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GLENN T. SEABORG

Chief, Section C-1, Metallurgical Laboratory, Manhattan Engineer District 1942-1946

May 1, 1945 - May 19, 1946

Lawrence Berkeley Laboratory
University of California

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Journal

of

GLENN T. SEABORG

1942-1946

VOLUME 4

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PREFACE

This is volume IV, the last of a four-volume history of the research work of the many chemists who worked with me in the University of Chicago Metallurgical Laboratory during World War II. The work of these groups, which were affiliated under my direction in a unit which became known as "Chemistry Section C-I" as part of a broader Chemistry Division, was concerned with the development of chemical procedures for the extraction of plutonium, for the purification of plutonium and, in the later phases, for research on the isotopes of other heavy elements including other transuranium elements. Volume I, "History of Met Lab Section C-I, April 1942 to April 1943," was published as LBL special report PUB-112 in February 1977; Volume II, "History of Met Lab Section C-I, May 1943 to April 1944," was published as LBL special report PUB-112 in May 1978; and Volume III, "History of Met Lab Section C-I, May 1944 to April 1945," was published as LBL special report PUB-112 in May 1979. I began this project about twelve years ago, while I was Chairman of the Atomic Energy Commission, never suspecting that it would take so long to complete.

This history is written on the basis of some 60 categories of information, including a wide range of Met Lab Progress Reports, notes on meetings, an almost complete file of all the Laboratory notebooks, personnel records, patent files, travel vouchers, organization charts, records of pile and cyclotron bombardments, health monitoring records, administrative bulletins, etc. Unfortunately, I did not keep a diary, but we were able to locate hundreds of notes I had taken to cover meetings, telephone calls, etc.; these were often written in a style that amounted to a code (in order to protect the secret nature of the information), but I found it possible to decipher them. This was one of the most important categories of information, as was the file of meeting notes which had been issued as informal reports and which usually included names of the attendees. Another welcome source was a very brief diary, actually an intermittent daily notation or a few words in a small pocket-size appointment book, kept by my wife Helen; this made it possible to include a large number of entries concerning our social life during most of the period 1942-1946 when we were in Chicago.

The style is such that the entry for each day is written as though it was entered in a diary on the basis of information available to me at the end of the day. There is more reliance on quotations from letters, written or received, than would be usual for an actual diary, which is justified on the basis that this kind of information comes closest to emulating a diary. An exception to this style of diary imitation is the footnotes that are included to give additional background material; these go beyond the activities of Chemistry Section

C-I, covering meetings that I did not attend, and hence often include information that I could not have had on those entry dates.

Essentially all of the events, and the dates on which they are recorded, are based on the numerous categories of documentation; only a very small portion is based on memory alone, and even then it is usually associated with related information based on documentation. Chicago and other newspapers for this period, available in libraries, and weather records were used to embellish the narrative with some additional information on current events.

To help me in this writing task I wrote and talked to many who participated with me in the Met Lab experience to seek their recollections to augment the documented record. I am very grateful for this help. Since photography was not encouraged, the supply of illustrations is limited. I appreciate receiving photographs from the personal albums of Milton Ader, Herman Robinson, Don Stewart, and Roy Thompson and I express my thanks to Milton Burton, Phillip and Olga Fineman, Donald Stewart, and Gertrude Steel for help in identifying a number of the photographs. I also appreciate the opportunity to use a number of photographs taken by Fritz Goro, photographer for Life magazine.

I am indebted to Joseph Katz, Carol Flaumenhaft, Sydney Gaarder, Bernard Saunders, Donald Stewart, and Peter Seaborg for help in gathering and preparing materials, and to Margie Hollander, Kathleen Van Der Haeghen, and Helen Seaborg for much help in putting this volume into publishable form. Margie Hollander played a key role in assembling the material for all four of the volumes.

MAY 1945

Tuesday, May 1, 1945

A 1-ml sample of Hanford dissolver solution was delivered to the Metallurgical Laboratory.

I read copies of three memos dated April 30 from Zachariasen to Stearns covering identification of the following compounds: (a) $KPuF_5$ which is isomorphous with KUF_5, $KThF_5$, and $NaUF_5$. This phase had previously been identified (CN-1813) as $K_2PuF_6 \cdot xH_2O$; (b) the system $NaF-UF_4$ which shows four phases (results still incomplete); (c) U_2F_9 and $NaTh_2F_9$ which have identical x-ray diffraction patterns.

Hogness prepared a summary of the manpower distribution in the Chemistry Division which shows the following for my section:

		Number of Men Mar.	Apr.
Albaugh (Separation Processes, 26 men)	Thompson, Extraction and decontamination	6	6
	Gilbreath, Process development	7	7
	Lawroski, Solvent extraction	12	12
Cunningham (Basic Chemistry, 36 men)	Simpson, High vacuum work	9	9
	Hindman, Basic chemistry	11	11
	Stewart, Recovery	6	6
	Ghiorso, Instruments and physical instruments	9	9
Katzin (23 work, 8 men)	23 work	7	7
Administration -	Seaborg, Manning, Jones, Albaugh, Cunningham, Katzin	6	6
	Total	73	73

The extraction and decontamination group is still included, but now it has been disbanded.

I prepared a detailed organization of my section as of May 1. It is as follows:

Glenn T. Seaborg - Section Chief
Edrey Albaugh - Secretary to Seaborg
Ruth P. Rogers - Secretary to Seaborg

Winston M. Manning - Associate Section Chief
Jane Horwich - Secretary to Manning
Thomas O. Jones - Assistant to the Section Chief
Mary Williams - Secretary
Beatrice Greenberg - Typist

Sub-section I, Separation Processes

Frederick W. Albaugh - Assistant Section Chief
Dorothy Black - Secretary to Albaugh

Group 3, Process Development,
Gilbreath, James R. - Group Leader
Blaedel, Walter J. - Research Associate
Bradt, Rexford - Research Associate
Hopkins, Horace [SED] - Research Assistant
Post, Roy [SED] - Research Assistant
Sedlet, Jacob - Research Assistant
Walling, Mathew T. - Research Assistant
Winner, Bernard - Research Assistant
Bolden, Mildred - Secretary
Boykin, Pearline - Technician

Group 4, Solvent Extraction,
Lawroski, Stephen - Group Leader
Egan, C. J. - Assistant Group Leader
Schaffner, Irwin J. - Research Associate
Simon, Wilbur C. - Research Associate
Hyman, Herbert H. - Research Associate
Ader, Milton [SED] - Research Associate
Hausman, Eugene A. [SED] - Research Assistant (half-time)
Kelley, Alec [SED] - Research Assistant
Reinhardt, Richard - Research Assistant
Schraidt, John H. [SED] - Research Assistant
Dorcy, Dan J. - Draftsman
Giacchetti, Olga - Technician (half-time)
Murray, Betty - Technician
Krinsky, Jerome - Technician (half-time)
Summers, Mildred - Technician

Sub-section II, Basic Chemistry and Service

Burris B. Cunningham - Assistant Section Chief

Group 5, Basic Dry Chemistry,
Simpson, Oliver C. - Group Leader
Davidson, Norman R. - Assistant Group Leader
Abraham, Bernard - Research Associate
Fried, Sherman - Research Associate

Phipps, Thomas E. - Research Associate
Seifert, Ralph - Research Associate
Westrum, Edgar F. - Research Associate
Sheft, Irving - Research Assistant
Erway, Norman - Glassblower
Thomson, Helen - Technician

Group 6, Basic Wet Chemistry,
Hindman, James C. - Group Leader
Howland, Jerome J. - Research Associate
Morgan, Leon - Research Associate
Ames, Donald [SED] - Research Assistant
Dixon, Jonathan S. - Research Assistant
Florin, Alan E. - Research Assistant
Greenlee, Roy W. - Research Assistant
James, Ralph A. - Research Assistant
La Chapelle, T. J. - Research Assistant
Magnusson, Lawrence - Research Assistant
McLane, Keith - Research Assistant
O'Connor, Paul - Research Assistant
Peterson, Sigfred - Research Assistant
Mokstad, Betty - Technician

Group 7, Recovery,
Stewart, Donald C. [SED] - Group Leader
Anderson, Herbert H. [SED] - Research Associate
Asprey, Larned B. [SED] - Research Assistant
Britain, J. W. [SED] - Research Assistant
Fields, Paul - Research Assistant
Fineman, Phillip [SED] - Research Assistant
Giacchetti, Olga - Technician (half-time)

Group 8, Instruments and Physical Measurements,
Ghiorso, Albert - Group Leader
Jaffey, Arthur H. - Assistant Group Leader
Krueger, Albert C. - Research Associate
Robinson, Herman P. - Research Associate
Beard, Walter - Research Assistant
Crawford, John A. - Research Assistant
Dorsey, John - Research Assistant
Hufford, Duane - Research Assistant
Scott, Benjamin F. - Research Assistant
Walsh, Patricia - Research Assistant
Weissbourd, Bernard [ERC] - Research Assistant
Billington, Hubert - Technician
Towle, Virginia - Technician

Group 9, 23 work,
Leonard I. Katzin - Assistant Section Chief in charge of the following men:
Hagemann, French T. - Research Associate

Hellman, Nison N. - Research Associate
Larson, Raymond G. - Research Associate
Studier, Martin [ERC] - Research Associate
Wolf, Michael - Research Associate
Hyde, Earl - Research Assistant
Van Winkle, Q. - Research Assistant
Pinckard, Marian - Technician

No group assignment:
Thompson, Roy C. - Research Associate
Hausman, Eugene A. [SED] - Research Assistant (half-time)
Brody, Bernard B. - Research Assistant
Malm, John G. - Research Assistant

Service Group:
Florin, Kathleen - Supervisor
Freeman, Elsie May - Laboratory Assistant
Porter, Lillie May - Laboratory Assistant

A recent snapshot of a few members of the section is shown in Figure 1.

I called Perlman at Site W and told him about a Whitaker-Doan-Compton-Stearns meeting, making him available to Chicago. I also mentioned Stearns' phone call to Tilley and Tilley's answer that Stan Thompson will be released soon from his job at Hanford.

We then discussed ways to get another 1-cc sample of Hanford neutron-irradiated uranium solution, this time without H_2SO_4. The two possibilities are (1) dissolver solution although this is not good because of the danger of freezing in the sample tube, or (2) a sample from storage tank No. 4-7 before H_2SO_4 is added. There would be quite a bit of H_2SO_4 left in the tank from the previous run, however, perhaps enough to give a H_2SO_4 concentration of 0.1 N. We could point out we want a minimum heel left. The evidence for high alpha-particle activity is not satisfactory in the dissolver solution. For a high specific activity in plutonium, one of the two methods at Site Y gives a higher value than that at Site W.

According to today's paper Moscow said that the Russians have hoisted the banner of victory over the Reichstag. In the Pacific U.S. troops have captured the Okinawa air field.

