the 60-Inch Cyclotron at Berkeley, under the leadership of Joe Hamilton. This highly radioactive material was then shipped to Chicago. Art Jaffey, Truman Kohman, and Isadore Perlman led a team of chemists who put this material through the ether extraction process and the oxidation and reduction cycles to bring it down to a few milligrams of rare earths containing perhaps 100 micrograms of plutonium. This was turned over to Cunningham, Werner and Cefola. These men prepared the first sample in pure form by going through the plutonium iodate and the hydroxide, etc., on to the oxide.

This 2.77-microgram sample (Figure 7) was weighed on September 10, 1942. The first aim was to weigh it with a so-called Emich balance, which was somewhat complicated and had electromagnetic compensation features. As it turned out, owing to the heavy load in the shops, this weighing balance would have taken perhaps six months to build.

Cunningham then had the idea of using a simple device consisting of a quartz fiber about 12 centimeters long and 1/10 of a millimeter in diameter suspended at one end with a weighing pan hung on the other end. Then the depression of that end of the fiber with the pan containing the sample would relate to the weight of the sample. Cunningham measured the depression of the quartz fiber with a telescope. He built this balance himself, although he found out later that an Italian named Salvioni invented it earlier, and so it became known as the Salvioni balance. A description of this first isolation and first weighing of plutonium was published by Cunningham and Werner [11] after World War II.

The chemical separation (extraction) process that finally evolved had three stages: (1) the separation from uranium (extraction) and from the fission products (decontamination) used oxidation-reduction cycles with bismuth phosphate as the carrier precipitate; (2) the concentration (volume reduction) step used an oxidation-reduction cycle with rare earth fluoride as the carrier precipitate; (3) the isolation step consisted of the precipitation of pure (carrier-free) plutonium peroxide from acid solution. There was widespread concern that bismuth (III) phosphate would not carry plutonium (IV) quantitatively at the concentrations that would exist in the chemical separation plant. The critical experiments on the ultramicrochemical scale showed that plutonium (IV) phosphate is carried completely (>95%) at these concentrations. The so-called Bismuth Phosphate Process operated very successfully in both the plutonium pilot plant at Oak Ridge, Tennessee, and the production plant at Hanford, Washington.

The search for additional transuranium elements continued at the Metallurgical Laboratory, resulting during 1944-1945 in the discovery of americium (95) and curium (96).

Return to Berkeley

After the war and my return to Berkeley, in addition to my continuing research in nuclear chemistry [which led to the discovery of the transuranium elements berkelium (atomic number 97), californium (98), einsteinium (99), fermium (100), mendelevium (101) and nobelium (102)], I became involved with the new chemistry of political and societal aspects of nuclear energy as an advisor to or official in the administration of five consecutive presidents---Harry Truman, Dwight D. Eisenhower, John F. Kennedy, Lyndon B. Johnson, and Richard M. Nixon.

Near the end of 1946, President Harry Truman appointed me as a member of the nine-person General Advisory Committee (GAC) of the newly established and appointed Atomic Energy Commission (AEC). The initial members of the GAC were an awesome group--J. Robert Oppenheimer (who served as Chairman), Enrico Fermi, James B. Conant, Isidor I. Rabi, Lee A. Du Bridge, Cyril S. Smith, and industrialists Hood Worthington and Hartley Rowe. With such a membership the GAC exerted a tremendous influence on the initial Commissioners of the AEC--David E. Lilienthal (Chairman), Lewis L. Strauss, Robert F. Bacher, Sumner T. Pike and William W. Waymack. The first meeting of the GAC was held in Washington on January 3, 1947, and we met on the average of every other month until the end of my term, August 1, 1950. We advised the AEC, in a very influential manner, on the rehabilitation of the Los Alamos Weapons Laboratory (which had become somewhat disorganized after the end of the war), the operation of its facilities for the production of fissionable material, the diminishing role of secrecy in its operations, the distribution of radioactive isotopes produced in its facilities, the instigation of its marvelous program of support of basic research in U.S. universities and colleges, the operation of its national laboratories, the direction of its emerging civilian nuclear power program, its organizational structure, and many other areas where we thought our advice, sought or unsought, would be helpful.

An action that gained the most publicity was the recommendation, at a meeting in October 1949, which I missed due to a visit to Sweden, that the AEC not proceed with a high priority program to develop the hydrogen bomb. I had sent a letter to Oppenheimer saying that I had reluctantly come to the conclusion that the United States should proceed with such a program because it was certain that the Soviet Union would do so. The members of the GAC met with President Harry Truman in the Oval Office of the White House on January 31, 1950, to learn of his decision that the United States should proceed with the development and production of the hydrogen bomb.

On January 26, 1959, while visiting New York, I received a telephone call from James R. Killian, Jr. asking me to serve on the President's Science Advisory Committee (PSAC), which he then chaired. I gladly accepted this important assignment. As of March 1959, the PSAC membership was as follows: Killian (Massachusetts Institute of Technology), Robert F. Bacher (California Institute of Technology), William O. Baker (Bell Telephone Laboratories), John Bardeen (University of Illinois) Hans A. Bethe (Cornell University), Detlev W. Bronk (The Rockefeller Institute), Britton Chance (University of Pennsylvania School of Medicine), James B. Fisk (Bell Telephone Laboratories), George B. Kistiakowsky (Harvard University), Edwin H. Land (Polaroid Corporation), Emanuel R. Piore (International Business Machines Corporation), Edward R. Purcell (Harvard University), Isidor I. Rabi (Columbia University), H. P. Robertson (California Institute of Technology), Cyril S. Smith (The University of Chicago), Paul A. Weiss (The Rockefeller Institute), and Jerome B. Wiesner (Massachussetts Institute of Technology). In May 1959, Killian announced his resignation and Kistiakowsky assumed the chairmanship.

PSAC considered a wide range of scientific issues of national importance, spending a lot of time on defense matters and on arms limitation and the nuclear test ban. I think, without question, my most important contribution as a member of PSAC was serving as chairman of the subcommittee which wrote the report, "Scientific Progress, the Universities, and the Federal Government," commonly known as the Seaborg report. In releasing this report to the public in November 1960, President Eisenhower called particular attention to its conclusion that ". . . the process of basic scientific research and the process of graduate education in universities must be viewed as an integrated task if the nation is to produce the research results and the new scientists that will maintain the leadership of American science (Figure 8)."

Chairman of the U.S. Atomic Energy Commission

Early in January 1961, I received a telephone call from President-Elect John F. Kennedy, inviting me to serve as Chairman of the Atomic Energy Commission in his new administration. I accepted and arrived in Washington, D.C., to witness his inauguration as president on January 20, 1961. I began my duties as chairman soon thereafter. After President Kennedy's death on November 22, 1963, I was asked by President Lyndon B. Johnson to continue as AEC chairman and, at the start of his term of office, President Richard M. Nixon also asked me to continue.

