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NUCLEAR FISSION AND TRANSURANIUM ELEMENTS - FIFTY YEARS AGO

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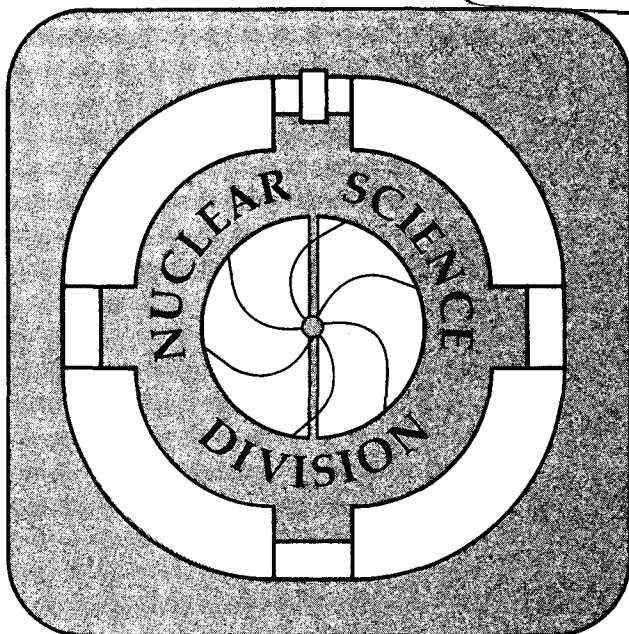
Nuclear Fission and Transuranium Elements — Fifty Years Ago

G.T. Seaborg

September 1988

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September 1988

Presented at the 196 ACS Meeting, Los Angeles, California, September 26, 1988

Golden Anniversary Symposium in celebration of the Hahn-Strassmann
experiments and the discovery of nuclear fission

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Pre-Fission and Fission

Some five years before the discovery of nuclear fission, as a first year graduate student at Berkeley in 1934, 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. Segrè 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 paper¹ written by Fermi, 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 Noddack², entitled "Über das Element 93" ["On 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 grössere Bruchstücke zerfallen, die zwar Isotope bekannter Elemente, aber nicht Nachbarn der bestrahlten Elemente sind."
 [One could think that in the bombardment of heavy nuclei with neutrons these nuclei disintegrate into several larger fragments which, although they are isotopes of known elements, are not neighbors of the irradiated elements.]

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 Strassmann³, which I read, part of a series they published during 1935-1938, they reported a 16-minute ${}_{93}\text{Eka-Re}^{237}$, 2.2-minute ${}_{93}\text{Eka}^{239}$, 12-hour ${}_{94}\text{Eka-Os}^{237}$, 59-minute ${}_{94}\text{Eka-Os}^{239}$, 3-day ${}_{95}\text{Eka-Ir}^{239}$, 12-hour ${}_{96}\text{Eka-Pt}^{239}$.

In 1938 I. Curie and P. Savitch⁴, working in Paris, 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 Radioélément De Période 3,5 Heures Formé Dans L'Uranium Irradié Par Les Neutrons" ["On the Nature of a Radioactive Element with 3.5 Hour Half-Life Produced in the Neutron Irradiation of Uranium"], and included the following:

"Nous avons montré qu'il se forme dans l'uranium irradié par les neutrons un radioélément de période 3,5 heures dont les propriétés chimiques sont semblables a celles des terres rares. Nous la désignerons 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 élément transuraniens possédant des propriétés très différentes de celles des autres éléments transuraniens connus, hypothèse qui soulève des difficultés d'interprétation."

["We have shown that in the neutron irradiation of uranium a radioactive element with a half-life of 3.5 hours is produced, with chemical properties similar to those of rare earths. In the following we will refer to it as $R_{3.5h}$.

$R_{3.5h}$ separates cleanly from Ac by going to the 'head' (beginning) of the fractionation while Ac goes to the 'tail' (end). It seems, therefore, that this species cannot be but a transuranic element having properties very different from those of the other known transuranic elements, a hypothesis which raises interpretational difficulties."]