Wednesday, May 2, 1945

At 8:30 a.m. I held a meeting in my office of the Council of my section, attended by Albaugh, Cunningham, Davidson, Egan, Ghiorso, Gilbreath, Hindman, Jaffey, Jones, Katzin, Lawroski, Manning, Seaborg, Simpson, Stewart, and R. Thompson. I mentioned Nickson's need for a plutonium complex that would remain dissolved in both alkaline

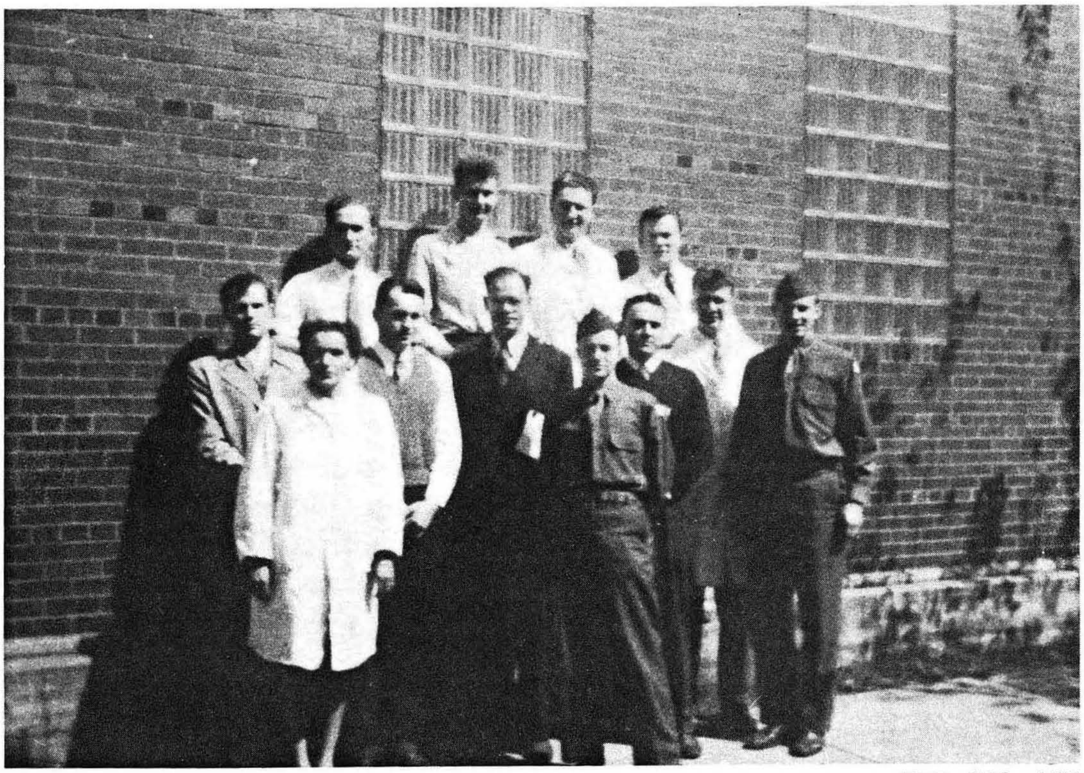

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Figure 1. Outside New Chem, May 1945. (Standing on sidewalk, left to right) Jim Gilbreath, Mildred Summers, Fred Albaugh, Roy Thompson, Milt Ader, Rex Bradt, John Malm, Horace Hopkins. (Standing on ledge, left to right) Tom Morgan, Sig Peterson, Jerome Krinsky, Roy Greenlee.

(intestine alkalinity) and acid (stomach acid of 0.1 N). I suggested the citrate as a possibility. Cunningham proposed the oxalate, and Hindman expressed the opinion that neither citrate nor oxalate would do.

I announced the dissolver solution has arrived; unfortunately H_2SO_4 was added before the sample was taken. It will be used for heavy isotope and solvent extraction work. For the heavy isotope work, one microliter will be put on a counting disc and examined in the pulse analyzer, etc. One-hundred microliters will be treated to get rid of the uranium and to work up the heavy isotopes. I asked what could be done with the other 900 microliters in solvent extraction work, to which Gilbreath replied "almost nothing" (because of the presence of H_2SO_4). We discussed the possibility of getting a sample from the next storage tank before the H_2SO_4 is added (although there will be some H_2SO_4 left in the heel from the previous run).

Albaugh asked about the rumor that Monsanto is taking over Clinton labs. I replied that it seems to be true--the University of Chicago has no interest in running such a big industrial plant. Manning mentioned that Daniels will speak next Wednesday on his "rock" pile. I asked about the status of Abraham's and Sheft's writing; Davidson assured me they will finish by July 1.

In a memo to Daniels about the procurement of items for the construction of the 1-inch columns to be used to test the Redox process, Lawroski mentions that, in order to test the process adequately, a total of five columns will be required, varying in size from 1/2" to 1-1/2" I.D.

I had a conference with Moulton about the following patent cases that seem to have some conflicts: Case-1699 (Seaborg and Wahl) covers a method for electrolytic deposition of plutonium from any liquid and appears to conflict with a similar case from Site Y. Case-1701 (Seaborg and Gofman) covers the electrolytic deposition of plutonium in the oxidized state. Case-1702 (Seaborg and Wahl) covers the electrolytic deposition of neptunium from solutions. Case-781 will probably use the names of La Chapelle and Fields as the inventors and covers the separation of plutonium by adsorption from a TiO_2 gel. Case-781 (Seaborg, S. Thompson, and Davidson) covers the use of various scavenger precipitates in the bismuth phosphate process.

At 7:45 p.m. I attended a meeting in Room 209, Eckhart Hall, of Katzin's group. Others present were Cressman, Cunningham, Daniels, Davidson, Dixon, Egan, Fineman, Ghiorso, Hagemann, Hellman, Hindman, Jaffey, James, Jones, Katzin, Larson, Lawroski, Manning, McLane, Nickson, Peterson, Schaffner, Stewart, Templeton, R. C. Thompson, Paul Tompkins (Clinton), Van Winkle, and others. I opened the meeting and turned it over to Katzin for the presentation of speakers from his group.

Katzin made a few introductory remarks on our recent "mass production" program for U^{233}. In spite of the magnitude of the task involved, the deadline was met and the material delivered for physical measurements. Katzin noted that the determination of eta by the physicists using this material has resulted in conflicting values. The earlier value of 2.5 has not been confirmed, and the present best estimate seems to be 2.35. This is about the same value as was determined in the first rough measurement, and Katzin said that this illustrates again the action of the "Ghiorso principle," to wit, "the value obtained in a preliminary rough measurement will always turn out to be the best value."

Larson then spoke on the recovery of Pa^{231} from "carbonate residues" obtained in the processing of pitchblende ores which, on the basis of 50 percent U_3O_8, should contain about 0.2 ppm of protactinium. He outlined the process used, which so far has yielded about 2 mg of Pa^{231} concentrated upon 20 mg of lanthanum carrier. Treatment with HF of the residues from the nitric acid leach of the alkaline residues should yield additional protactinium. In answer to my question as to how much protactinium the barrel contained on the basis of 0.2 ppm, Larson replied about 20 mg. I also raised some questions on how polonium is allowed for in the analysis for protactinium, the manner in which the Pa^{233} beta-particle tracer is added, and where the polonium and ionium go in the process. We also discussed the ionium content of the ore, that may be as much as 60 ppm. I expressed the thought that an isolation of ionium should certainly be worthwhile.

Daniels asked where the techniques for protactinium isolation were developed. Katzin and I indicated that we developed the procedure entirely here.

Roy Thompson then reviewed the efforts to devise a somewhat more efficient method for the recovery of protactinium from two additional barrels we have obtained. Tompkins from Clinton suggested the use of ion exchange resins (such as IR-1).

Katzin announced that Studier was scheduled to speak also but is ill. He suggested that out of respect to Studier the meeting adjourn on time for a change. Before adjournment Daniels announced that the forthcoming Chemistry Division Monday evening meetings will feature Young on power piles (May 7) and Jeffries on postwar problems (May 14).

Today's paper reports that Hitler has died and that Karl Doenitz has become the new Fuehrer.

Thursday, May 3, 1945

At 8:30 a.m. the Heavy Isotopes Group meeting in my office was attended by Cunningham, Florin, Hagemann, Hindman, Hyde, Jaffey, James, Jones, Katzin, La Chapelle, McLane, Magnusson, Manning, Morgan, O'Connor, Peterson, Scott, Studier, R. C. Thompson, Van Winkle, and Weissbourd. I briefly discussed laboratory space and nomenclature.

We then planned the measurement of the fissionability of the Pu^{240}-Pu^{239} mixture by measuring fissions per alpha and fissions per unit mass of Pu^{239} (with the mass of the Pu^{239} determined by weighing or spectrophotometrically).

Plans for the 1-ml of Site W dissolver solution were described. One microliter of the solution will be mounted on a plate for examination in the pulse analyzer, etc. A second plate will be prepared from 100 microliters treated to get rid of the uranium. The remaining 900 microliters will be used to study the heavy isotopes.

We then discussed the decay of Pu^{236} from the uranium plus deuterons and plutonium plus deuterons targets.

Peterson talked about the carrying of actinium by zirconium iodate. McLane and Peterson will start to dissolve 1/4 gram of actinium concentrate received from Hamaker's lab.

Activities scheduled at this meeting were (a) Specific activity determinations on the 100 gt material will start on Monday, May 7. (b) Jaffey and Hyde will measure the reaction Io(n,γ)UY tomorrow. At the next meeting we will discuss the uranium plus helium ions target and Np^{237} extraction.

I wrote to Vance Cooper at Site W to tell him of the results of the Hanford manpower meeting here on April 16 at which Tilley had indicated that the company wanted to retain Cooper at Hanford for the duration of the war but at the same time was quite non-committal as to the extent of the company's permanent interest in him. I explain to Vance that after the meeting I pointed out to Tilley that it was unfair to force him to stay at Hanford with such indefinite prospects. Then, about a week later, word filtered back that Vance was going to be placed on the list of those who would be given the option of leaving there this summer. I tell him that, in yet a further development, I discussed his case with Squires when he was in town on April 28 and Squires seemed very anxious to keep him.

I say that probably somebody in the company will approach him fairly soon and that I think this background information may be useful to him. According to the procedure which has been set up I am not permitted to approach any of the men from Site X to come here, otherwise I would have asked for him. I add that I understand that the people down South are requesting him so that he will have this as an opportunity.

Manning wrote a memo to Hogness on the status of plans for the hot lab in Room 12. Preliminary plans have been drawn up by Krol and discussed with Nickson and Rose. The preliminary cost estimate is within the $10,000 limitation.

In a memo to Hilberry, Stearns confirmed Whitaker's willingness to release Perlman so that we could add him to the staff of the Met Lab. He proposed that a salary adjustment for Perlman be handled by transferring him immediately to the Met Lab payroll.

Perlman called from Site W. He told me that word has come through that Stan Thompson will be made available to arrive here on June 15; he will leave Hanford on May 27 or so--he may see Follis of Standard Oil in San Francisco. His salary is $425 per month. Perlman mentioned there is nothing new on Vance Cooper. He probably will leave Hanford.

I then asked him to ascertain Kohman's and Goeckermann's salaries. Perlman said he will visit here from noon, Tuesday, May 15, to the following Tuesday night.

I wrote to George Watt at his home in Richland to thank him for his appraisal of Goeckermann. I mention that I have heard that du Pont has an interest in him (Watt) although I know he has expressed interest in going back to Texas.

Wayne Johnson of Personnel phoned to tell me that a letter from Skerry to Compton of April 25 said that Stan Thompson would be available June 30--the question of vacation was not mentioned.

Berlin has fallen to the Russians, and they have captured 70,000 German troops. The Germans have given up Hamburg to the British.

<u>Friday, May 4, 1945</u>

I read a copy of a May 3 memo from Zachariasen to Stearns identifying four certain phases in the system NaF-UF_4: $NaUF_5$, $Na_{4-x}U_xF_{4+3x}$ (x between 1.33 and 1.60), beta Na_2UF_6 and $(Na_{6-x} + U_x)U_2F_{14+3x}$ (x between 0 and 0.3).

I sent Vance Cooper at Site W a copy of a letter from Stearns to Whitaker I just received which I interpret as meaning that he may be back in the good graces of the du Pont Company.

Allison called me from Site Y. He asked whether we can supply them with some Np^{237}. I told him we have 30-40 mg and suggested a 50-50 split. He said they will need theirs around June 1 if they decide they want it. The experiment would be completed in 3-4 weeks. He will let me know their decision. I mentioned the possibility of a higher yield in another W run.

Allison said that Farmer's No. 2 will be opened in approximately one week after some radiation tests. He will send us 25 mg of Pu^{239} and may want us to do some work for them on the remainder. I asked him for a few or up to 25 mg of U^{235} from Farmer's No. 2. He will look into the possibility.

I mentioned our growth of 95^{241} from long-lived Pu^{241}; it was news to Allison although he had heard rumors since Kennedy's return. I then asked him for the rare earth fraction from their purification process for plutonium. He will investigate the feasibility of this also.

I received a letter dated April 29, from Stan Thompson in which he expresses extreme disappointment that the du Pont Company seems reluctant to release him before July. He feels that this attitude is not justified because he is no longer needed at Hanford and he is anxious to return to the Met Lab as soon as possible in order to participate in our new work. He also mentioned that Squires talked earlier in the day (April 29) to Kennedy at Los Alamos. Squires learned that it is very unlikely that the responsibility of purifying plutonium for the nuclear weapons program will be transferred to Hanford.

I immediately dictated a reply to Stan Thompson that many of his questions have already been answered in phone conversations. I tell him that I have asked Hogness to discuss with Stearns the measures which should be taken to obtain an earlier release date. I suggest that he visit Follis to arrange an extension of his leave. I also mention the possibility that the research work he has accomplished may be applicable toward a Ph.D. However, such a program cannot be definitely outlined at this time. I hope the letter will help reassure Stan that we are concerned about his future.