As chairman, I reported directly to the president. I kept a daily journal the whole time I was in Washington, covering the wide range of contacts with Executive Office officials, members of Congress, foreign officials, industrialists, and people in many other walks of life. I also found it useful to make notes of what was discussed in these meetings, which I have since found invaluable in writing _Kennedy, Khrushchev and the Test Ban_, an account of the negotiations for the Limited Test Ban Treaty of 1963 (prohibiting testing of nuclear weapons in the atmosphere) in which I participated, and _Stemming the Tide: Arms Control in the Johnson Years_, a description of the Johnson administration's nuclear arms control efforts, especially the Nonproliferation Treaty of 1968 (which attempted to control the spread of nuclear weapons to additional countries).

The Atomic Energy Commission was responsible for many activities other than the development and testing of nuclear weapons and sponsorship of nuclear energy as a source of electricity, its most publicized projects. We also had major programs for the production of nuclear materials, reactor research, and development for the armed services (including the then-new nuclear navy), research in high and low energy physics and in chemistry and biology, sale of radioisotopes for use in nuclear medicine, agriculture, industry and research, licensing of nuclear materials for power plants and other peaceful purposes which resulted in efforts to establish international cooperation in developing the "peaceful atom."

As chairman, I was actively involved in the development of all of these programs and I found the job both challenging and fascinating. In a way, my appointment to this position was a real departure from tradition; I was the first scientist to head the AEC and new to the world of Washington politics. Apropos of the observance of the 25th anniversary of the Karlsruhe Establishment, the European Institute for Transuranium Elements, I should mention that I visited the Nuclear Research Center here 25 years ago (September 27, 1963) and among those I met at the time were Walter Schnurr (Technical Director of Karlsruhe), Erwin Willy Becker (Head of the Institute for Nuclear Process Technology), Karl Wirtz (Head of the Laboratory for Neutron and Reactor Physics), Wolf Haefele (Head of the German Fast Breeder Reactor Project), Walter Seelemann-Eggebert (Head of the Laboratory for Radiochemistry) and Rudolf Greifeld (Administrative Director for the Center) (Figure 9). Quoting from my journal:

"We were driven to the Karlsruhe Nuclear Research Center at Karlsruhe. Here we heard one hour of descriptions of the research program. We had lunch with a number of people at the Center. Then we toured the FR-2 (12

MW heavy water reactor) area, the area of the Isochronous (50 MeV deuteron) Cyclotron (that I had suggested in 1957 to Seelemann-Eggebert, during his visit to Berkeley, that they build), and the Transuranium Institute (under construction--to cost $20 million). Schnurr (Director of Karlsruhe) and Becker were our guides. After a social hour, at which I spoke about my trip to the USSR to discuss cooperation in the peaceful uses of nuclear energy and the U.S. nuclear power program, we were driven to Baden-Baden and checked into the Hotel Europiascher Hof. We visited the huge gambling casino there.

My visit to the Euratom project at Karlsruhe, although brief, gave me the impression that work there was progressing well; it seemed evident that the Institute would make significant contributions in the years to come."

U.S. civilian nuclear power

In March 1962 President Kennedy asked the AEC to take a "new and hard look at the role of nuclear power in our economy." The president asked that the study identify the objectives, scope and content of a nuclear power development program in light of the nation's prospective energy needs and resources and of advances in alternative means of power generation.

The year 1962 was an appropriate one for a "new and hard look." By this time 25 experimental or prototype nuclear power reactors had been funded by the government, while 12 others had been funded under cooperative programs with industry. From this work had come substantial advances in nuclear technology and considerable operating experience, sufficient to make the goal of economically competitive nuclear power seem attainable, at least in areas of the country with high conventional fuel costs. Not surprisingly, such progress had stimulated increased industry interest in nuclear power and in the private ownership of nuclear fuel. On the other hand, general economic conditions did not seem to warrant the construction of additional experimental facilities without more definitive program guidance. Guidance was needed particularly to help determine what reactor concepts should be emphasized in the coming period. The plants thus far built had been of several different types, each having its virtues and its champions.

Light water-cooled reactors had demonstrated their reliability, having been used extensively, for example, in nuclear submarines and in the Shippingport Atomic Power Station near Pittsburgh. They were not extremely complex either in construction or operation, and could be built and operated with available technology.

The use of nuclear superheating, to obtain higher thermal efficiencies and steam conditions more compatible with conventional turbogenerators, had been explored, for example, with the 50 Mwt Boiling Nuclear Superheat Power Station [BONUS] in Puerto Rico.

Gas-cooled systems were known to permit relatively high thermal efficiency. Potentially the coolant gas could drive a turbine directly, and this concept, known as the HTGR (High Temperature Gas-Cooled Reactor), showed promise of being able to use thorium fuel, which was in abundant supply.

Through operation of experimental reactors, it was known that liquid metal-cooled reactors could achieve high temperatures and thermal efficiency, permitting low net power costs. In addition, the liquid metal-cooled reactors could be breeder reactors. Their further development could therefore be considered essential to achieve the full benefit of nuclear power.

Heavy water-cooled and moderated reactors had been examined, but had limited support in the U.S., because of the availability of enriched uranium fuel material. (Heavy water reactors could use natural uranium fuel and required larger facilities because they could not produce as much energy per cubic foot of reactor as those using enriched fuel.)

At the end of 1971, 130 central station nuclear power plants, representing an aggregate capacity of more than 108,600 net megawatts of electricity (Mwe) were built, under construction or planned in the United States, as follows: there were 25 operable units (including two licensed for fuel loading and subcritical testing), representing a total capacity of 11,400 Mwe; 52 units (44,500 Mwe) were under construction or being reviewed for operating licenses;

39 units were under AEC review for construction permits, representing 38,400 Mwe of initial capacity; and there were 14 units for which utilities had contracted but not yet filed construction permit applications, representing 14,000 Mwe.

However, in the following years, anti-nuclear sentiment in the United States (a phenomenon shared by many other countries) led to the cancellation of many of the orders by utilities for the purchase of nuclear power plants and to a cessation by utilities of orders for new nuclear power plants.

The Limited Test Ban Treaty (LTBT)

President Kennedy (Figure 10) was deeply committed to achieving a nuclear test ban treaty with the Soviet Union and he pursued this goal persistently, despite numerous discouragements, showing sensitivity and patience in his diplomatic relations with both the Soviet Union (meaning, basically, with Nikita Khrushchev) and with the United States Senate. Discussions within the Committee of Principals, in which I participated, to define a U.S. position began immediately, in February 1961, and negotiation with the Soviet Union, within a matter of weeks thereafter, in March 1961. A draft treaty was introduced by the U.S. and U.K. in April 1961. It would have banned all but smaller underground tests; offered a moratorium on such tests; and allowed the Soviets to inspect devices we proposed to use for seismic research or for AEC's Plowshare (peaceful nuclear explosions) program. We also agreed to a Soviet suggestion that the number of onsite inspections on the soil of each party be limited to an annual quota. The most serious disagreement was over the size of this inspection quota: we proposed it be 20, the Soviets, while contending that no inspections were necessary, offered to accept three as a political concession to Kennedy. Over the ensuing two years we several times modified our quota demand until in February 1963 our chief negotiator was authorized to produce the number six as a final fall-back offer. But the Soviets would go no higher than three.