Then came the breakthrough. Early in 1939, Hahn and Strassmann⁵, 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, "Über den Nachweis und das Verhalten der bei der Bestrahlung des Urans mittels Neutronen entstehenden Erdalkalimetalle" ["On the Identification and the Behavior of Rare Earth Metals Produced in the Neutron Irradiation of Uranium"] and contained the following conclusion:

"Als Chemiker müssten 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' können wir uns zu diesem, allen bisherigen Erfahrungen der Kernphysik widersprechenden, Sprung noch nicht entschliessen. Es könnten doch noch vielleicht eine Reihe seltsamer Zufälle unsere Ergebnisse vorgetäuscht haben."

["We, as chemists, based on the briefly described experiments, should rename the above-mentioned scheme and replace Ra, Ac, Th with the symbols Ba, La, Ce. As nuclear chemists, being in some respects close to physics, we have not yet been able to take this leap which contradicts all previous experiences in nuclear physics. It could be that a series of strange coincidences could have mimicked our results."]

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.

First Transuranium Elements (93 and 94)

With those radioactivities identified as fission products, there were no longer any transuranium elements left. However, in later investigations by Edwin M. McMillan⁶ at Berkeley and others elsewhere, one of the radioactivities behaved differently from the others. The beta radioactivity with a half-life of about 2 days did not undergo recoil. It did not separate by recoil from thin layers of uranium, as did the energetic fission products, when uranium was bombarded with slow neutrons. Along toward the spring of 1940, Ed began to come to the conclusion that the 2.3-day activity might actually be due to the daughter of the 23-minute uranium-239 and thus might indeed be an isotope of element 93 with the mass number 239 (93-239). Phil Abelson joined him in this work in the spring of 1940, and together they were able to chemically separate and identify and thus discover⁷ element 93 (Figure 3). They showed that element 93 has chemical properties similar to those of uranium and not similar to those of rhenium as suggested by the periodic table of that time (Figure 1).

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.

During this time my interest in the transuranium elements continued. Since Ed McMillan and I lived only a few rooms apart in the Faculty Club, we saw each other quite often, and, as I recall, much of our conversation, whether in the laboratory, at meals, in the hallway, or even going in and out of the shower, had something to do with element 93 and the search for element 94. I must say, therefore, that his sudden departure for M.I.T. came as something of a surprise to me--especially since I did not even know when he had left.

In the meantime, I had asked Arthur Wahl, one of my two graduate students, to begin studying the tracer chemical properties of element 93 with the idea that this might be a good subject for his thesis. My other coworker was Joe Kennedy, a fellow instructor at the University and, as I have indicated, also very interested in the general transuranium problem.

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_9O_8 , which was literally plastered onto a copper backing plate. From this bombarded material Wahl 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 about 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 communication also served for later publication in The Physical Review under the names of Seaborg, McMillan, Kennedy, and Wahl^a. 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, 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. Thus, we showed that the chemical properties of element 94 were similar to those of uranium and not like osmium (as suggested by Figure 1).

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 Kennedy⁹ (Figure 4).

How element 94 eventually got the name plutonium is an interesting story and one worth telling. This work was carried on under self-imposed secrecy in view of its potential implications for national security. Following the discovery in February 1941 and well into 1942, we used only the name "element 94" among ourselves and the few other people who knew of the element's existence. But we needed a code name to be used when we might be overheard. Someone suggested "silver" as a code name for element 93, and we decided to use "copper" for element 94. This worked fine until, for some reason I cannot recall now, it became necessary to use real copper in our work. Since we continued to call element 94 "copper" on occasion we had to refer to the real thing as "honest-to-God-copper."