Jaffey and Hyde went to Argonne to measure the cross section of Io(n,γ)UY.

In today's war news thousands of Germans are fleeing to Norway; the British are crossing into Denmark; New Zealand and Yugoslavian troops have taken Trieste; but the U.S. has suffered 16,964 casualties in Okinawa.

<u>Saturday, May 5, 1945</u>

I talked at the afternoon meeting of group leaders in Room 251, Ryerson Laboratory, concluding the discussion of the effect of heavy isotopes on the operation and products of converter and breeder piles, which I had begun at the last Saturday's meeting. I considered two types of piles: (1) those operating on enriched U^{235}, and (2) those operating on U^{233}. With regard to the U^{235} pile, I pointed out that all the reactions occur that were described last week for the plutonium-fueled pile. In the later stages of the reactions, Pu^{238} is produced faster in a U^{235}-U^{238} mixture than in a pile containing Pu^{239} and no U^{238}; assuming that the pile is operated until the Pu^{240} produced is

20 percent of the Pu^{239}, the alpha activity from the Pu^{238} is equal to about 10 percent of the alpha activity of the Pu^{239} and the alpha activity due to Pu^{240} is nearly equal to the alpha activity due to the Pu^{239}. I mentioned that Pu^{238} would also be produced even faster in later stages of operation from the U^{236} formed by the n,γ reaction on U^{235}. The formation of Pu^{236} was also discussed.

In considering the breeder pile operating on U^{233} to produce more U^{233} from thorium, I discussed the production of U^{234} from U^{233} and Pa^{233}. I mentioned the importance of measuring the cross section for the reaction $Pa^{233}(n,\gamma)Pa^{234}$ in order to determine the efficiency loss in <u>eta</u> from the U^{234} decay product of Pa^{234}.

Nickson talked during the second part of the meeting on the health hazards involved in converters and breeders.

I received from T. S. Chapman the spectrographic analysis of the 1.243 g of UO_3 (83.2% 25) that Section C-I received the middle of April. Chapman included information on the method of preparation.

Gore issued an Organization Chart of the scientific divisions of the Met Lab as of May 5, 1945.

<u>Physics</u>
A. J. Dempster, Director
E. P. Wigner, Associate Director

<u>Sections</u>

<u>P-I Instrument</u>
W. P. Jesse, Section Chief

<u>P-IV Crystal Structure</u>
W. H. Zachariasen, Section Chief

<u>P-V Mass Spectroscopy</u>
A. J. Dempster, Section Chief

<u>P-VII Theoretical</u>
Gale Young, Section Chief

<u>P-VIII Properties of Solids</u>
Gale Young, Section Chief

<u>P-IX Engineering Physics</u>
L. A. Ohlinger, Section Chief

<u>P-XIII</u>
L. Szilard, Section Chief

<u>Health</u>
R. S. Stone, Director
L. O. Jacobson, Associate Director

<u>Sections</u>

H-I Clinical Medicine and Medical Research
L. O. Jacobson, Section Chief

H-II Biological Research
K. S. Cole, Section Chief
C. L. Prosser, Associate Section Chief
A. Brues, Assistant Section Chief

H-III Medical Industrial Hazards and Health Physics
J. J. Nickson, Section Chief
J. E. Rose, Associate Section Chief

Advisory Committee
William Bloom
Alexander Brunschwig
W. R. Harrison
Paul Hodges
C. J. Watson
Sewell Wright

Chemistry
T. R. Hogness, Director
J. Franck, Associate Director
F. Daniels, Associate Director

Sections

C-I Separations Studies and Basic Chemistry of the Heavy Elements
G. T. Seaborg, Section Chief
T. O. Jones, Assistant to Section Chief
W. W. Manning, Associate Section Chief
B. B. Cunningham, Assistant Section Chief
L. I. Katzin, Assistant Section Chief
F. W. Albaugh, Assistant Section Chief

C-II Radiation Studies
M. Burton, Section Chief
A. O. Allen, Associate Section Chief

C-III Chemistry of the Fission Products
W. Rubinson, Associate Section Chief

C-IV Analytical Services
J. I. Watters, Section Chief
M. S. Fred, Associate Section Chief

Metallurgy
F. Foote, Acting Director

Sections

M-I Metallurgy
F. Foote, Section Chief

M-II Corrosion
E. W. Brugmann

M-III Fabrication
J. H. Chapin, Section Chief

Technology
J. C. Stearns, Director

T-IV Optics
G. S. Monk, Section Chief
W. H. McCorkle, Associate Section Chief

"War Races Toward an End" says today's paper, referring to the European front.

Sunday, May 6, 1945

I played 27 holes of golf at Evergreen with French Hagemann, Jerry Howland, and Al Ghiorso. (Ghiorso and I won in a "low ball - low total" match, 9 and 6. FH-118, JH-111, GS-103, AG-116.) In a nine-hole match Al and Jerry won, 1 up (AG-58, JH-57, GS-56, FH-59).

Monday, May 7, 1945

Cunningham sent Koch a list of the precautions to be observed in handling the 10-milligram sample of plated plutonium which Koch is to use for measurements at the University of Illinois.

Perlman called me from Site W with the following information: (1) There is now confusion as to whether the June 15 date for Stan Thompson includes his vacation. Stan has phoned Follis for an appointment. (2) Kohman's salary is $325-$350 a month; Goeckermann's is $240 a month. (3) Stearns has requested that people at Hanford get no salary raise if they are to be transferred back here or to Site X--however, those people would prefer a raise even if it were later taken away. (4) Perlman wants me to make hotel reservations for him for May 15-22.

Today's headline reads "Expect War's End!" again referring to the European front. In the Pacific U.S. troops captured Tarakan, city and air base on Tarakan Island, just off Borneo.

Tuesday, May 8, 1945

Today is V-E Day. Germany surrendered unconditionally today at 2:41 a.m. French time in the red Reims schoolhouse--General Eisenhower's headquarters.

I received another letter from Stan Thompson confirming his plans. He will terminate at Hanford on May 24, leave for San Francisco on the 25, spend two days there, and arrive in Chicago about June 1. He would like me to have someone of authority write to Follis about the extension of his leave of absence, before he (Stan) arrives in San Francisco. He believes such a letter will carry weight. Stan also requests that we keep our eyes open for housing for them near the university.

I read a copy of a May 7 memo from Stearns to Hogness giving the information that the District Engineer has approved construction of the additional solvent extraction columns within the limits of $5,000 to $7,000. Stearns took the occasion to thank Hogness for the many jobs which have been and will be well done by the Chemistry Division.

Manning transmitted performance review reports for Ruth Rogers and Dan Dorcy to Gibson. He recommended Mrs. Rogers for a raise of $2.00 per 40-hour week. She has recently been given the responsibility of supervising all of the secretarial and stenographic work in Section C-I as a replacement for Mrs. Albaugh who is leaving this month. She is now acting as my secretary.

Dorcy's work continues to be good, but he is already at the top of his salary classification so no raise can be recommended.

Report CN-2887, "Chemical Research - Separation Processes for Plutonium (Seaborg, Section Chief; Manning, Associate Section Chief; Albaugh, Assistant Section Chief)," Report for Month of April 1945 was issued. The following investigations are reported:

Extraction-Decontamination (R. Thompson, Group Leader) Decontamination with respect to neptunium in the Bismuth Phosphate Process. Peterson and Malm have shown that decontamination factors for neptunium are 259 when the present flowsheet is followed and 1.1×10^4 when $(NH_4)_2SiF_6$ is omitted from the plutonium precipitation steps of both bismuth phosphate cycles. Methods for recovery of neptunium in the Bismuth Phosphate Process. Malm and Peterson, using the extraction supernatant from the above runs, find that 85 percent of the total neptunium can be recovered with slight changes in the extraction and bismuth phosphate precipitation steps. Decontamination studies (conversion pile). Hopkins has studied decontamination of directly separated plutonium compounds by washing $Pu(OH)_4$ with different complexing agents and by batch type solvent extraction using TFA and hexone.

Process Development (Gilbreath, Group Leader) Use of TFA for analysis of Pu(III,IV,VI). Winner has found that TFA extracts less than 2 percent of Pu(VI) from dissolver solution. TFA cannot be used for analyses of Pu(III,IV) in bismuth phosphate extraction supernatants because extraction of Pu(IV) is only 20 percent complete. Solvent extraction and decontamination-Redox Process. Blaedel, Walling, Post, and Sedlet have shown that Pu(VI) is quite soluble in hexone, whereas Pu(IV) is only moderately soluble. The uranium in the hexone effluent from the second column is decontaminated with respect to beta-particle and gamma-ray activity by a factor of 10^3 to 10^4. Successive batch extractions to simulate scrubber section in the oxidation and reduction columns have yielded minimum decontamination factors of 10^6. Waste hexone can be completely decontaminated and purified by steam distillation.

Solvent Extraction (Lawroski, Group Leader) Hexone extraction-engineering development. Egan, Schaffner, Hausman, Schraidt, Simon, and Struminski have tested the reassembled and modified 3-inch I.D. column with hexone water-nitric acid systems. The apparatus has been readied for preliminary testing of the Redox Process with inactive UNH solutions.

Bismuth phosphate extraction-solvent extraction process. Hyman, Ader, Brody, Kelley, and Reinhardt have found the distribution ratio for ruthenium between hexone and aqueous acid solutions to be about 80 (aqueous/hexone) for the reduced form and was as low as 5 to 10 for the oxidized form. The presence of hydrazine in scrub solutions containing aluminum nitrate as salting agent increases the extraction of plutonium by hexone, but the effect is not noticed when NH_4NO_3 is the salting agent. Fluoride ion has been shown to improve the distribution coefficient of zirconium. Multi-stage laboratory batch extractions of large volumes of active bismuth phosphate feed solutions indicate that a decontamination factor of at least 10^7 could be achieved in countercurrent extraction columns.

Report CN-2888, "Chemical Research - Basic Chemistry of Plutonium (Seaborg, Section Chief; Manning, Associate Section Chief; Cunningham, Assistant Section Chief)," Report for Month of April 1945, was issued today. The following investigations are reported.

Basic Dry Chemistry Group (Davidson, Assistant Group Leader). Equilibrium in the vapor phase hydrolysis of $PuBr_3$. Sheft and Davidson have measured equilibrium constants in the temperature range 540-640°C for the reaction $PuOBr + 2\ HBr \rightarrow PuBr_3 + H_2O$. The data were used to derive the linear free energy equation $\Delta F_{kcal} = (22.8 \pm 0.4) + (33.2 \pm 0.4)\ T/1000$. By analogy with other substances, ΔC_p for the reaction has been estimated as +2.4 calories degree^{-1} mole^{-1}.

Basic Wet Chemistry Group (Hindman, Group Leader) Absorption spectra of uranium(III) and uranium(IV) in 1.0 M HCl have been determined by Howland with the Beckman quartz spectrophotometer. Formal potential of the U(III) - U(IV) couple in 1.0 M HCl has been determined by Howland and Magnusson to be +0.633 ± 0.02 v.

Recovery Group (Stewart, Group Leader) Distribution Coefficients for plutonium in various oxidation states have been determined by Stewart, Asprey, Britain, and Studier. Preparation of new plutonium compounds. Anderson has prepared the following compounds, the formulae of which were established by Zachariasen: Rb_2PuF_6, K_2PuF_6, and Na_2PuF_6. As for the compound $Pu(SO_4)_2 \cdot 4H_2O$, its formula has been established by chemical analysis. Final concentration of plutonium. Fineman investigated the suitability of hydroxide precipitation for final concentration of plutonium.

Wednesday, May 9, 1945

At 8:30 a.m. our Council met in my office. Present were Albaugh, Cunningham, Davidson, Egan, Ghiorso, Gilbreath, Hindman, Jaffey, Jones, Katzin, Lawroski, Manning, Simpson, Stewart, and R. C. Thompson. I opened the meeting by reading a list of reports not yet returned. I then relayed a suggestion that performance appraisals of all men be made by the group leaders at this time for permanent file records. Manning, Cunningham, Albaugh, and Katzin then reviewed the status of writing on the Project Record Volumes.