In August 1961 the Soviets surprised us by breaking an informal test moratorium begun three years earlier and launching a massive series of atmospheric tests. After some hesitation, President Kennedy authorized a series of U.S. atmospheric tests which took place in the Pacific between April and November 1962.

President Kennedy's extraordinary commencement address at American University on June 10, 1963, finally set the stage for the high-level negotiations with the Soviet Union. Kennedy chose W. Averell Harriman, the experienced American diplomat, who had the respect of the Soviet leadership, to lead the U.S.-U.K. negotiating team in Moscow. On the specific issue of a test ban, Harriman was instructed that the achievement of a comprehensive test ban remained the U.S. objective. If that was unobtainable, he was to seek a limited treaty in three environments, (atmosphere, water and space) along the lines of a Western draft treaty of August 1962. Khrushchev made it clear before the emissaries arrived, however, that he was prepared to accept only a limited test ban, not the comprehensive agreement Kennedy wanted.

Harriman made an unsuccessful attempt to negotiate a Comprehensive Test Ban Treaty, then went on to negotiate the details of the Limited Test Ban Treaty. In 12 days of intensive negotiation in July, which Kennedy supervised on a daily basis, Foreign Minister Gromyko and Averell Harriman, leader of the small U.S. negotiating team, with minor British participation reached agreement on a treaty. It banned all tests in the atmosphere, outer space, and under water, environments where verification was feasible without onsite inspection. In order to achieve agreement with the Soviets, Harriman had to give up the U.S. peaceful uses of nuclear explosives (the Plowshare) provision in exchange for Soviet acceptance of a withdrawal clause.

I was pleased to be a member of Secretary of State Dean Rusk's delegation, which flew to Moscow for the signing, on August 5, 1963, exactly 18 years after Hiroshima, of the Limited Test Ban Treaty. We met with Soviet Chairman

Nikita Khrushchev for an hour in his office in the Kremlin in the morning to discuss the significance of the Treaty, the future of East West relations, etc. The Treaty was signed at 4:30 p.m. in the Kremlin's Catherine Hall by Rusk, Soviet Foreign Minister Andrei Gromyko and British Foreign Minister Lord Home.

The Nonproliferation Treaty (NPT)

It was fear of the further spread of nuclear weapons more than any other consideration that prompted President Kennedy's push for a comprehensive test ban. Kennedy was so concerned about China acquiring the bomb that he authorized Averell Harriman, when the latter was in Moscow negotiating the Limited Test Ban Treaty, to feel out Khrushchev on the subject of launching a joint preemptive strike on China's nuclear facilities. Khrushchev shrugged off the suggestion -he said he didn't think China would be a serious nuclear threat.

By the time Lyndon Johnson became president (Figure 11), the Arms Control and Disarmament Agency had adopted nonproliferation as its number one objective. This position conflicted with another objective, which had strong support in the State Department, namely, the establishment of a NATO naval force, manned by personnel from several nations, and equipped with U.S. nuclear weapons, the so-called Multilateral Force (MLF). The purposes of the MLF included giving NATO countries, particularly Germany, a greater role in planning their own defense, thereby helping to dissuade them from wanting to be independent nuclear powers; preserving allied cohesion in the face of the Soviet threat; and encouraging the budding movement toward a united Europe. While it could be, and was, argued that the MLF and a nonproliferation treaty were not inconsistent, the former tended to exclude the latter because of the Soviet Union's attitude. The Soviets were fiercely hostile to a scheme that they felt might place a revengeful West German finger on the nuclear trigger. They made it clear they would not join in an NPT unless we abandoned the MLF.

Despite the political problems, technical work on the MLF went forward, and when Johnson became president he was immediately subjected to strong pressures from MLF advocates in the State Department. Following some intense discussion within the administration he authorized a campaign to sell the idea to our allies, hoping to reach agreement by the end of 1964.

But then, on October 16, 1964, my journal contained the following entry:

"The big news today is that at 3 a.m. Washington time the Red Chinese exploded an atomic bomb in the atmosphere."

Our analysis of the debris convinced us, to our surprise, that the Chinese had detonated a ^{235}U device of sophisticated design, not a plutonium bomb such as the other four nuclear powers had used for their first tests. I reported these findings to a presidential Cabinet meeting on August 20.

The Chinese test had long been expected, but the actual occurrence nevertheless shook up the whole international equation. Potent forces in India immediately began agitating for an Indian bomb to match China's. This made the Pakistanis edgy. The Australians began to stir. Proliferation seemed to be in the air. The need for an NPT seemed more urgent.

President Johnson had to confront the MLF issue seriously in December 1964. The occasion was a visit by British Prime Minister Harold Wilson. The principal item on the agenda was the MLF, and the British had made no secret of their opposition. But it was probably the runup to the meeting rather than the meeting itself that had the biggest effect on the President's mind. In five days of intensive meetings with his principal advisors, Johnson grappled with the MLF question, seeking a policy position of his own. In the end he determined that the United States, while not opposing the MLF, would no longer actively try to bring it about.

The president's new position, by seeming to remove the MLF obstacle, really energized the diplomatic quest for an NPT. In August 1965 the United States unfurled a complete draft at the Eighteen Nation Disarmament Conference

(ENDC). The draft did not fully rule out a future MLF, however--die-hards in State had managed to keep it alive--so the Soviets promptly rejected the draft. The Soviets wanted to outlaw any transfer of nuclear weapons whatever--their position seemed to bar even existing NATO arrangements by which U.S. weapons were stationed in Europe. Then Secretary McNamara devised a substitute for the MLF--the idea of a consultative committee to devise NATO nuclear strategy. This seemed to satisfy the motive of giving Germany and other NATO allies a voice in their own nuclear defense.

The situation now seemed ready for forward movement on an NPT. The missing ingredient was presidential involvement. President Johnson had become somewhat disengaged from arms control matters because of his preoccupation with the Vietnam War following the major escalation early in 1965. Pressures to get him to focus again on the NPT came from a number of directions. One was a Senate resolution in May 1966 that urged "additional efforts by the president. . . for the solution of nuclear proliferation problems." Next, some inside the administration managed through Bill Moyers, to get to the president and make the case on the urgency of getting an NPT. The break seemed to come on July 5, 1966, when, in answer to a question at a news conference, the president stated: "We are going to do everything within the power of our most imaginative people to find language which will bring the nuclear powers together in a treaty which will provide nonproliferation." Secretary of State Rusk, previously quite removed form the issue, now became for the first time an active and very effective NPT advocate.

On October 10, 1966 Foreign Minister Gromyko showed up at the White House in a visit full of smiles, indicating that the process had borne fruit. On December 5, 1966, the two sides unveiled the text of the first two articles of an NPT. Article I forebade states having nuclear weapons from transferring them "to any recipient whatsoever." Article II forebade States not having nuclear weapons from accepting their transfer or manufacturing them. Article

I essentially ruled out the MLF. The United States, however, prepared a series of interpretations which we told the Soviets would be submitted to the Senate with the treaty. Most important of these was that the treaty would not prevent a federated European state, if one ever developed, from inheriting the nuclear weapons of Britain or France, or both. Apparently, the Soviets considered this eventuality sufficiently remote that they were willing to take a chance on it.