The first time a true name for element 94 seemed necessary was in writing the report to the Uranium Committee in Washington in March of 1942, which was published later under the authorship of Seaborg and Wahl.¹⁰ I remember very clearly the debates within our small group as to what the name should be. It eventually became obvious to us that we should follow the lead of Ed McMillan, who had named element 93 neptunium because Neptune is the next planet after Uranus, which had served as the basis for the naming of uranium 150 years earlier. Thus we should name element 94 for Pluto, the next planet beyond Neptune. But, and this is a little-known story, it seemed to us that one way of using the base name Pluto was to name the element "plutium." We debated the question of whether the name should be "plutium" or "plutonium," the sound of which we liked much better. We finally decided to take the name that sounded better. I think we made a wise choice, and I believe it is also etymologically correct.

There was also the matter of the need for a symbol. Here, too, a great deal of debate was engendered because, although the symbol might have been "Pl," we liked the sound of "Pu"--for the reason you might suspect. We decided on "Pu," and, I might add, we expected a much greater reaction after it was declassified than we ever received.

Fission of Plutonium

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 Segrè 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. This work involved, the 60-Inch Cyclotron, the Old Chemistry Building, the Crocker Laboratory, and the 37-Inch Cyclotron, all of which have by now been removed from the Berkeley campus. 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 sample of uranyl nitrate weighing 1.2 kilograms was distributed in a large paraffin block (neutron-slowing material) placed directly behind the beryllium target of the 60-Inch Cyclotron and was bombarded for two days with neutrons produced by the impact of the full deuteron beam on beryllium. The irradiated uranyl nitrate was placed in a continuously-operating glass extraction apparatus, and the uranyl nitrate was extracted into diethyl ether. Neptunium-239 was isolated from the aqueous layer by use of the oxidation-reduction principle (described later in this section) with lanthanum and cerium fluoride carrier and was reprecipitated six times in order to

remove all uranium impurity. Measurement of the radiation from the neptunium-239 made it possible to calculate that 0.5 microgram was present to yield plutonium-239 upon decay. The resulting alpha activity corresponded to a half-life of 30,000 years for the daughter plutonium-239, in demonstrable agreement with the present best value for the half-life of 24,360 years.

The group first demonstrated, on March 28, 1941, with the sample containing 0.5 microgram of plutonium-239, that this isotope undergoes slow neutron-induced fission with a probability of reaction comparable to that of uranium-235. The sample was placed near the screened window of an ionization chamber that could detect the fissions of plutonium-239. Neutrons were then produced near the sample by bombarding a beryllium target with deuterons in the 37-Inch Cyclotron of Berkeley's "Old Radiation Laboratory" (the name applied to the original wooden building, since torn down to make way for modern buildings). Paraffin around the sample slowed the neutrons down so they would be captured more readily by the plutonium. This experiment gave a small but detectable fission rate when a six microampere beam of deuterons was used. To increase the accuracy of the measurement of the fission cross section, this sample, which had about five milligrams of rare-earth carrier materials, was subjected to an oxidation-reduction chemical procedure that reduced the amount of carrier to a few tenths of a milligram. 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.¹¹

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 (MED) centered at the Metallurgical Laboratory 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 in 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 5).

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 was weighed on September 10, 1942 (Figure 6). 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^{1,2} 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.

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Figure Captions
for
Nuclear Fission and the Transuranium Elements--50 Years Ago

- 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: Edwin M. McMillan, Berkeley, June 8, 1940
- Fig. 4: Glenn T. Seaborg with geiger counter equipment, Berkeley, 1941
- Fig. 5: L. B. Werner and B. B. Cunningham, Room 405, Jones Laboratory, University of Chicago, August 20, 1942
- Fig. 6: First weighed sample of plutonium (as an oxide), University of Chicago Metallurgical Laboratory, September 10, 1942