I next brought up the question of section meetings after July 1, suggesting that we have only one meeting on the order of the Thursday morning meetings (of the Heavy Isotopes Group) where the work for the week is planned; probably a two-hour meeting would be necessary. I indicated I would like notes taken and typed immediately--the note-taking would be spread among three or four men.

I asked whether the meeting should be held in the morning or at night. Ghiorso said that he thought a morning meeting would be necessary since it would be impossible to do anything like that after a hard day's work. I explained that I did not want him to go soft. I told the men that I wanted them to think about the general question of meetings after July 1 and give me their opinion.

The meeting concluded with a discussion of O'Donnell's plan to clean out the present shop in this building of all things like lockers, unnecessary tables, etc., and install a ten-man machine shop, fully equipped, to be devoted only to chemistry orders and not subject to seizure by high priority demands. I indicated that I think this is a step in the right direction for getting our work done seeing that jobs have been held up at Ryerson so many times.

I received a May 5 memo from Blaedel and Gilbreath in which they spectulate on the possibility that butyl phosphate might be a tool for the extraction and decontamination of plutonium from irradiated uranium and for the decontamination of 95 and 96 by a continuous liquid-liquid extraction method. The affinity of butyl phosphate for Pu(IV) is well known; whether this affinity extends to trivalent elements could be established by simple experiments. Separation of rare earths by this method was also suggested.

I wrote a long letter to Hamilton in Berkeley listing the following reasons why I think it is important to continue to use the recently rebuilt Berkeley 60-inch cyclotron for investigations in heavy isotopes: (1) The cyclotron offers a good method of making Pu^{240} of high purity (needed for fission measurements) by the reaction $U^{238}(\alpha,2n)Pu^{240}$. (2) The bombardment of uranium with high energy helium ions will also produce Pu^{241} in a much higher proportion compared with Pu^{239} than is the case for plutonium produced in a pile, permitting the study of its radiation properties. (3) The bombardment of Pu^{239} with high energy helium ions will produce 96^{242}, whose study is important because it probably is produced in the present chain-reacting structure. (4) The bombardment of U^{233}, U^{234}, and U^{235} with helium ions or deuterons should lead to relatively short-lived isotopes of $Pu^{(234,235,236)}$, useful in biological experimentation and in exploratory experiments on separation methods. (5) The bombardment of U^{233} with high energy deuterons or helium ions might produce a neptunium isotope (233,234,235,236) of intermediate half-life that would be a useful tracer. (6) The cyclotron offers a most convenient method for preparation of the 50-year Pu^{238} which has a unique importance in tracer experiments. (7) The cyclotron offers a convenient method for the preparation of U^{232} which has the highest specific alpha-activity of any isotope of uranium known at present. Bombardment of Pa^{231} and Io^{230} by high energy deuterons should lead to the formation of U^{231} and U^{230} which might be useful short-lived isotopes. (8) Some of the isotopes which may be produced by the high energy helium ion or deuteron bombardment of heavy isotopes may possess the property of undergoing fission at a very high rate, giving rise to the opportunity to study the spontaneous fission process in a convenient manner. (9) The general investigation of the whole region of atomic numbers 89 to 96 is important from the standpoint of increasing our general knowledge of the radioactive and nuclear properties of the important isotopes in this region. Such investigations could lead to the discovery of new fissionable nuclei of importance. (10) The production of a number of isotopes in the heavy element region, particularly isotopes of elements 95 and 96 and higher, will lead to increased general knowledge of the chemical properties of elements in this region. It is important to prepare element 97 in order to study its chemical properties. This could best be accomplished with the Berkeley cyclotron using heavy projectiles such as quadruply-charged Be^9 ions. (11) An understanding of the chemical properties of elements 95 and 96 and above will make it possible to continue a search for isotopes of these elements in nature. Some may be important from the standpoint of atomic power, and the possibility of their existence in nature leading to a relatively available source should not be ignored.

I wrote a short note to Stan Thompson in reply to his letter of May 4. I tell him that I asked Hogness to have Stearns call Tilley. He did, and we learned that Stan will be released from Hanford about June 1. I say that the argument which was advanced to obtain this release date is that he (Stan) will then be able to tell Follis he is on the University of Chicago payroll. Therefore it will be safer and less complicated for him to start on Chicago payroll June 1 and then take two weeks vacation later in the summer.

Report CS-2924, "Chemistry Division Summary Report for April 1945," was issued today. All Section C-I information in this report appears in the summaries of work prepared by Katzin (April 16), Albaugh (April 16), and Cunningham (April 17) for my use at the Project Council Information Meeting on Chemistry on April 18.

In the evening, Daniels spoke on his ideas for a "rock" pile.

Today's paper sums up the cost of the European war against Germany, including over three-quarters of a million casualties--some 150,000 of them dead--and 275 billions of dollars.

Thursday, May 10, 1945

At 8:30 a.m. I held a meeting in my office of the Heavy Isotopes Group, attended by Cunningham, Florin, Ghiorso, Hagemann, Hindman, Jaffey, James, Jones, Katzin, Magnusson, Manning, Morgan, McLane, O'Connor, Peterson, Scott, Studier, R. C. Thompson, Van Winkle, and Weissbourd. I outlined the following operations for Farmer's No. 2 Pu^{239} sample: O'Connor will dissolve it, give aliquots to Ghiorso and Weissbourd, separate the plutonium by ether-extraction and give the H_2O phase to James and Morgan. Plates will be made for Ghiorso and Weissbourd from the ether-phase (plutonium).

I said that Britain will dissolve Farmer's No. 2 U^{235} sample, give aliquots to Ghiorso and Weissbourd for preparation by Scott, run through an ether extraction with the bulk of the solution, give part of the ether phase (uranium) to Jaffey to measure the U^{237} and part to Ghiorso and Weissbourd who will look for U^{236}. All these operations will be done in a manner to recover the Np^{237}. The water phase will be held until somebody gets an idea.

Studier reported finding 9×10^6 gamma-rays associated with 1.6×10^6 alpha particles of U^{235}. These gamma-rays may be associated with U^{234}--according to Y-12 values, there are 3.04×10^7 alpha particles per minute from U^{234}. He found no evidence for the earlier reported 1.5 Mev gamma from Pa^{233}--only 330 kev.

Magnusson discussed the extraction of Np^{237} from the Site W solution.

Ghiorso told about his results from the uranium + helium ions target (bombardment 5a). In the plutonium fraction, he measured the relative abundance of the three alpha energies in each of the four layers machined from the target with these results:

(a) 4.3 cm -- 1/1.2/5.5/7.6; (b) 4.1 cm -- 1/1.6/2.9/5.1;
(c) 3.7 cm -- 1/0.9/2.5/2.5

The specific activity measurement on 100 gt plutonium was scheduled for completion by next Thursday.

Stearns sent a request to Compton for four grams each of 100 gt plutonium and 200 gt plutonium. He mentioned that I wanted to hold the present low gt material for further study, inasmuch as such material will not be available in the future.

According to today's paper American troops will start coming home from Europe at a rate of 270,000 to 500,000 a month, but it will take about a year to reduce our forces of 3½ million to the 400,000 garrison force which will remain.

Friday, May 11, 1945

I received a letter dated May 9 from Vance Cooper at Site W, thanking me for my letters about his status. After listening to rumors and guessing, he had just queried Squires who indicated he would be a free agent as of July 1. Vance asked for my opinion of the situation at Clinton Labs, particularly about the possibility of continuous employment. In preparation for a possible long trip, he has been overhauling his car.

I attended a meeting in Hogness' office with Hogness, Daniels, Allen, Watters, Rubinson, and Thetford. We went over the laboratory space situation. Analytical chemistry and the Health Division will take over our released rooms. The rest of Chemistry will compress into the section of the building north of C corridor. A shop for the use of Division scientists will be in D corridor. The glass-blowing shop will stay where it is and we will keep three of the five glass blowers.

The inventory was discussed--everyone is to take a day off to do it, preferably before June 1.

I learned that Thetford will take over L. B. Arnold's responsibilities plus some of Daniels' former duties. Daniels will be Chief of Section II with Willard as his Associate Section Chief and leader of the high temperature pile work.

We were asked to notify all people (especially technicians, lab assistants, and secretaries) who are to leave on July 1 as the termination date.

Saturday, May 12, 1945

Edrey Smith Albaugh terminated work at the Met Lab. She has been my secretary since November, 1942. This is also the last day of service for Walter C. Beard. He is transferring to K-25 in Oak Ridge.

George Watt wrote to thank me for my letter of May 3. He discussed a number of other men who are at Hanford and their plans for the future--including some in whom du Pont has expressed "permanent interest." He, although "approached" several times, plans to return to Austin July 1. Watt also expanded on the information I had heard about the mental illness of one of his colleagues at Austin.

I received and read a draft of a very forceful letter Compton wrote about Stan Thompson to R. G. Follis of Standard Oil, requesting an additional six-months leave of absence. Compton said, "In fact, his work has provided the basis for design and construction of a chemical plant of great magnitude. Mr. Thompson's experience with us has made him proficient in an important new field of chemical research, ..."

Since April 26, James and Florin have been processing sample TαC (uranium plus 40 Mev helium ions Berkeley bombardment of March 28 to April 15) to isolate the plutonium and neptunium fractions. Today, the neptunium fractions (four layers of the target were processed separately) were turned over to Florin. James is continuing to decontaminate the plutonium fractions.

Today's paper reports that Stalin sent a note to Churchill declaring that Churchill has not lived up to the Yalta agreement and that the Soviets could not coöperate with Britain and the U.S. as a result.

Sunday, May 13, 1945

I played 18 holes of golf at Hickory Hills Golf Club (8201 West 95th Street, Palos Park) with Jerry Howland, Luther Arnold, and Steve Lawroski. Jerry and Lu won a "low ball - low total" match, 5 and 2. Scores were JH-116, LA-103, GS-105, and SL-112.

Monday, May 14, 1945

Kohman sent me a memo dated May 11 with some suggestions about the adoption of a system for designating the amounts of pile irradiation received by pile samples. He made several proposals. He suggests that

$$gt = \frac{\text{grams}}{\text{metric ton}} = \text{ppm} \approx \frac{\text{megawatt days}}{\text{short ton}} .$$

Kohman made several other suggestions about terminology. I think these suggestions should be discussed at a Section C-I Council meeting.

I read a copy of a memo dated May 12 from Hogness to Wayne Johnson asking him to make a formal offer to Willard for the position of Associate Section Chief of Section II in the Chemistry Division. The offer is to be for a period of six months beginning July 1, with a salary of $6,000 per year.

Katzin sent me a summary of the work of Group 9, Section C-I for my use at the Project Council Chemistry Information Meeting of May 16, 1945. It covers the following topics: (1) The neutron capture cross section of ionium by the reaction $Th^{230}(n,\gamma)Th^{231}$ was determined by irradiation of a sample of ionium (prepared at Berkeley) in the thermal column of the Argonne pile. (2) A redetermination of the radiations from Pa^{233} disproves the existence of a gamma-ray of 1.5 Mev making up as much as 2 percent of the radiation. Present evidence is that there is a maximum of 0.1 percent of any such hard radiation. (3) Measurements on a sample of 85 percent U^{235} disclosed a gamma-ray of 170 kev energy whose abundance is very high and most likely is due to the U^{234} present at 0.5 percent by weight. (4) A new procedure has been worked out for the recovery of Pa^{231} from uranium residues (using the precipitate of the carbonate neutralization step). The first step is treatment with strong caustic to remove the soluble silica which previously caused trouble.

Albaugh sent me a summary (dated May 14) of the work of Subsection 1, Section C-I, for my use at the Project Council Chemistry Information Meeting on Wednesday. The topics covered are (1) Three successful batch-extraction demonstration runs have been completed using the recommended conditions for the Redox Process. (2) Tracer experiments indicate that the Redox Process is probably capable of eliminating zirconium, ruthenium, and cerium to the required extent. (3) There were two unsuccessful first cycle blank runs (no plutonium, no FPE) of the Redox Process in the 3-inch I.D. columns because of lack of stability of the hydroquinone reducing agent. Two new reducing agents, ferrous-urea and ferrous-hydroxylamine, have since been found which appear very promising. (4) The key to prolonged maintenance of Fe(II) in the reduction column of the first cycle appears to lie in maintaining the nitric acid concentration below some as yet undetermined value. (5) Work on the construction of the 1-inch diameter columns for testing the Redox Process will be started in the very near future. The presently available equipment is not suitable as it is somewhat large as well as unshielded.