After the breakthrough on Articles I and II, there was still one other important matter to clear up. This concerned so-called "safeguards," meaning inspections and other mechanisms for detecting on a timely basis any diversion of nuclear materials from peaceful to weapons uses. In this matter the AEC became embroiled in a dispute with other parts of the U.S. government. We wanted safeguards, preferably administered by the International Atomic Energy Agency, to be made mandatory. Our European allies resisted mandatory safeguards, ostensibly because they did not like the idea of inspectors from other countries roaming around in their nuclear plants. They were supported in this attitude by elements in our State Department. The ACDA, bowing to allied and State Department pressure, at first introduced in Geneva a miserably weak treaty provision specifying merely that the parties to the treaty would "cooperate in facilitating the application of safeguards." The AEC bitterly protested the weakness of this provision, and our position won support from the Joint Committee on Atomic Energy. In fact, the JCAE implied that any treaty that did not have mandatory safeguards would be in trouble in the Senate. This helped tilt the balance and mandatory safeguards for all non-nuclear weapon countries soon became the U.S. position.

It did not, however, settle the question of who would administer the safeguards. In deference to our European allies, the U.S. argued in Gene[illegible] for a formula specifying "International Atomic Energy Agency or equivalent" safeguards. "Or equivalent" was a reference to safeguards already being applied to its members by the European Atomic Energy Community (EURATOM). Several allied countries very much preferred EURATOM to IAEA safeguards. Their argument was that IAEA inspectors might make off with industrial secrets about their growing nuclear businesses.

But the Soviets stated that "self-inspection" by EURATOM of its own members was unacceptable. Various compromise proposals were then thrown into the mix, all seeking some way that EURATOM safeguards could remain, at least for a while, subject to some verification of their adequacy by the IAEA. At length, informal talks among negotiators from the two sides produced basic agreement on a compromise solution. This was that each non-nuclear party to the treaty would within a specified time reach a safeguards agreement with the IAEA. This formula allowed for the possibility of continued EURATOM safeguards in that the agreements could be negotiated either individually or together with other countries.

A key step to soften allied opposition to the proposed safeguards article was taken on December 2, 1967, when President Johnson announced that the United States would accept the application of IAEA safeguards to all its own peaceful nuclear activities at the time that such safeguards were generally applied to other nations under the NPT. This announcement was the culmination of a series of prior suggestions and events in which the AEC had played a key role. The British immediately followed our example. These actions tended to cut the ground from under previous allied objections based on presumed commercial disadvantage. The allies then agreed to the text of the safeguards article and, after some last minute haggling with the Soviets over wording, the agreement was announced in Johnson's State of the Union message in January 1968.

The first three article of the NPT (Articles I and II setting out the basic obligations of nuclear-weapon states not to transfer, and nonweapon states not to acquire nuclear weapons, and Article III prescribing safeguards) pretty well encompassed what the superpowers hoped the final treaty would be. Not so the non-nuclear countries who were the main object of the treaty. There was very great resentment among them about what they considered the draft treaty's discriminatory nature. They felt they were being asked to renounce a future means of defense and without any compensation.

Ultimately three articles were added to the treaty in an effort to appease the non-nuclears. Article IV stated the right of all countries to pursue the peaceful atom without discrimination. It also announced the obligation of more advanced countries to provide technical assistance in peaceful uses to others, particularly to those in "the developing areas of the world."

Article V referred to a technology that has since declined in importance, namely, the use of nuclear explosions for peaceful purposes like excavation, mining, and research. Both Brazil and India objected to the draft NPT on the grounds that it would preclude their independent development of such explosives. In a trip to Brazil in 1967 I spoke to Brazilian officials at length about this. I pointed out to them that the USAEC stood ready under an NPT to provide a peaceful nuclear explosives service to them at a fraction of what it would cost them to provide it for themselves. I found that they were generally not well informed about the issues and that their arguments did not hold up. I became convinced that their avowed interest in peaceful nuclear explosions was mainly a cover to keep alive a nuclear weapons option. Nevertheless, to meet such objections as the Brazilians advanced, an Article V was added to the NPT providing for such a nuclear explosives service as I had described to them.

The most clamorous demand of the non-nuclears was that, in exchange for their abjuring nuclear weapons, the superpowers must do something to halt their bilateral arms race, which was regarded as a threat to everybody. The tide of revolt on this issue ran very strongly--so much so that the superpowers felt that if they did not give ground they might lose the treaty.

They therefore added an Article VI pledging "to pursue negotiations in good faith on effective measures regarding cessation of the nuclear arms race and disarmament..." Later they were forced by the efforts of Sweden's Alva Myrdal to agree to an amendment requiring that these negotiations take place "at an early date."

Formal UN debate on the NPT began in the General Assembly on April 24, 1968. It was approved on June 12 by a vote of 95 to 4, with 21 abstentions. The treaty was opened for signature on July 1, 1968, in Washington, London, and Moscow. It was signed on that day by the Big Three and more than 50 other countries.

Arms limitation

On July 1, 1968, the very day they signed the Nonproliferation Treaty, President Johnson and Soviet Premier Kosygin announced their intention to enter into definitive talks on the limitation and reduction of offensive and defensive nuclear weapons.

This was by no means the first approach to this subject, but it may have been the first serious one. During the previous four years the United States and the Soviet Union had batted back and forth a series of proposals, some of which were obviously unacceptable to the other side and probably intended mainly for propaganda effect. In January 1964, President Johnson proposed a "verified freeze on the number of strategic nuclear offensive and defensive missiles." As details of this idea were worked out in Washington, it proved quite complex, much more so than its simple statement by the president would have indicated. The Soviets never took it seriously, possibly because verification of the freeze would have required intrusion into some of the most secret Soviet facilities.

One week after Johnson's freeze proposal the Soviets proposed that the major powers destroy all their bombers. This was obviously unacceptable to the United States, which held a large lead in number of bombers. The United States responded with a proposal that both superpowers destroy an equal number of bombers. The Soviets promptly rejected this since it would have increased the proportional U.S. advantage.

The superpowers also flirted briefly during Johnson's term with reductions in military budgets as an approach to arms limitation. Late in 1963 Chairman Khrushchev announced a 4.3 percent cut in planned Soviet military expenditures for 1964. President Johnson then announced a small reduction in the U.S. defense budget for fiscal year 1965. After both sides announced they intended to make additional cuts the process was aborted by the sharp escalation in the Vietnam War initiated by Johnson early in 1965. From that time forward, military spending by both superpowers resumed an upward course.