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		

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

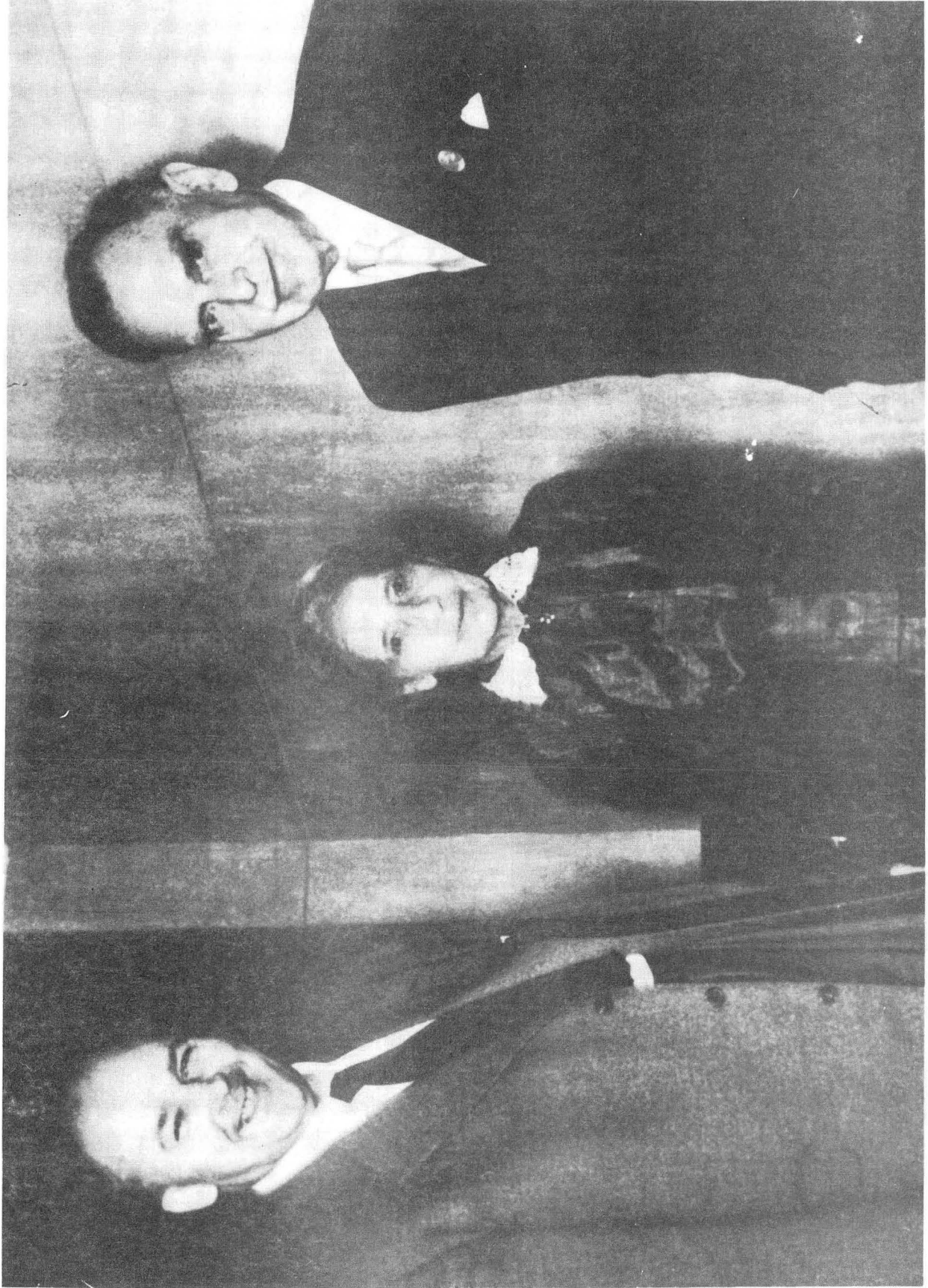
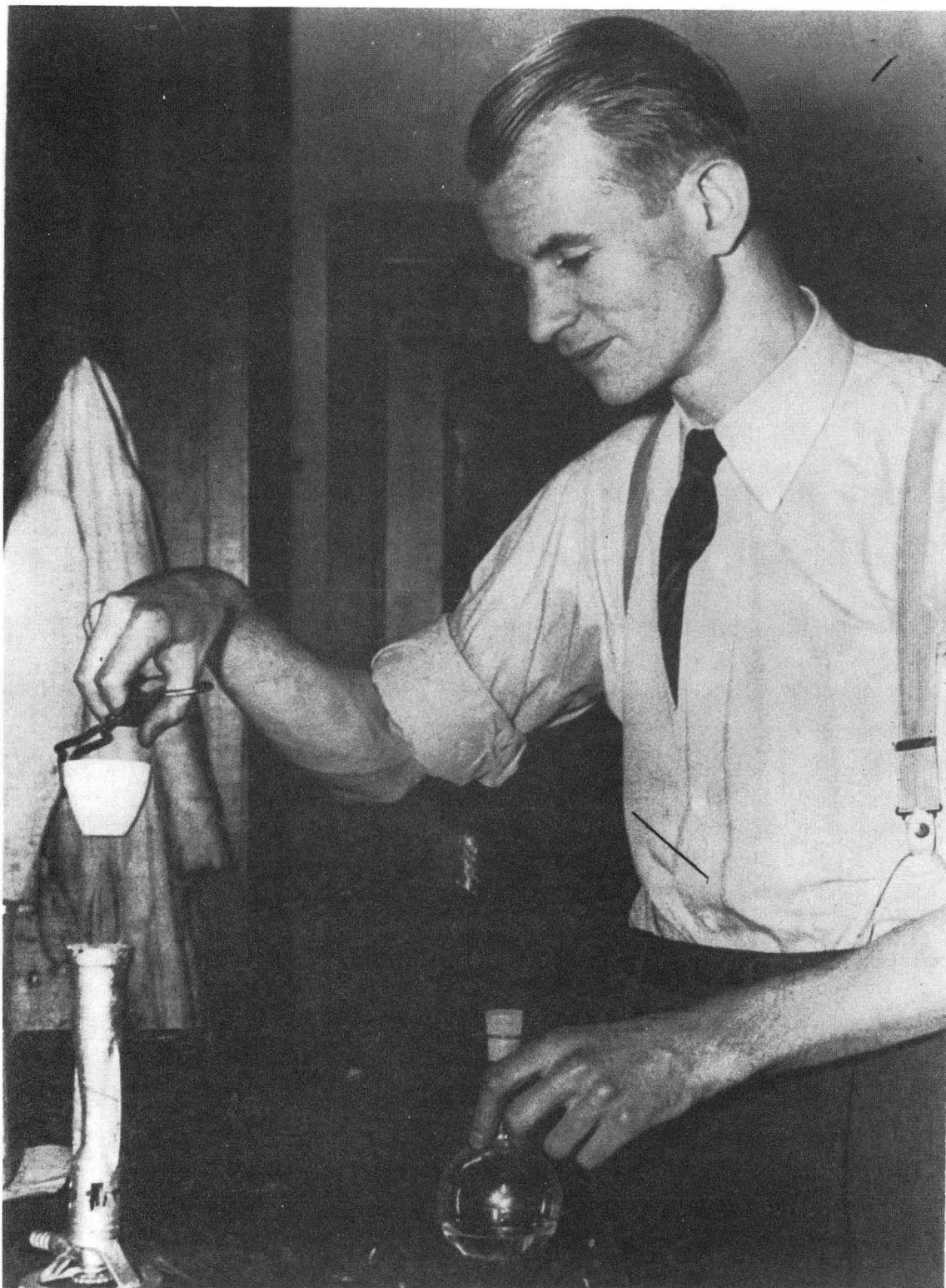