Cunningham's summary of the work of Sub-section 2, Section C-I covers the following topics (1) The presence of heavy isotopes is being investigated in about 1 ml of Hanford dissolver solution containing plutonium at the level of 100-125 gt. Preliminary indications are that there are no alpha-emitting isotopes other than those of uranium in this solution that contribute as much as 2 percent of the activity of the 3.7 cm alpha particles. There appear to be present in small amounts alpha-particle activities similar in properties to those of 95^{241} and 96^{242}. The expected 4.1 cm alpha particles of Pu^{238} have not yet actually been measured. (2) Accurate comparative measurements have been made of the specific activity of plutonium produced at various gt levels. Using the results of a mass spectrographic analyses of the Pu^{239}:Pu^{240} ratio determined for one of these samples and the difference in specific activity between this sample and that of one containing a negligible amount of Pu^{240}, they have calculated the specific activity and half-life of Pu^{240} to be 293,500 c/m/microgram and 6,000 years respectively. (3) The n,γ cross section of Th^{230} has been calculated as 50 ± 25 b as a preliminary figure (work also reported by Katzin). (4) A metallic substance, probably neptunium metal, has been produced by Fried by reduction of 50 micrograms of NpF_3 with barium at 1,250°C. (5) The following plutonium sulfates have been prepared and identified by chemical analysis: $Pu(SO_4)_2 \cdot 4H_2O$, $Pu_2(SO_4)_3(OH)_2 \cdot 7H_2O$ or $Pu_2O(SO_4)_3 \cdot 8H_2O$, $K_4Pu(SO_4)_4 \cdot H_2O$(?), $(NH_4)_4Pu(SO_4)_4 \cdot H_2O$, $Pu_2(SO_4)_3 \cdot 7\text{-}8H_2O$. (6) Solubilities were measured of salts produced by precipitation of Pu(VI) with KOH, NaOH, and LiOH. (7) As practice for the possible preparation of a 200 mg sample of U^{233} as metal, centrifugal bomb reductions of UF_4 by lithium were carried out. (8) The most stable allotropic modification of UO_3 was studied. (9) Chemistry of actinium: The Haissinsky method of separating $MsTh_{II}$ has been further improved by the substitution of isopropyl alcohol for ethyl alcohol. Experimental results on the carrying of trace concentrations of actinium, using $MsTh_{II}$ as tracer, are summarized.

At 7:45 p.m. there was a Chemistry Division Seminar in Room 251, Ryerson Laboratory at which Zay Jeffries spoke on postwar problems.

More than 500 superforts dumped 3500 tons of fire bombs on the industrial section of Nagoya, Japan's third largest city, today. Churchill gave the U.S. power the credit for winning on the western front, according to today's paper.

Tuesday, May 15, 1945

Bernard Brody terminated work at the Met Lab to go to the Mound Laboratory in Dayton. He joined my section on June 15, 1943.

The rare earth fraction from 160 grams of Hanford-bombarded plutonium was received from Site Y today. Their identification number is 1945-5-5. The sample consists of 1-2 g of moist solid material and is presumably the evaporated-down water layer from ether extraction.

At 8:30 a.m. Manning and I attended the Project Council Physics Information Meeting in Room 209, Eckhart Hall. Items of interest reported were (1) Zachariasen discussed Burton's sample of graphite which had twice the exposure of the sample reported last month and showed twice the change in spacing and also a distinct intensity change. He also reported that the compound reported as $K_2PuF_6 \cdot xH_2O$ has now been found to be $KPuF_5$, and that the black fluoride of uranium has been found to be really U_2F_9 instead of another form UF_4. (2) Lewis discussed mass spectrometric measurements of the Ba-La^{140} mixture which show normal Ba^{135} and Ba^{138} isotopes and also lines at 152, 154, 158, and 162. (3) Dempster confirmed Spedding's results as to rare earth separation in an IR-1 adsorption column. (4) Spedding said he has used U^{232}-U^{238} mixtures and has obtained a 15 percent separation in one pass. (I remarked that he was probably separating uranium from the U^{232} descendants which are alpha-emitters and present in rather large amounts.) (5) Maurer discussed the heat transfer problem in the breeder and converter piles. (6) Soodak reported on considerations about the breeder piles and discussed the possibility of using very high energy neutrons to take advantage of the increase in <u>eta</u>. (7) Estermann reported on a theoretical calculation of the Wigner effect, calculating the number of displaced atoms. (8) Borst of Clinton reported on some results of analysis of U^{234} in the neutron spectrometer. He also talked about the photographic emulsion method for determining the fissionability of Pu^{240}. Trouble has been found because of n,p reactions on the nitrogen present in the emulsion. (9) Wigner talked about the oscillating danger coefficient method that avoids errors due to slow drift. (10) Zinn discussed Langsdorf's method for measuring <u>eta</u> by its swing effect. (11) Dancoff told us about the experimental work of Goett and Wattenberg on exponential pile measurements on a D_2O pile. A mechanism for rapidly transmitting a sample from near the pile to outside the pile shield (called the rabbit) has been used to bombard the uranium and measure the delayed neutrons within a few tenths of a second after the end of the bombardment. (13) Langsdorf discussed an unsuccessful search, using the D_2O pile, for mesotrons like those found at the University of Illinois.

James completed decontaminating the plutonium fractions from sample TαC (uranium plus 40 Mev helium ions Berkeley bombardment of March 28 to April 15). He mounted the sample and took absorption curves to identify the plutonium isotopes present.

I received a carbon of the letter dated May 14 that Compton wrote to Follis about Stan Thompson. It is identical to the draft I was shown on Saturday.

I read a copy of a letter from Wayne Johnson to Stan Thompson about Stan's return to our laboratory on or near June 1. He mentioned that, in spite of the district rule which states that a person being reemployed by the laboratory must be returned at the same salary he was receiving when he left, we are proposing the salary he is now receiving from du Pont, $5,100, although there is a possibility that the figure may be rejected by the Area Engineer.

Perlman arrived at noon on a visit from Site W. He will be here until May 22.

A representative of the Security Division visited me to discuss personnel security matters. He asked a number of questions about Al Ghiorso, including one I considered improper, that is, was Ghiorso an ardent supporter of President Roosevelt during the last Presidential campaign. I reported the incident to Daniels who agreed that questions regarding the political views of the scientists were improper and said he would take the matter up with Stearns and Colonel Lansdale.

U.S. troops are still fighting on the edge of Naha, capital of Okinawa, where the Japanese are resisting fiercely, according to today's paper.

Wednesday, May 16, 1945

At 8:30 a.m. in Room 209, Eckhart Hall I attended the Project Council Information Meeting on Chemistry. Others present were Allen, Arnold, Borst, Brown, Burton, Captain Chapman, Cohn, Compton, Connick, Coryell, Creutz, Daniels, Dempster, Doan, English, Estermann, Franck, Fred, Hamilton, Hilberry, Hogness, Huffman, Johnson, Jones, Leverett, Manning, Metcalf, Mulliken, Perlman, Pittman, Rabinowitch, Rubinson, Rundle, Spedding, Stearns, Warner, Watson, Watters, Way, Whitaker, Wigner, Young, Zachariasen, and Zinn. I was first on the program and reported on separations process work and basic chemistry work as summarized in May 14 memos from Albaugh, Katzin, and Cunningham. In discussing the heavy isotopes work on the 1 ml of Hanford dissolver solution, I presented a pulse analyzer record of a 1 microliter aliquot of the solution, pointing out that the three bands (U^{238}, U^{234}, and Pu^{240} + Pu^{239}) account for more than 99 percent of all the alpha pulses. I also offered a curve of the uranium fraction that shows no evidence of U^{236}.

I described a third experiment in which repeated LaF_3 precipitations were carried out from the whole available oxidized solution until the alpha activity became constant, indicating all plutonium was removed. Alpha pulse analysis showed a few pulses that correspond to ranges definitely greater than 4.1 cm, but the beta activity is too strong for successful identification of the 4.05 cm and 4.75 cm range alpha particles from 95^{241} and 96^{242} respectively.

I then reported on our newest value for the half-life of Pu^{240} (6,000-7,000 years), on Jaffey's and Hyde's measurement of the absorption cross section of ionium and on the investigation of the gamma-radiation of enriched uranium.

I also discussed in some detail our U^{233} work, recalling that about six months ago a decay scheme was suggested which included a gaseous emanation of mass number 221 (after the first alpha emitters). I explained that we have returned to the study after interruptions caused by other work and have now found evidence that Ra^{225} emits beta rays (rather than alpha rays as previously postulated); the following chain of intermediates is now suggested between Ra^{225} and Bi^{213}:

$$_{88}Ra^{225} \xrightarrow[26\ d]{\beta} {}_{89}Ac^{225} \xrightarrow[10\ d]{\alpha} 87^{221(?)} \xrightarrow[5\ m]{\alpha} 85^{217(?)} \xrightarrow[<1\ m]{\alpha} {}_{83}Bi^{213}$$

I mentioned that the identification of elements 87 and 85 in this decay series is made somewhat uncertain by the fact that the 5-minute alpha activity can be volatilized from the LaF_3-covered counting plate by heating to red heat (this helped to mislead us earlier into believing that a gaseous emanation was formed).

Items of interest reported by other speakers were (1) Pittman from Site Y discussed recovery work there. He pointed out that the use of peroxide is very helpful in separating grams of plutonium from kilograms of other substances--they have tried practically all the elements and none precipitates under their conditions like plutonium except for uranium. He gave some results of solubility behavior of plutonium and its compounds. The metal dissolves readily in HCl but poorly in HNO_3. The oxide dissolves in HI without liberation of iodine. The most difficult thing to dissolve is a peculiar green precipitate that seems to crop up in unexpected places. Nobody knows what it is. (2) Rundle of Ames reported on the structures of a number of uranium and thorium compounds determined by x-ray diffraction. (3) Connick discussed the formula for plutonium peroxide. (4) Connick described continued work at Berkeley with TFA in which a complete extraction and decontamination process resulted in a decontamination of 10^5 for soft gamma-rays and 10^6 for beta particles. (5) Leverett discussed the uranium recovery work which has not been too encouraging. He also reported that the plant at Clinton for Ba^{140} separation is complete and the equipment is being tested. (6) English of Clinton discussed the determination of Pu(V) and Pu(VI) in dissolved pile uranium. (7) Coryell discussed the hot laboratory operations at Clinton and the problems they are having with repairs. The last run for Ba^{140} separation is now in process, and Leverett's crew will take over the job. Coryell also described some other new data his group has obtained on the activities of neodymium, element 61, and samarium. The group has verified, by the ion exchange adsorption elution method, the existence of an approximately 2-year isotope of element 61.

Presently, they think it probably has a mass number assignment of 149. Coryell suggests that 161^{147} is probably long-lived. (8) Work has continued in Davies' group at Clinton in studying the oxidation of atoms produced by radioactive processes. (9) Metcalf reported on the preparation of active antimony by a Szilard-Chalmers reaction on SbF_5.

James began isolating element 95 from the rare earth fraction (from the 160 g plutonium) received from Site Y yesterday (Y number 1945-5-5; Met Lab number 51C). He has determined that the sample contains between 35 and 40 mg of plutonium. I received a letter from Joe Kennedy today describing Site Y's treatment of this sample--I immediately routed the letter to James and Morgan.

I received a carbon of a May 12 memo from Allison to Stearns indicating that half of the Np^{237} will probably be returned to us by August 1 as I requested.

Frank Pittman from Site Y and I met to discuss the recovery of plutonium; others present were Asprey, Cunningham, Hindman, Jones, Perlman, Schubert, and Stewart. Perlman reported on the techniques developed at Hanford to eliminate iron from the peroxide precipitate. Pittman reported, as he did at the Chemistry Information Meeting today on the extensive study the Recovery Group at Site Y has been making of the use of peroxide precipitation in recovery work. Pittman then asked Schubert if he could recommend an adsorption method for recovering product from large volumes of solution containing considerable iron. Schubert suggested that IR-1 adsorption columns (40-60 mesh size resin) be used with an HCl solution, if possible.

At 2:00 p.m. Morgan, Ghiorso, Jaffey, and Simpson attended a meeting of the Committee for the Instrument Volume of the Project Record in Room 251, Ryerson Laboratory. Others present were Aebersold, Borkowski, Borst, Collins, English, Jesse, Mulliken, Parker, Rose, Shonka, Stephenson, and Zirkle.