Though the president succeeded to some extent in surrounding these actions with the aura of arms control, they were prompted largely by the excess of materials production capacity built up during the 1950s. This same excess contributed to some U.S. proposals that both sides transfer already produced stocks of weapons grade U-235 to civilian use. In August 1963 the United States formally offered to transfer 60,0000 kilograms of such U-235 if the Soviet Union would transfer 40,000 kilograms. There was scant risk in this since our stockpile at the time was about five times that of the Soviets. Early in 1964 President Johnson suggested a halt in production of fissionable materials for weapons purposes and offered to act quickly on our past offer of a transfer to peaceful purposes in a 60-40 ratio. The Soviet response on both occasions was cold. They claimed that the amounts transferred would not diminish the U.S. nuclear potential, because we had excess weapons, that the verification procedures would require the most intrusive controls, and that, in general, the proposals amounted to "control without disarmament." To meet the last objection, we proposed that the transferred material be obtained from destruction of weapons chosen by each side from its stocks. U.S. efforts on behalf of such proposals reached their peak in 1965 and early in 1966. We ceased to press them thereafter, in part because our lead over the Soviets in stockpiles of fissionable materials was diminishing rapidly.

Meanwhile, both sides had been adding new and better weapons to their arsenals. One aspect of the continuing arms race appeared particularly alarming to serious-minded individuals. This was the deployment, first noticed in 1964, of an antiballistic missile system around Moscow, and rising pressure within the United States to deploy similar systems, then under development, to protect American cities.

In March 1966, Secretary of Defense MacNamara tried to still the clamor for an American ABM by stating it would not be capable of defending against a Soviet attack, although it might be effective against a lesser Chinese attack. He suggested that funds already authorized for an ABM system not be spent until arms limitation was explored with the Soviet Union. President Johnson agreed and was strengthened in this belief by a climactic meeting of his advisers held in Austin, Texas, in December 1966. He wrote to Kosygin in January 1967 setting forth the situation quite bluntly: if the Soviets deployed an ABM, we would follow suit, and also would increase our capabilities to penetrate their system. They would then increase their offensive and defensive capabilities and both sides would have incurred "colossal costs without substantially enhancing...security.." Johnson therefore suggested that some of the two sides' "highest authorities" meet to "carry the matter forward."

In response to the president's initiative, conflicting signals came from Moscow. Kosygin made public statements defending the Soviet ABM. This was in keeping with the Soviet military doctrine's emphasis on defense. At length, a month after the president's letter, the Soviets replied, stating their willingness to exchange views on strategic weapons but without suggesting a date. Meanwhile, discussions began within the U.S. government about the position we should take in the talks. The Joint Chiefs wanted any agreement to take the form of a treaty and that it both assure continued U.S. strategic superiority and allow future development of an American ABM. State and ACDA were less obdurate.

Preliminary discussions with the Soviets about arms limitation took place at a hastily arranged summit meeting between Johnson and Kosygin at Glassboro, New Jersey on June 23 and 24, 1967. The climax of the meeting was a passionate effort by MacNamara, over lunch, to persuade Kosygin that the security interests of both sides required some limitation of strategic arms. Kosygin appeared not to respond, continuing to argue that defense threatened no one. Yet there was evidence that he and his aides were indeed impressed with the logic and force of the American presentation.

They were not impressed enough to schedule strategic arms talks, however, and in the absence of such talks weapons developments continued apace. In September 1967, at the end of a long speech in which he argued the futility of a "heavy" ABM system to protect against the Russians, MacNamara announced a "light" one (SENTINEL) to defend against the Chinese. In December it was revealed that the United States was developing MIRVs.

President Johnson continued to pressure the Soviets to schedule talks and on July 1, 1968, as indicated above, the two sides announced their intention to enter into near-term talks "on limitation and reduction of offensive strategic nuclear weapons delivery systems as well as systems of defense against ballistic missiles." Still no date was announced.

Now the task of preparing a U.S. position began in earnest. A staff in the Pentagon prepared a draft treaty. Essentially it proposed a quantitative, but not a qualitative, freeze on strategic missile launchers, and an agreement to limit ABMs to an equal, but as yet unspecified, number. An ominous limitation of the proposal was that, at the insistence of the Joint Chiefs, it did not restrict MIRVs. Thus, while the number of missile launchers might be held steady, the number of warheads could increase substantially.

On August 19, the Soviet Union finally agreed to schedule a summit conference that would launch SALT, the Strategic Arms Limitation Talks. The date was to be in the first ten days of October, the site probably Moscow. On the night of August 20, however, a few hours before the joint announcement was to be issued, news came of the invitation of Czechoslovakia by Warsaw Pact forces. Anticipating a popular outcry, President Johnson felt he had to call of the scheduled announcement.

In the remaining months of Johnson's administration, some efforts were made to get the summit conference back on the rails. These were finally defeated by President-elect Nixon, who made it clear that he would not be bound by the results of such a meeting involving his predecessor.

The Nixon administration (Figure 12) took several months to prepare before indicating a willingness to initiate SALT. A variety of options were considered. ACDA's new director, Gerard Smith, advocated an across-the-board freeze of the number and characteristics of strategic weapons. This "Stop Where We Are" proposal, which I supported, would have banned MIRVs on both sides. It would also have saved vast sums of money. The Joint Chiefs opposed this, and any other, limitation on technology.

The options were considered in a series of White House meetings in June 1969 which I attended. At one of these President Nixon stated with great emphasis that he would personally make all decisions regarding U.S. policy, setting the stage for very close White House control of the negotiations to follow. Discussions continued in coming months but before a more limited group, from which I and White House science adviser Lee DuBridge were excluded. President Nixon and Security Adviser Henry Kissinger apparently did not feel that the advice of scientists was of much use in matters like this.

SALT did not in fact begin until November 1969. There was early agreement on the desirability of limiting ABMs. But the assymetry between the forces on the two sides led to difficulties in reaching agreement on an offensive arms.

The Soviets then sought to limit negotiations to ABMs, but the United States, fearing unlimited growth in the Soviet Union's burgeoning ICBM arsenal, insisted that offensive weapons be included as well. After a prolonged deadlock, it was decided to negotiate a permanent treaty limiting ABMs and, as a holding action, to add an interim agreement (not a treaty) restricting the growth of offensive arms for five years.

International cooperation

In 1954 the Atomic Energy Act was liberalized to permit the AEC to transmit peaceful atomic energy information, research tools, and nuclear materials to other nations under "Agreements for Cooperation" pledging the recipient not to use what was received for any military purpose. The number of such agreements greatly increased during the decade of my chairmanship. By the end of 1971 they were in effect with 30 individual nations and two international organizations (EURATOM and the IAEA).

At first, the "safeguards" to prevent military use were implemented by the United States and the cooperating nation. In accordance with what had always been the U.S. intention, this responsibility began in the mid-1960s to be transferred to the IAEA through trilateral agreements among the agency, the United States, and the recipient nation. The principle of international safeguards administration was further strengthened by the 1968 Nonproliferation Treaty, which required non-nuclear weapons signators to negotiate safeguards agreements with the IAEA.