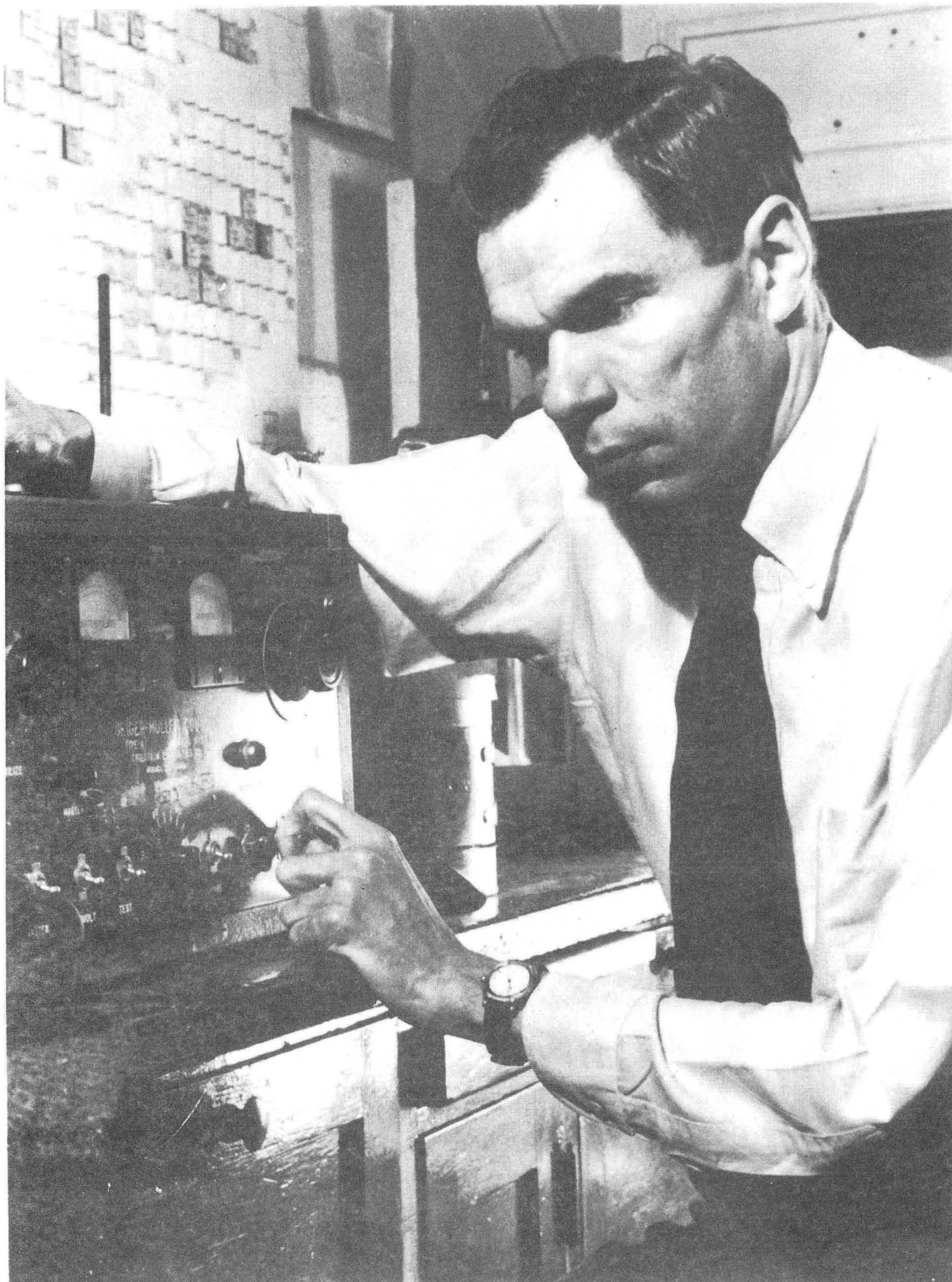
Figure 2: F. Strassman, L. Meitner, and O. Hahn, Mainz, 1956