At 7:45 p.m. I attended a meeting in Room 209, Eckhart Hall, of the Basic Chemistry, Recovery, and Instrument groups of my section. Others present were Abraham, Asprey, Connick, Cressman, Cunningham, Daniels, Dixon, Fineman, Florin, Ghiorso, Gilbreath, Hagemann, Hellman, Hindman, Howland, Hufford, Hyman, Jaffey, James, Jones, Katzin, Kelley, Lawroski, Manning, McLane, Morgan, Nickson, Perlman, Peterson, Phipps, Reinhardt, Schaffner, Seifert, Sheft, Simpson, Stewart, Studier, R. C. Thompson, Van Winkle, and others. I introduced Nickson who stated the tolerance value for beta and gamma-radiation remains unchanged at 100 mr per day. He then described the conditions expected in studies made on those who have received over-exposure to beta and gamma-radiation. These include in order of occurence proceeding to more serious results: erythema, pigmentation, atrophy, blood vessel changes (decrease in number of vessels and/or telangiectasia), hypertrophy, ulceration, cancer. He gave some more recent data on tolerance levels of plutonium that indicate a limit of 0.7 microgram fixed in the body. To detect such a quantity, 24-hour urine samples will be required. Extreme precautions, such as remaining at home during the collection period, will be necessary to avoid contamination. I asked whether the worker could go out and play golf for that day, and Nickson said it could be done.

After Nickson's talk, I asked Asprey to describe his solubility measurements on compounds of Pu(III) and Pu(VI). He first gave the results for Pu(VI) compounds formed with lithium, potassium, and sodium hydroxides. He also reported on the considerable amount of work done on the solubility and composition of crystalline Pu(III) sulfate.

I next introduced Jaffey who gave the results of a neutron bombardment of ionium. The sample, which was 25 percent Io^{230} and 75 percent Th^{232}, was bombarded alongside a sample of ordinary thorium and two gold monitors. The cross section of ionium was calculated to be 88 barns. I suggested the possibility of measuring the ratio of U^{233} to Pa^{231} formed in bombarding such a sample, saying it would be desirable to put a milligram of this ionium-thorium mixture in the Clinton pile for an extended bombardment.

According to today's paper, with fierce fighting still raging near Davao--a city on Mindanao Island--General MacArthur says the island is 90 percent liberated.

The Project Council Policy Meeting held at at 2:05 p.m. in Room 209, Eckhart Hall was attended by Captain T. S. Chapman, Cohn, C. M. Cooper, Compton, Daniels, Dempster, Doan, Franck, Greninger, Hamilton, W. B. Harrell, Hilberry, Hogness, Howe, J. R. Huffman, Jacobson, Johnson, Leverett, McKinley, Major E. J. Murphy, Nordheim, Spedding, Stearns, Stone, Szilard, Tracy, Warner, C. J. Watson, W. W. Watson, J. E. Wirth, and Zinn. Compton recalled some conversations he had in Washington with Groves, Nichols, and Bush with regard to trends in the future. Concern for postwar development of this project is growing more and more real, and the intention obviously is to go ahead. Compton added that when Groves passed this information on to him it was with the intention that it be given to the Council but should go no further. Progress is being made in appointing the new policy committee although the death of the President and recent war developments have interfered. Compton explained that the committee would be responsible to the President and would be made up exclusively of civilians; reporting to the committee would be several panels made up of Army, Navy, scientific men, etc.

Compton said that (1) the Chicago contract has definitely been extended to June 30, 1946, (2) there appear to be no obstacles to the transfer of the Clinton Laboratories to Monsanto, and (3) the budget for the Argonne Laboratory was included in the Met Lab budget and the groups there will probably be made into a single unit.

There was an inconclusive discussion of a possible dividing line between the type of pile investigations to be conducted at Chicago and at Clinton.

Various plans for future policy and information meetings were considered, and Compton decided there would be a policy meeting next month but no general information meetings. The meeting adjourned at 4:15.

Thursday, May 17, 1945

The Heavy Isotopes Group met at 8:30 a.m. in my office. The meeting was attended by Cunningham, Ghiorso, Hagemann, Hindman, Hyde, James, Jones, Katzin, La Chapelle, Manning, McLane, Morgan, O'Connor, Perlman, Peterson, Scott, Studier, R. C. Thompson, Van Winkle, Weissbourd, and me. Jones listed the samples we have from Site W:

W-1	26 gt
W-3	34 gt
T-5-03-RE5	62 gt
B-5-04-B1	90 gt

Manning read the proposals Kohman sent me about nomenclature:

$$\text{gt} = \text{g/long ton} = \text{ppm} \cong \frac{\text{megawatt days}}{\text{short ton}}$$

pb = pile bombardment

gte = gt equivalent (away from U) using additive terms

I then spoke on a number of general matters including the status of Berkeley bombardments.

Hindman discussed the status of the Np^{237} extraction.

Weissbourd described the search for U^{232} in the uranium + deuterons target. There is an alpha-particle peak at approximately 3.7 cm. He questions whether it is from Pu^{239} or U^{232}.

We scheduled the specific activity determination on 100 gt plutonium to be finished by next Thursday.

A memo, "Preparation of Absolute β Standards and Application to the Determination of Neutron Flux," by T. B. Novey and P. W. Levy arrived for me. I immediately routed it to some of the men.

I played 15 holes of golf at Evergreen with Morgan, Lawroski, and Perlman. (Tom Morgan and Steve Lawroski won our "low ball plus low total" match, 2 up. TM-56, SL-46, GS-45, IP-56 for nine, TM-93, SL-75, GS-80, IP-89 for 15.)

U.S. Marines have been battling in the suburbs of Naha, capital of Okinawa, for four days, and Admiral Nimitz announced that the island campaign has cost the lives of 3,781 U.S. troops and ground casualties of 20,950, according to this morning's paper.

Friday, May 18, 1945

At 8:30 a.m. I held a meeting in my office of the Council of Section C-I, which was attended by Albaugh, Cunningham, Davidson, Egan, Gilbreath, Hindman, Jaffey, Jones, Katzin, Lawroski, Manning, Simpson, Stewart, and R. Thompson. Manning reported on the Project Council Physics Information Meeting of May 15 and Chemistry Information Meeting of May 16. During the discussion of the Chemistry Meeting Jaffey interposed the question of the future status of the Information Meetings. I indicated that after July there is likely to be a quarterly meeting at which the men doing the work will report rather than the section chiefs--we do not know whether this will include men from other sites. I added that as far as our own work is concerned, the procedure probably will be to prepare a brief monthly report like our present report to the division head with the details written up as collected papers; this will save writing long monthly reports and will make it unnecessary to rewrite them as collected papers next year. I explained that our weekly section meetings will be closely covered and written up the same day. (The notetaking will be rotated among a number of men.) I also pointed out that the usual type of monthly report will be necessary for June to finish reporting all old work.

I read a copy of Cunningham's memo to Daniels giving the Problem Abstracts for Sub-section 2, Section C-I, for April 15 through May 15, 1945.

In a memo to Wayne Johnson, I explain that when Stan Thompson returns to the Met Lab next month his status will be entirely changed from his previous assignment here; we plan to have him work in a broader field with more independent research effort.

I wrote to Vance Cooper at Site W, answering his request of May 9 for my advice on his future course of action. I say, with some hesitation, that Clinton is probably the choice to make on the basis that the possibility there of continuous employment is fairly good and that I know he really enjoys this field. I add that there is a fair chance that this field has a very good future. I mention that Warren Johnson will be at Site W for a few days beginning next Thursday to make arrangements for the return of men to Clinton. Cooper should take the opportunity to complete the final arrangements if this is his decision.

I also replied to Watt's letter of May 10 and, among other things, tell him that we have been very pleased with both Roy Thompson and Tom Morgan, Texans whom Watt suggested we hire.

Compton sent a memo to Whitaker requesting the transfer of Perlman from the Clinton payroll to the Metallurgical Project payroll as of May 21. He stated that it will be necessary for Perlman to spend from ten to fifteen days a month at Hanford on Project office business for the next four to six months, consequently he is being transferred for the time being directly to Compton's office rather than to the laboratory organization.

According to today's paper Japanese newspapers are preparing "the national mind" for word of the defeat at Okinawa.

Saturday, May 19, 1945

Mulliken sent all volume editors a memo dated May 18, giving the present status of the outline and editorial organization of the Metallurgical Project Record. I am listed as volume editor for Volumes 14A and 14B, Chemistry and Metallurgy of the Transuranium Elements; and Volumes 17A and 17B, Production and Separation of U^{233}. I am also on the editorial committee for Volumes 2A and 2B, General Nuclear Physics; Volumes 9A and 9B, Radiochemistry and Fission Products; and Volume 24, Project Handbook. Members of Section C-I are on the committees of Volumes 8A and 8B, Physical Instrumentation, (Ghiorso, Jaffey); Volumes 9 and 9A (Jaffey); Volumes 11 and 11A, Chemistry of Uranium, (Katz, Assistant Editor); Volumes 14A and 14B (Manning, Associate Editor, Cunningham); Volumes 15A and 15B, Bismuth Phosphate Separations Process (Albaugh, Associate Editor); Volumes 16A and 16B, Alternate Separations Processes (Manning, Associate Editor, Lawroski); Volumes 17A and 17B (Katzin, Associate Editor).

I received another letter from Stan Thompson dated May 17. He is following the suggestions I made to him. He mentioned many of the men will be leaving Hanford about the middle of June and taking vacations en route to their destinations. These include Goeckermann, Miller, Ballou, Hill, Gaarder, Lincoln, and Turk. A few men have been offered employment at other locations with du Pont. Bernie Fries has accepted a position with California Research Company.

I sent Joe Hamilton, at his request, a description of our best method for separating protactinium from thorium, uranium, and fission products. I also tell him that we have prepared a solution of about three micrograms of Pa^{231} for him which we will send as soon as we learn the mechanism for doing so.

The efforts in San Francisco to complete the formation of the United Nations are being delayed by Stalin. Gromyko keeps saying that he has not yet received his instructions.

Sunday, May 20, 1945

I played 18 holes of golf at Cog Hill No. 1 with French Hagemann, Steve Lawroski, and Iz Perlman. French and Steve were teamed in a "low ball and low total" match against Iz and me. French and Steve won the match, 7 and 6. (FH-114, SL-106, GS-110, IP-114). Helen, Iz, and I then played nine holes of golf at Jackson Park.

Monday, May 21, 1945

Perlman was officially transferred from the Clinton Laboratories payroll to the Metallurgical Project payroll today.

I received a copy of a May 21 memo from Albaugh to Daniels giving the problem abstracts for Sub-section 1, Section C-I for the period April 15 to May 15, 1945.

Heavy fighting still continues in Okinawa, according to today's paper, and U.S. troops are closing in on Shuri, Okinawa's fortress city.

Tuesday, May 22, 1945

Budd Gore notified the employees that Memorial Day, May 30, will not be regarded as a holiday, but that weekly-rated employees will be paid time and one-half for that day.

Hogness informed Stearns that since Egan is leaving to return to his former job, it has been necessary to reorganize his group. Therefore, we find it necessary to retain the services of Bernard Weissbourd who had been slated to go to Clinton Laboratories.

Compton sent a letter of appreciation to Franck for his important contributions to our activities during the past years. Franck is leaving the Project as of June 30.

At night Perlman left to return to Hanford.

An article entitled, "Secret Army Fought Nazi Atom Bomb" appeared in the London Express dated May 21, 1945. It is the story of the sabotage efforts on the heavy water plant at Rjukan, Norway, in 1943 and the sinking of a ferry in April of 1944, carrying 12 tons of heavy water bound for Austria.

Great Britain's first general election in ten years appears imminent, according to today's paper, rejecting Churchill's proposal for extension of the present coalition government until the end of the Japanese war.

Wednesday, May 23, 1945

The Council of Section C-I met in my office at 8:30 a.m. Present were Albaugh, Cunningham, Davidson, Egan, Ghiorso, Gilbreath, Hindman, Jaffey, Jones, Katzin, Lawroski, Manning, Simpson, Stewart, and Thompson. I announced that Stewart will take over Jones' job of arranging for irradiations after July 1.

I then asked for a review of the writing situation. Davidson, Cunningham, Albaugh, Lawroski, Simpson, and Manning reported. Albaugh mentioned that most of his chapters are finished because of a ghost writer who is available, namely, Hyman. Lawroski said that his chapters are progressing; again the good ghost writer, Hyman, is being used.