The enthusiasm engendered by the U.S. Atoms for Peace Program led in 1955 to the convening in Geneva of a huge UN Conference on the Peaceful Uses of Atomic Energy. The success of this conference led to a second one being held in 1958, a third in 1964 and a fourth in 1971. At the first two Geneva Conferences I was a member, at the third the Chairman, of the U.S. delegation. I had the honor of being elected president of the fourth (1971)

Conference. Another repeated occasion for travel aboard was the IAEA General Conference. During my ten and a half years as AEC chairman, I, along with one or more of my fellow commissioners, attended this annual event eleven times, held in Vienna except in 1965 when it was held in Tokyo. In 1966 I had the honor of presenting the AEC's Fermi Award to Otto Hahn and Fritz Strassmann during the meeting of the IAEA in Vienna (Figure 13).

It became my practice to visit other countries before and after the various conferences I attended. Thus, in 1965, when the IAEA General Conference was held in Tokyo, I visited nine countries in a trip around the world. A presidential plane was placed at my disposal for three of my trips: in January 1967 when I circled the globe in visiting five countries; in January 1970 for a trip to six African countries, Spain, and Germany; and in July 1971, when I visited six South American countries. One highlight of my travels abroad occurred in September 1964. Leaving the third Geneva Conference for a weekend, I served as host to high-ranking officials of 15 national nuclear energy organizations abroad the USNS Savannah, the world's first nuclear-powered cargo-passenger ship (Figure 14). The Savannah, which had started operation in August 1962, was completing a tour of the Scandinavian countries and was at anchor in Halsingborg, Sweden. My guests and I spent the night aboard ship, then cruised the Baltic the next day.

These trips involved extended separations from my family, disruptions of normal eating and sleeping habits, exhausting schedules at nearly every stop, intensive in-flight "homework" to prepare for the next visit, a host of minor frustrations and inconveniences, and, on return, a mountain of accumulated work. But the rewards were great. I am convinced that my personal discussions with scientists and statesmen of other nations, and visits to their scientific facilities, contributed significantly to the constructive use of the peaceful atom and nuclear safeguards and to better international relations generally. It was gratifying to know that President Johnson, for one, in repeatedly urging me to take such trips, felt the same way.

During my travels I met a rather large number of heads of state or high government officials--British Prime Minister Harold Macmillan, Soviet chairman Nikita S. Khrushchev, Soviet President Leonid I. Brezhnev, Soviet Foreign Minister Andrei A. Gromyko, and V. M. Molotov of the Soviet Union, Swedish Prime Minister Tage Erlander, Indian Prime Minister Indira Ghandi, Pakistani President Ayub Khan, President Chiang Kai-shek and Premier C. K. Yen of Taiwan, Finnish President Urho Kekkonen, Austrian Chancellors Josef Klaus and Alfors Gorbach, Austrian State Secretary Karl Gruber, Yugoslav Vice President Aleksandar Rankovic, Trygve Lie of Norway, U.N. Secretary General U Thant, Israeli Prime Minister Levi Eshkol, Irish President Eamon De Valera, Prime Minister Kittikachorn Thanan of Thailand, Brazilian Foreign Minister Jose da Magahaes Pinto, President Juan Carlos Ongania of Argentina, Mexican Foreign Minister Antonio Carrillo Flores, President Nicolae Ceausescu of Rumania, Moroccoan Foreign Minister Mohamed Syilnassi, Tunisian Foreign Minister Habib Bourguiba of Tunis, Ethiopia's Emperor Haile Selassie and Crown Price Asfa-Wossen Haile Selassie, Vice President Daniel arap Moi of Kenya, Prime Minister Kofi A. Busia of Ghana, Spanish Foreign Minister Gregorio Lopez Bravo, Prince Juan Carlos and Princess Sofia of Spain, Korean President Park Chung Hee, President Suharto of Indonesia, Prime Minister Amir Abbas Hoveyda of Iran, and Canadian Foreign Minister Mitchell Sharp.

The trips were not without some personal "spin-off"--the Danube at Budapest on a clear September day, Roman paving-stones on the Appian Way, the Bibi Khanym Mosque in Samarkand, Inca ruins in Peru, the Great Buddha at Kamakura, the Temple of Bacchus at Baalbek, the Acropolis in Athens, the ruins of Carthage, the house where Beethoven composed "Fidelio," the mighty Congo 2,000 feet below me winding through green jungle toward a dam construction site, canals in Venice, the charm of exotic animals in Australia, sunset over Scotland's downs--kaleidoscopic contacts with nature and the history of man.

Reflections

I left my position as Chairman of the U.S. Atomic Energy Commission in 1971 to resume my professorshp and nuclear research at the University of California at Berkeley. My research, with my coworkers, which resulted in the discovery of the as-yet-unnamed element 106 in 1974, has been focussed on heavy ion reactions in the transuranium region and the synthesis and identification of "superheavy" elements. (The next three transuranium elements were discovered during the 1980s in the GSI Laboratory in Darmstadt, Germany.) As throughout my career, my research has been conducted with the participation of graduate students. My contacts with Washington in the nuclear area have almost ceased. However, I am still active in the political arena as a staunch advocate of a comprehensive test ban treaty, which I regard as the "litmus test" of a country's serious intentions in the arms limitation field.

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Figure Captions

Fig. 1: Periodic Table before World War II. Parentheses indicate elements undiscovered at that time.

Fig. 2: F. Strassman, L. Meitner and O. Hahn, Mainz, 1956

Fig. 3: E. O. Lawrence, G. T. Seaborg, and J. R. Oppenheimer, Berkeley, 1946

Fig. 4: Edwin M. McMillan, Berkeley, June 8, 1940

Fig. 5: Glenn T. Seaborg with geiger counter equipment, Berkeley, 1941

Fig. 6: L. B. Werner and B. B. Cunningham, Room 405, Jones Laboratory, University of Chicago, August 20, 1942

Fig. 7: First weighed sample of plutonium (as an oxide), 2.77 micrograms, University of Chicago Metallurgical Laboratory, September 10, 1942

Fig. 8: Members of the President's Science Advisory Committee (L to R standing: George W. Beadle, Donald F. Hornig, Jerome B. Weisner, Walter H. Zinn, Harvey Brooks, Seaborg, Alvin M. Weinberg, David Z. Beckler, Emmanuel R. Piore, John W. Tukey, Wolfgang K. H. Panofsky, John Bardeen, Detlev Bronk, Robert F. Loeb; [seated] James B. Fisk, James R. Killian, Jr., Isidor I. Rabi) with President Dwight D. Eisenhower, White House, December 16, 1960

Fig. 9: Visit to German Nuclear Research Center at Karlsruhe, September 27, 1963. L to R: Karl Wirtz, Wolf Haefele, Walter Seelmann-Eggebert, Seaborg, Walter Schnurr, W. W. Williams, Rudolf Greifeld, and Erwin Willy Becker

Fig. 10: Seaborg with President John F. Kennedy, Germantown Headquarters of the Atomic Energy Commission, February 16, 1961

Fig. 11: Seaborg with President Lyndon B. Johnson, White House, January 17, 1964

Fig. 12: Seaborg with President Richard M. Nixon on the occasion of the presentation of the Atomic Pioneer Award to General Leslie R. Groves, Vannevar Bush and James R. Conant at the White House, February 27, 1970

Fig. 13: O. Hahn and F. Strassman receiving the Enrico Fermi Award from Seaborg in Vienna on September 23, 1966

Fig. 14: On board NS Savannah in cruise from Halsingborg to Malmo, September 4, 1964, (L to R): A. R. Fritsch, U. M. Staebler, (back) Gunnar Randers, (front) Harry Brynilesson, J. H. Boer, John B. Anderson, T. Gustafson, Bertrand Goldschmidt, Carlo Salvetti, Siegfried Balke, Richard L. Doan, Oscar A. Quihiilalt, H. D. Smyth, Seaborg, I. U. Usmani, Homi J. Bhabha, Sir William G. Penney, Gen. Letor, Anton Moljk, Luiz Cintra do Prado, Sakuji Komagata, Daniel M. Wilkes.