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Figure 3: Edwin M. McMillan, Berkeley, June 8, 1940



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Figure 4: Glenn T. Seaborg with geiger counter equipment, Berkeley, 1941

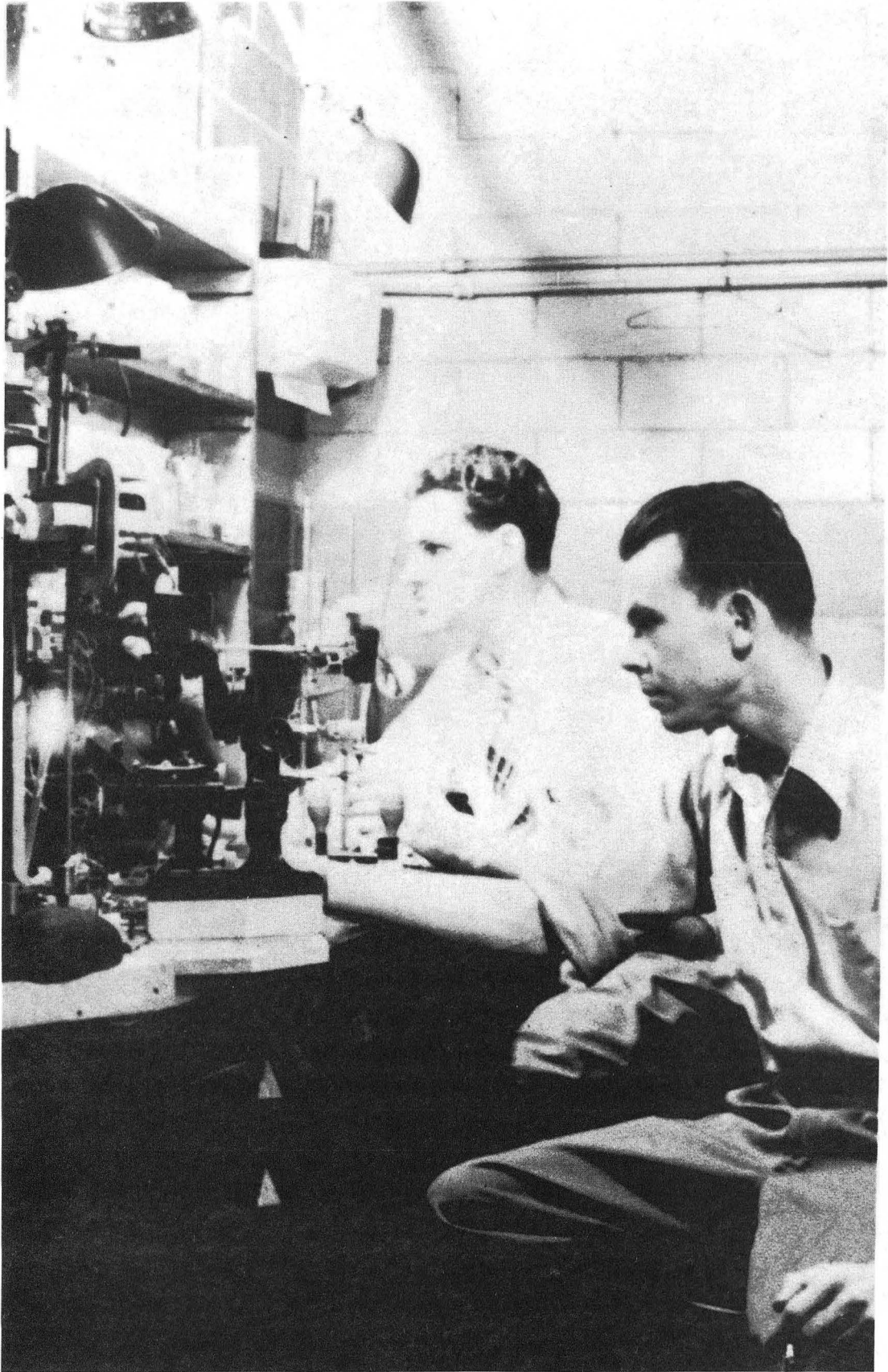
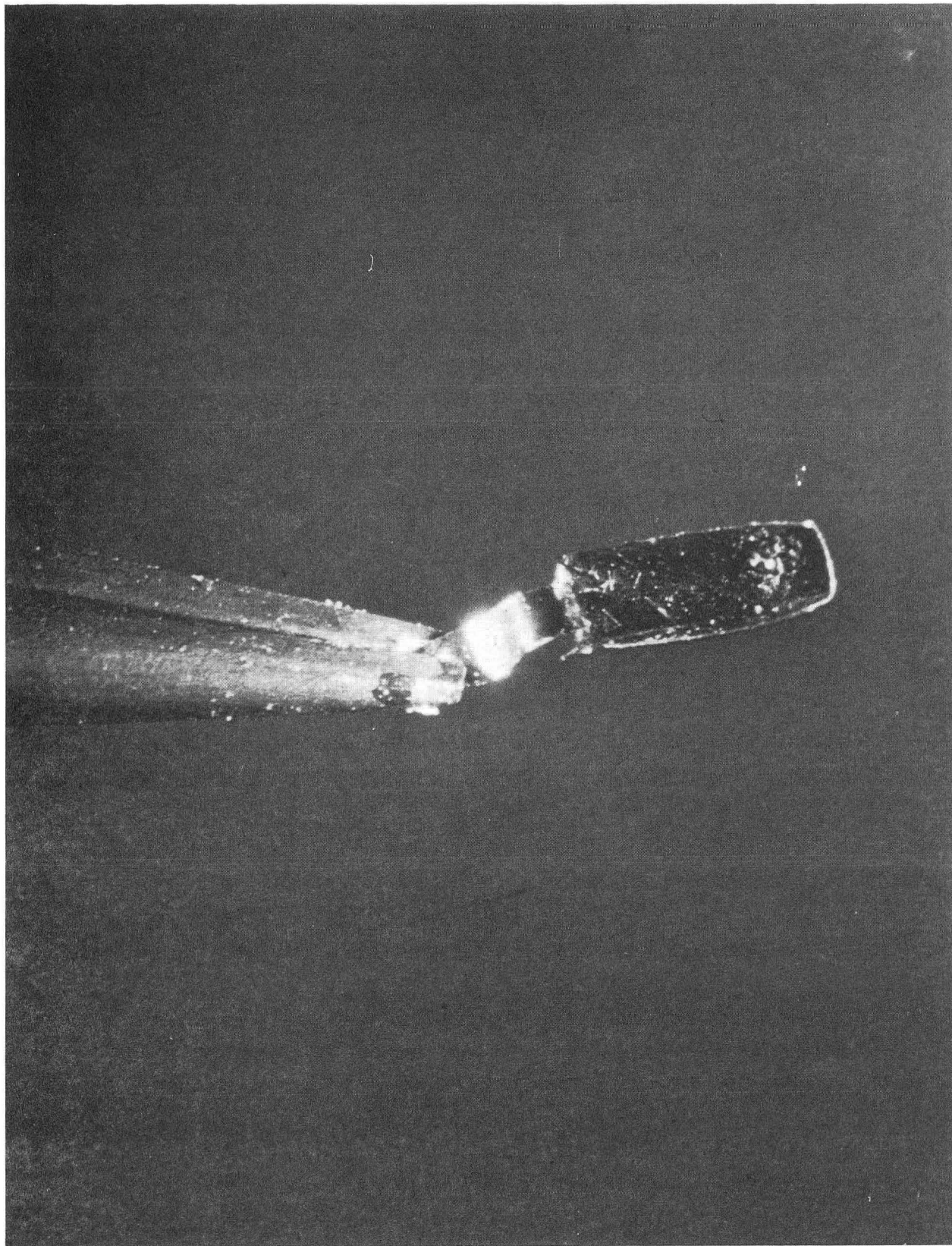


Figure 5: L. B. Werner and B. B. Cunningham, Room 405,
Jones Laboratory, University of Chicago, August 20, 1942

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Figure 6: First weighed sample of plutonium (as an oxide),
University of Chicago Metallurgical Laboratory, September 10, 1942

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