I mentioned we are associated with some other volumes as well as those for which we have primary responsibility, pointing out that I am on the editorial board of Volume 2 (General Nuclear Physics) and that the section on heavy isotopes of that volume will probably contain the heavy end of the isotope table, brought up-to-date, and a chart of the four radioactive series. Volume 8 on instrumentation will have Ghiorso, Jaffey, and English on the editorial board; and I am on the editorial committee of Volume 24, the Project Handbook (although I do not know yet what is to be done).

Stewart again urged that recovery samples be sent to him.

I told the group that Allison has sent us the rare earth fraction from plutonium purified at Site Y and that James has found approximately 2×10^7 c/m of 95^{241} which would correspond to 0.5 micrograms if the half-life is 50 years (we know it is greater than 15 years). The material still has to be separated from 1 gram of lanthanum.

Simpson brought up the matter of finding documents marked "restricted" when he was patrolling Monday night. During a general discussion of security rules which followed, I remarked that once you are down for a violation, no matter whether it is justifiable or not, it is just about as irreversible as getting a sock in the head.

Hindman asked if the "no smoking" rule could be modified to allow smoking in offices and laboratories where no product is handled. I thought this might lead to the loss of the filtered air, which perhaps we do not want any more. I added that I have become very used to clean laboratories. After determining that the "no smoking" rule works a hardship on four of the men, I agreed to investigate the possibility of relaxing the "no smoking" rule for the offices.

I brought up the question of meetings after July 1. Jaffey proposed that the planning-type meeting be held in the daytime because at night one is too weary to do that sort of thing carefully. I asked about the Wednesday night type of meeting. We discussed dropping it because everybody will be in on the regular planning meetings which will cover all the material. Finally it was decided that there should be a meeting periodically to coordinate all the work of a particular type. The opinion was expressed that once every two weeks should be adequate for such meetings and, in that case, an evening meeting would not be objectionable.

Katzin sent Daniels a memo dated May 21 giving the Problem Abstracts for Group 9, Section C-I, for April 16 to May 15, 1945.

I read a copy of a memo dated May 22 from Zachariasen to Stearns about the results of his examination of a new sample of protactinium oxide submitted by Larson of Section C-I. He found only one crystalline phase in the sample, the same as observed in an earlier sample. The lattice constant is still too low for the phase to be pure PaO_2. He stated that further progress on this problem depends upon the ability of the chemists to provide reliable chemical information about the sample.

In a memo dated May 22 to Laboratory and Division Directors, Mulliken asks for comments to be used in preparing a two-page cross section of laboratory opinion on the following questions:

> What will the nation gain by open publication of the great bulk of the information obtained in the work of the Project?
>
> Where should the line be drawn between material to be published and material to be withheld? (For example, detailed designs of major installations or details of special processes might be withheld.)

Compton was notified by Colonel Nichols that an additional 25 grams of U^{235} and 15 grams of Pu^{239} will be shipped to the Project at the earliest possible date.

At 7:45 p.m. in Room 209, Eckhart Hall, I attended a meeting of the U^{233} Group. Others present were Ader, Asprey, Egan, Florin, Ghiorso, Gilbreath, Hagemann, Hindman, Hyde, Jaffey, Jones, Katzin, Larson, Manning, Morgan, Peterson, Phipps, Reinhardt, Simpson, Stewart, Studier, Roy Thompson, Van Winkle, and others. I opened the meeting and turned it over to Katzin to preside.

5/23/45 (cont.)

Studier, the first speaker, reported on absorption studies of the radiations from enriched U^{235} and from Pa^{233}. In the U^{235} sample he has found a 170 kev gamma-ray of yield exceeding the number of alpha disintegrations of U^{235} present in the sample. It is suggested that the gamma-ray may originate with the U^{234} present which has a much higher specific activity. The examination of the Pa^{233} sample did not show the 1.5 Mev component that had been reported by Stoughton. There seems to be at least seven components, in addition to the alpha-ray component, in the radiation spectrum of U^{233}.

Hagemann discussed some further work on the decay products of U^{233}, first reviewing the work of six months ago which resulted in a tentative scheme involving alpha decay of Ra^{225} to an alpha-emitting emanation. Work was discontinued to allow time for a new sample of U^{233} to decay and improve the carrying chemistry involved. The pressure of other more urgent assignments prevented resumption of the work until very recently. A few weeks ago word was received that the Montreal group had found Ra^{225} to be a beta-emitter. The resulting Ac^{225} decayed by successive alpha emissions through 87^{221} and 85^{213}. At that time work was resumed here and resulted in confirmation of the following:

$$U^{233} \xrightarrow[1.6 \times 10^5\ y]{\alpha} Th^{229} \xrightarrow[ca.\ 3000\ y]{\alpha} Ra^{225} \xrightarrow[ca.\ 26\ d]{\beta^-} Ac^{225} \xrightarrow[10\ d]{\alpha}$$

$$\begin{tikzcd}
 & & & Po^{213} \arrow[dr, "\alpha"] & & \\
87^{221} \arrow[r, "\alpha", "4\text{-}5\ m"'] & 85^{217} \arrow[r, "\alpha", "very\ short"'] & Bi^{213} \arrow[ur, "\beta^-"] \arrow[dr, "\alpha"'] & & Pb^{209} \arrow[r, "\beta^-", "3.3\ h"'] & Bi^{209} \\
 & & & Tl^{209} \arrow[ur, "\beta^-"'] & &
\end{tikzcd}$$

Hagemann then described the experimental procedure followed to isolate the decay products and measure the yields.

Katzin next gave a report on the work of Larson and others, originally meant to be presented by Larson. He described the extraction of protactinium from a barrel of carbonate ore residues that resulted in 1.76 mg of pure white material, expected to be mainly protactinium. After sending a sample to Zachariasen for analysis, the remaining material was dissolved. A portion was submitted for spectrographic analysis, and aliquots were counted in the nitrogen chamber. If it is assumed that the white ignited material is Pa_2O_5, the counts obtained calculate to a half-life of 52,000 years for Pa^{231}. The literature value is 32,000 years ± 10 percent. The results of Zachariasen's examination suggest that there is either some seven percent by weight of zirconium dioxide present or there are vacancies in the metal sites of the dioxide crystal which lead to a formula of $Pa_{0.9}O_2$. Spectrographic results show no definitely detectable zirconium;

the major impurity is 14 percent calcium. Some of the men present questioned whether the reported calcium content is real.

Examination of a sample of the material in the pulse height analyzer showed a subsidiary alpha-particle peak at a range of 3.23 cm. This means there must be an equally abundant gamma-ray of about 325 kev energy (also observed many years ago in studies by Miss Meitner of Hahn's laboratory). Katzin suggested that this gamma-ray of known energy and abundance could be used to calibrate the counting efficiency of radiation of this energy. I pointed out that the conversion coefficient of the radiation must be known also. There was a general discussion of how the data desired for obtaining counting efficiencies of quantum radiation could be obtained. The meeting was then adjourned.

Yonabaru, fifth city of Okinawa and eastern anchor of the formidable line across the island, fell to U.S. troops yesterday, according to today's paper.

Thursday, May 24, 1945

The Heavy Isotopes Group meeting in my office at 8:30 a.m. was attended by Britain, Cunningham, Florin, Ghiorso, Hagemann, Hindman, Hopkins, Hyde, Jaffey, James, Jones, Katzin, La Chapelle, Magnusson, Manning, Morgan, O'Connor, Peterson, Scott, Studier, R. C. Thompson, Van Winkle, and Weissbourd. I read Allison's letter dated May 21 about the irradiated Pu^{239} and U^{235} samples (Farmer's Special No. 2). We discussed in detail the plans for working on them.

Magnusson reported on the status of the Np^{237} isolation. He has observed x-rays in some of the Np^{237}.

Hindman described his specific activity measurements on Pu^{239}-Pu^{240} mixtures.

Peterson talked about the latest results on actinium chemistry.

James completed isolating the 95^{241} from sample 51c (rare earth fraction from 160 g of plutonium received from Site Y on May 15). Pulse analysis indicates 2.5×10^7 alpha c/m; about 3 percent is Pu^{239}.

I replied to Mulliken's May 22 request for opinions on the question of publication of Project information. I state that my present view is to favor open publication of the majority of the information obtained in the work of the Project, both in the broad sense of informing the public and in the scientific tradition of using the regular scientific channels. I state that I believe it is to the advantage of world security to do this and, hence to the advantage of our nation. It would appear nearly impossible for a conference such as the San Francisco Security Conference to evolve an effective peace plan without taking the atomic weapon into account.

With regard to the question of what proportion of the material and which of the material should be published, I indicate that my present inclination is to publish most of it, as to withhold information would only breed distrust with no good accomplished since any other large country could obtain the information in a short time. Examples of what I think should be published include the existence of and the important nuclear properties of the heavy isotopes, the fundamental chemical information about the nuclear chain reactions and the chain reacting structures. I observe that perhaps it would be all right to withhold some of the information about the fabrication of the final weapon and the actual detailed design of the major manufacturing installations.

Today's paper reports a shakeup in the President's cabinet--Attorney General Biddle will be succeeded by Tom C. Clark, Secretary of Labor Perkins by Federal Judge Lewis B. Schwellenbach, and Secretary of Agriculture Wickard by Representative Clinton P. Anderson.

Friday, May 25, 1945

I received a memo from Nickson with an unusual request. He notes that, as we know, the most favored site for deposition of plutonium in the body is the bones. His group has found in experiments with dogs that teeth, while not containing as much plutonium as bones, contain distinctly more plutonium than soft tissue. Therefore, he would like to receive any teeth extracted from individuals working with plutonium.

I played 16 holes of golf at Jackson Park with Arnold. (LA-45, GS-50 for nine; LA-82, GS-88 for 16.)

According to today's paper the campaign on Mindanao is in the mopping-up stage.

Saturday, May 26, 1945

I attended a meeting in Eckhart Hall at which Colonel Lansdale spoke about security. At the close of the meeting Daniels and I spoke to him about the questions on political views being asked by security investigators. Specifically, I mentioned the investigator who asked whether Al Ghiorso had been an ardent supporter of Roosevelt during the last election. Daniels and I consider such questions to be improper and possibly deterimental to the morale of the scientists.

Helen and I went to Tam O'Shanter Golf Course to see Bob Hope, Bing Crosby, Jim Hines, and Chick Evans play in a Bob Hope Golf Tournament.

Japanese suicide pilots heavily attacked U.S. shipping and air field installations in the Okinawa area on Thursday and Friday nights, according to today's paper. This is a new form of suicide attack with bombers heavily laden with Japanese soldiers carrying grenades attempting crash landings.

Sunday, May 27, 1945

French Hagemann, Jerry Howland, S. Sheel, and I travelled by I.C. RR to the Cherry Hills Golf Club in Flossmoor, Illinois, to play 18 holes of golf. We walked about a mile to and from the Golf Club from the Flossmoor station. (French and I won a "low ball plus low total" match, 2 up. FH-110, GS-103, JH-110, SS-116.)

Monday, May 28, 1945

Stearns requested that Bartky release him from the Met Lab as of July 1. Stearns feels he has completed the work he came to the Met Lab to do and he now wants to accept a position offered him at Washington University. Stearns points out that being in St. Louis to assist Compton will enable Compton to give more of his time to the Met Lab.

I read Eugene Rabinowitch's response to Mulliken's request for comments about the publication of Project results. Rabinowitch says that the publication of the Project Record is a problem of national importance and "it cannot be decided by present Army Security authorities or other government departments concerned exclusively with military problems of the present war." He feels the basic facts concerning the successful release of nuclear power and its immense destructive possibilities should be made public and impressed upon public opinion in this country and all over the world without further delay. Rabinowitch concludes, "The main point I wanted to make in the present remarks is that revelation of the fundamental facts concerning nuclear weapons and an attempt to arrive at a world-wide agreement as to their control should precede the final decision concerning the publication of Project results."

I also received and read Farrington Daniels' response to Mulliken's request. He generally favors publication for reasons such as more rapid advances in nucleonics and showing the American public that there is an absolute necessity for the elimination of war. He does say, however, that if an armament race appears inevitable, we should change our policies and keep secret details on how to build a pile or a weapon, use such secret information as a bargaining power to force international cooperation and control, and recognize that we need, under such circumstances, a vigorous research program.