PERIODIC TABLE - BEFORE WORLD WAR II

1 H																	2 He
3 Li	4 Be											5 B	6 C	7 N	8 O	9 F	10 Ne
11 Na	12 Mg											13 Al	14 Si	15 P	16 S	17 Cl	18 Ar
19 K	20 Ca	21 Sc	22 Ti	23 V	24 Cr	25 Mn	26 Fe	27 Co	28 Ni	29 Cu	30 Zn	31 Ga	32 Ge	33 As	34 Se	35 Br	36 Kr
37 Rb	38 Sr	39 Y	40 Zr	41 Nb	42 Mo	(43)	44 Ru	45 Rh	46 Pd	47 Ag	48 Cd	49 In	50 Sn	51 Sb	52 Te	53 I	54 Xe
55 Cs	56 Ba	57-71 La- Lu	72 Hf	73 Ta	74 W	75 Re	76 Os	77 Ir	78 Pt	79 Au	80 Hg	81 Tl	82 Pb	83 Bi	84 Po	(85)	86 Rn
(87)	88 Ra	89 Ac	90 Th	91 Pa	92 U	(93)	(94)	(95)	(96)	(97)	(98)	(99)	(100)				

57 La	58 Ce	59 Pr	60 Nd	(61)	62 Sm	63 Eu	64 Gd	65 Tb	66 Dy	67 Ho	68 Er	69 Tm	70 Yb	71 Lu

XBL 769-10601

Figure 1: Periodic Table before World War II. Parentheses indicate elements undiscovered at that time

Figure 2: F. Strassman, L. Meitner, and O. Hahn, Mainz, 1956

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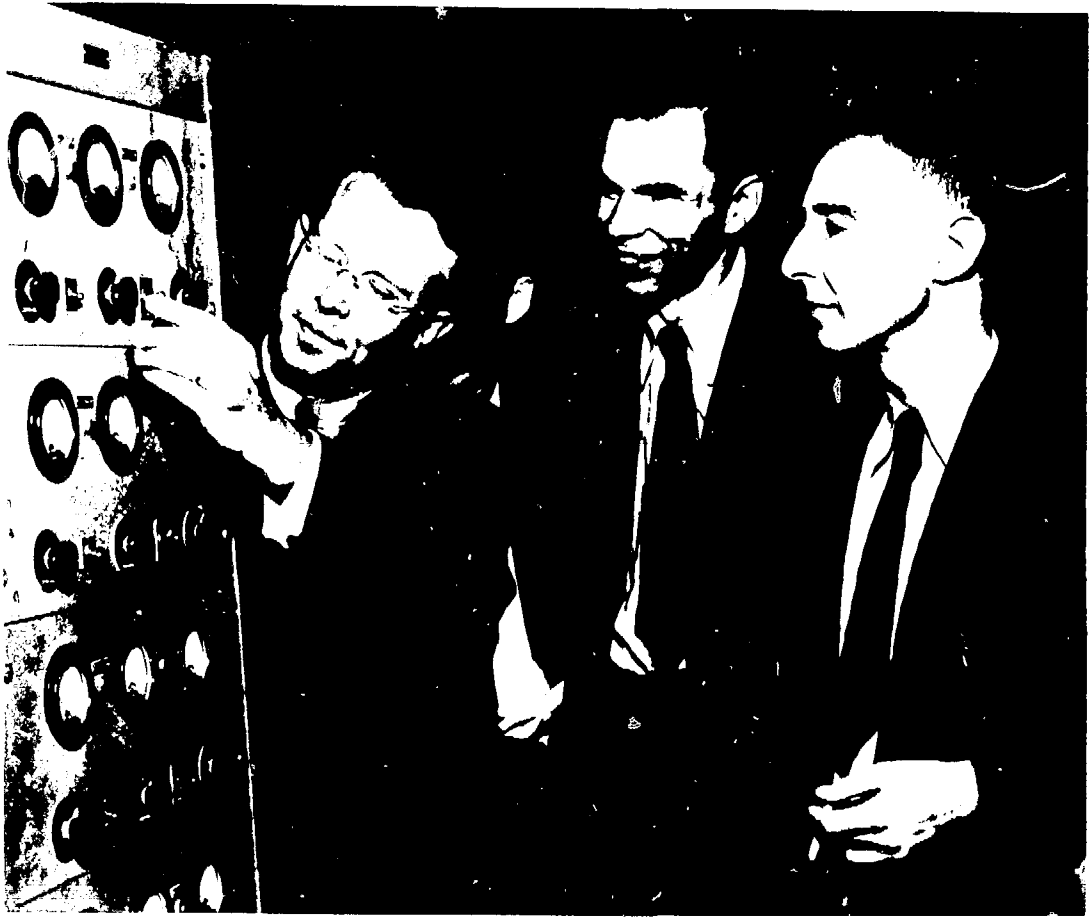

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Figure 3: E. O. Lawrence, G. T. Seaborg, and J. R. Oppenheimer, Berkeley, 1946

XBB 761-7256

Figu.e 4: Edwin M. McMillan, Berkeley, June 8, 1940

XBB 761-7413
Figure 5: Glenn T. Seaborg with geiger counter equipment, Berkeley, 1941

XBB 768-7456

Figure 6: L. B. Werner and B. B. Cunningham, Room, 405, Jones Laboratory, University of Chicago, August 20, 1942

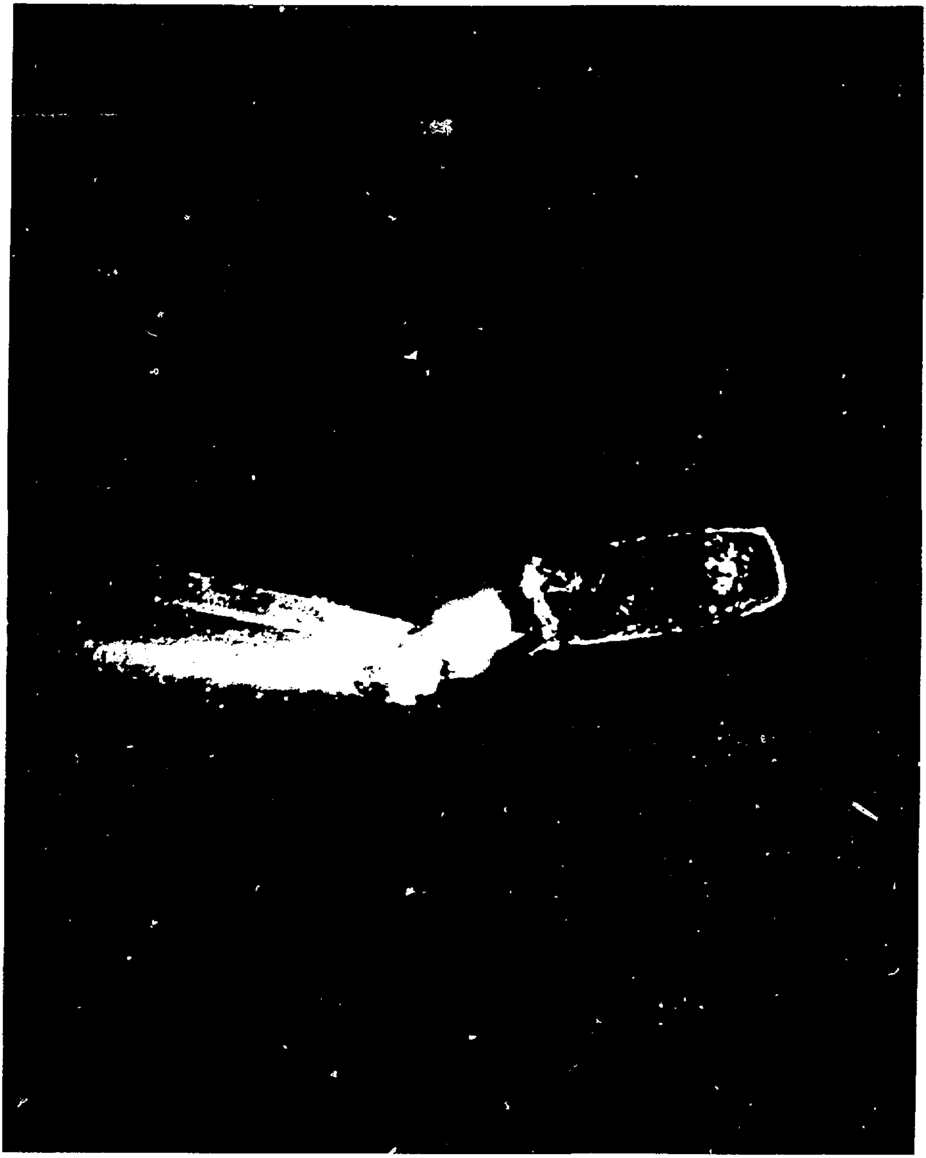

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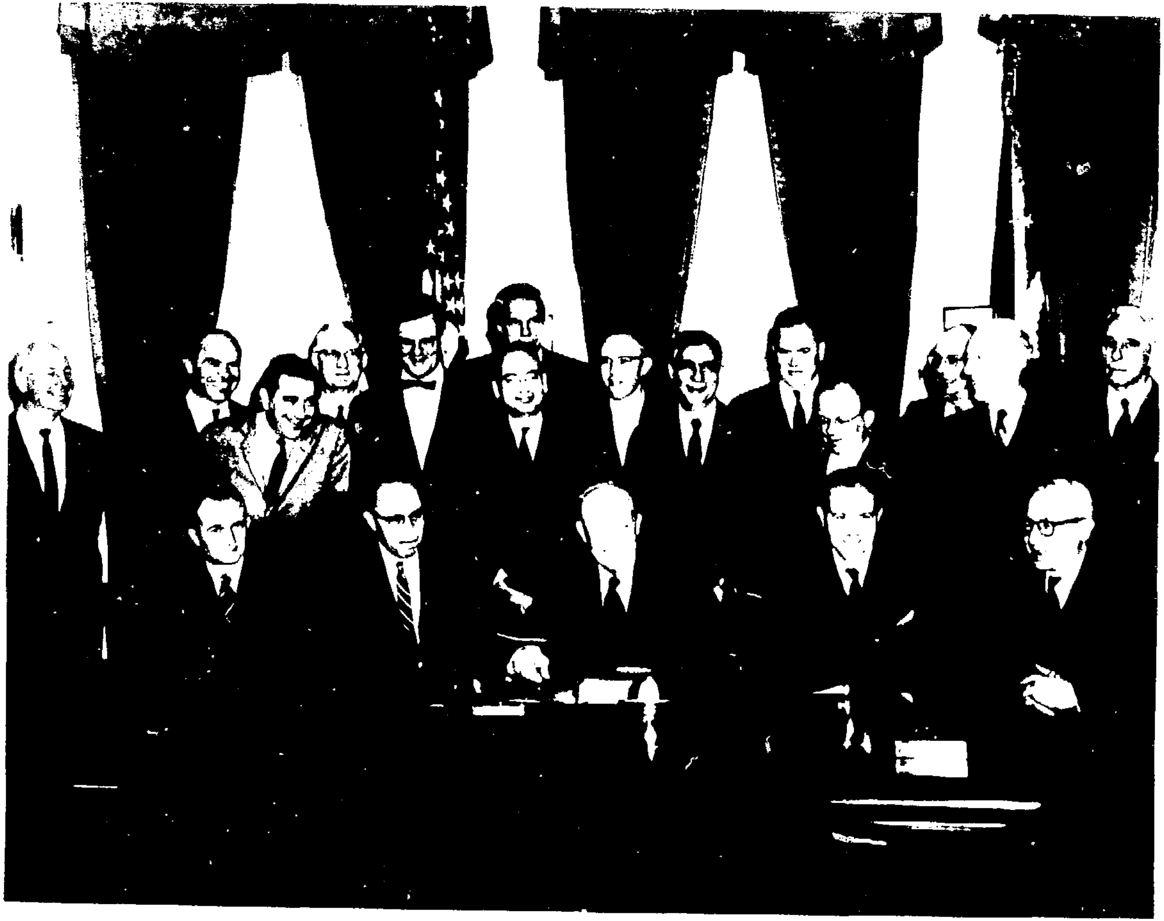
Figure 7: First weighed sample of plutonium (as an oxide)
University of Chicago Metallurgical Laboratory, September 10, 1942



XBB 888-8758

Figure 8: Members of the President's Science Advisory Committee, December 16, 1960

XBR 763-7048

Figure 9: Visit to German Nuclear Research Center at Karlsruhe, September 27, 1963

XBB 732-892

Figure 10: Seaborg with President John F. Kennedy, Germantown Headquarters of the Atomic Energy Commission, February 16, 1961

XBB 732-1147

Figure 11: Seaborg with President Lyndon B. Johnson, White House, January 17, 1964

XBB 884-3249

Figure 12: Seaborg with President Richard M. Nixon on the occasion of the presentation of the Atomic Pioneer Award to General Leslie R. Groves, Vannevar Bush and James R. Conant at the White House, February 27, 1970

XBB 732-1241

Figure 13: O. Hahn and F. Strassman receiving the Enrico Fermi Award from Seaborg in Vienna on September 23, 1966

XBB 761-7008

Figure 14: On board NS Savannah in cruise from Halsingborg to Malmo, September 4, 1964