Chinese troops, in an important victory, captured the important river port of Yongning (Nanning) on the Si River, shutting Japan's highway lifeline to Indo-China and Singapore, according to today's paper.

Tuesday, May 29, 1945

I read a copy of a May 28 memo from Zachariasen to Stearns reporting on the identification and crystal structure of $PuCl_4$, identified from a tan-colored volatile substance observed by Westrum in working with $PuCl_3$; the interpretation of the x-ray diffraction pattern shows the sample to consist of 65 percent $PuCl_4$ and 35 percent $PuCl_3$. It is tetragonal and has the UCl_4-type of structure.

Zachariasen goes on to point out that early attempts to prepare $PuCl_4$ were unsuccessful and that the chemists expressed doubt as to its existence. Because there is no obvious crystal chemical reason why the tetrachloride should not exist, he states he has been urging a new attempt to prepare $PuCl_4$. According to Zachariasen, Davidson does not have faith in the crystal chemistry argument nor does he share Zachariasen's doubts as to the conclusiveness of the early work. "Under these circumstances," wrote Zachariasen, "it gives some satisfaction to announce that $PuCl_4$ now has been made unintentionally as has been true of the first preparation of so many plutonium compounds."

I received a copy of a memo Hogness sent to Wayne Johnson asking that Albaugh's termination date be changed from July 1 to July 15 in order to permit him to complete his writing of Metallurgical Laboratory Reports. The same would apply to his secretary Dorothy Black who is working on the same reports.

As a result of Mulliken's request, Nickson wrote a memo to Jacobson today in which he states:

> Therefore it seems to me that one of the most important functions at hand is the education of the general public to the fact that the era of atomic power is at hand. Further, the implications of this fact must become general knowledge. If the public, as a whole, is not entirely conversant with the broad implications of the field of nuclear physics, it seems reasonably certain that the public will not accept any organization which attempts to take into account the implications of these facts.

Hogness sent Stearns data on the SED and ERC men now in the Chemistry Division about their availability for other assignment. Those of my men listed as not available for transfer are Ader, Asprey, Britain, Hopkins, Post, Schraidt, Stewart, Studier, and Weissbourd.

Two others are to be retained until October 1, Hausman and Kelley, who are on the solvent extraction program.

I wrote to Allison at Site Y that we sent him today (through the Area Office) 22.0 mg Np^{237} in the form of Np(V) nitrate solution. I remind him that we would like half the material returned when he is finished with his experiments. I mention we are retaining 2 mg and estimate that we have obtained about an 80 percent yield from the material sent from Hanford. I explain that the Hanford yield has not been so high as expected because of an extraneous precipitate in the extraction step that caused a 60 percent loss. I go on to say that a modified neptunium extraction process has since been successfully tested in the Hanford semiworks and that we could get at least 100 mg of Np^{237} from a second run at Hanford. I ask how he would regard asking them to make another run--I, of course, am in favor of it. Then in a P.S. to the letter I add that Perlman has just told me that June would be a good time for a special neptunium run. I also tell him about our finding 2.5 x 10^7 c/m of 95^{241} in the sample (1945-5-5) that he shipped us and ask that he save more fractions of this type for us if it is not too much trouble for him.

Allison later called me from Site Y to tell me that the CW-2 samples (U^{235} and Pu^{239} irradiated in the Hanford reactor--part of "Farmer's special") will leave Site Y by truck on Friday and arrive here about Monday--they are in a one-ton lead cylinder.

I told him about the 22 mg of Np^{237} sent to him and suggested we ask for another run. He asked me to have Wigner initiate his request for 50 g of heavy vitamin B(B^{11}) through proper channels.

According to today's headlines, 450 B-29s hit a wide area of Yokohama, Japan's fifth city and an important industrial and shipping center.

Wednesday, May 30, 1945

At 8:30 a.m. the Council of our section met in my office. Attendees were Cunningham, Egan, Ghiorso, Gilbreath, Hindman, Jones, Katzin, Lawroski, Manning, Simpson, and Stewart. I opened the meeting by observing it would be short because of time needed for the inventory being taken. I reminded the men to fill out rating sheets for all the men before July 1, including those who are leaving. I then read the memo from Nickson requesting that any persons working with plutonium bring in any teeth which have been extracted in order to examine them for plutonium deposition. I also reminded everyone that the guards are under pressure this week to find as many violations as possible.

We explored the availability of one of our three spectrophotometers for use by Clinton and concluded that all three instruments should be retained here. I then asked that copies of the Project reproduction of the Reviews of Modern Physics "Table of Isotopes" be traded with Mrs. Rogers for the actual reprints.

I announced a new work assignment which is presumably still quite secret. We are to develop an analytical program for the recovery of uranium from low grade ores. Six men will be assigned to work out the procedures, and then six men (these or others) will be sent to Mallinckrodt at St. Louis in about six months.

I concluded the meeting by saying that after July 1, the Separations Sub-section will consist of only one group working on solvent extraction and will be part of the other section of the Chemistry Division. Lawroski and Gilbreath will be group leader and assistant group leader, respectively. The group will consist of 12 men.

Everyone in Section C-I participated today in taking an inventory of all equipment and non-expendable supplies in the Section.

Zachariasen supplemented his May 28 memo to Stearns announcing the identification of $PuCl_4$ with another memo dated May 29, in which he mentions that he has learned that the sample may have contained small amounts of impurities which could have stabilized the $PuCl_4$ structure; hence the existence of pure $PuCl_4$ has not been definitely proved. He urges that renewed attempts be made to prepare the tetrachloride using highly pure starting material.

Stearns sent me a letter announcing that, by virtue of my work at the Met Lab, I am qualified to receive a Certificate of Merit from the OSRD. He asks me to notify him if I have already received a certificate from another OSRD employer or, if not, if I want a certificate sent to me.

At 7:45 p.m. in Room 209, Eckhart Hall, I attended the meeting of the Basic Chemistry, Recovery, and Instrument Groups. Others present were Ader, Ames, Anderson, Mrs. Cortelyou, Cunningham, Davidson, Dixon, Florin, Fried, Ghiorso, Gilbreath, Greenlee, Hagemann, Hindman, Hopkins, Howland, Hyde, Hyman, Jaffey, James, Jones, Katzin, Magnusson, Malm, Manning, McLane, Morgan, Nickson, Peterson, Phipps, Rabinowitch, Robinson, Simpson, Stewart, R. Thompson, Van Winkle, Westrum, Miss Young, and others. I opened the meeting and then turned it over to Cunningham who had Anderson speak about his analysis of four types of Pu(IV) solid sulfate compounds: $Pu(SO_4)_2 \cdot 4H_2O$; $Pu_2O(SO_4)_3 \cdot 8H_2O$; $K_4Pu(SO_4)_4 \cdot H_2O$; $(NH_4)_4Pu(SO_4)_4 \cdot 1$ or $2H_2O$.

Fried described the preparation of neptunium metal by reduction of the trifluoride at 1,200-1,300°C with barium, calcium, lithium, or sodium vapor. Four 25-40 microgram buttons were made and found to be malleable. Crystallographic examination by Zachariasen indicates the structure is not cubic as is plutonium of density 16. The density was determined pycnometrically to be 17.1-17.8

Morgan described work on the 1 ml dissolver solution from Site W. The solution was estimated to contain 270 mg of uranium. Bombardment level was about 110 gt. The pulse analyzer showed that 91 percent of the alpha activity is due to Pu^{239} and Pu^{240}, with the remaining 9 percent due to uranium. A uranium fraction was isolated. The uranium plates were prepared electrolytically by Scott and Hufford. It was expected that the sample would contain about 17 c/m of 95^{241} and 11 c/m of 96^{242}; the fact that 70 and 50 c/m, respectively, were found would seem to indicate that more of these isotopes than expected result when plutonium is bombarded in a uranium slug. The isotopes found in the material and the amount to be expected in 250 gt material are as follows:

Uranium Isotopes		Plutonium Isotopes % of total α-activity		
Mass	% of total α-activity, 110 gt material	Mass	110 gt material	250 gt material
234	4	238	0.3	1
235	0	239	89.8	-
236 (?)	~0.3	240	1.2	3 to 4
238	4	Other Isotopes		
		95^{241}	~0.01	0.1 to 0.5
		96^{242}	~0.01	0.1 to 0.5

In answer to a question from Simpson, I indicated that the identification of U^{236} is, at present, very tentative.

James spoke about some new work with 95^{241}, the source of the material being the by-product material from the processing at Site Y of 160 g of plutonium as peroxide from Hanford. The sample (Met Lab No. 51c) was received as 2 g of solid material containing about 1/4 g of lanthanum, ammonium nitrate, iron, magnesium, and 40 mg of plutonium. The time of decay was one week, measured from the peroxide precipitation at Hanford to the sodium plutonyl acetate precipitation at Site Y. Five LaF_3 cycles were carried out to rid the sample of plutonium. The 2.5×10^7 c/m that resulted were shown to be 97 percent pure 95^{241}.

James quoted results he and Morgan have obtained that indicate that the half-life of 95^{241} is between 100 and 10,000 years, in which case, the mass of 95^{241} corresponding to the 2.5 x 10^7 cm would be between 1.4 and 140 micrograms.

Manning asked whether further separations have been made from the original 250 mg of 25 gt plutonium in order to make a more accurate estimate of the half-life of Pu^{241}. I replied that extraction of 95^{241} from a new, more highly bombarded sample is planned and that measuring the rate of decrease in the rate of growth of 95^{241} would then serve to bracket the half-life of 94^{241}.

Memorial Day's headline reads "51 Sq. Mi. of Tokyo in Ruins." This is a result of six incendiary raids starting last February 25 and has led to our loss of 50 B-29s.

Thursday, May 31, 1945

At 8:30 a.m. the Heavy Isotopes Group met in my office. The meeting was attended by Britain, Cunningham, Florin, Ghiorso, Hindman, Hopkins, Hyde, Jaffey, James, Jones, Katzin, La Chapelle, Magnusson, Manning, McLane, Morgan, O'Connor, Peterson, Scott, Studier, R. C. Thompson, Van Winkle, and Weissbourd. I explained about note-taking at our planning meetings after July 1: first week - Jaffey, second week - R. Thompson, third week - James, fourth week - Morgan, fifth week - Peterson. O'Connor is the alternate.

I led a discussion about how to handle the W-2 samples (Farmer's specials - Hanford bombarded plutonium and U^{235}) coming from Site Y in a one-ton lead container.

The only work activity scheduled was to have Morgan make a plate of the recent Hanford Np^{237} (about 200 micrograms) free of Pa^{233} for Ghiorso; he is also to prepare a plate of about 200 micrograms of the second Clinton Np^{237}. Protactinium-231 will be used as a tracer for the Pa^{233}.

We then discussed the status of the work of each of the other men present at the meeting.

Today is the last day at the laboratory for Irving Sheft. He is transferring to Monsanto.

Don Stewart completed his paper, "Solubility of Plutonium in Organic Solvents; Partition with Aqueous Media." It is to be used as Chapter 5 of Volume 14A of the Plutonium Project Record.

Today's paper reports that Chiang Kai-shek has resigned as premier but has retained his position as president and head of state. It is assumed that he will devote himself to his main job of supreme commander of Allied forces in the China theater of operation.

Friday, June 1, 1945

Stanley G. Thompson has now rejoined the Met Lab.

Hogness prepared a summary of the manpower distribution for the Chemistry Division as of June 1, 1945. It shows the following for my section.

		Number of Men Apr.	May
Albaugh (Separation Processes, 18 men)	R. Thompson, Extraction and decontamination	6	0
	Gilbreath, Process development	7	8
	Lawroski, Solvent extraction	12	9
Cunningham (Basic Chemistry, 37 men)	Simpson, High vacuum work	9	7
	Hindman, Basic chemistry	11	13
	Stewart, Recovery	6	6
	Ghiorso, Instruments and physical instruments	9	10
Katzin (23 work, 9 men)	23 work	7	8
	No group assignment		2
Administration - Seaborg, Manning, Jones, Albaugh, Cunningham, Katzin		6	6
	Total	73	69

At 3:30 p.m. Perlman at Site W called me. He gave me some data on the dissolver solutions, W-11 and W-12. Perlman also told me that of the "Farmer's Samples," No. 2 received about six times the irradiation of No. 1 and was removed from the pile on April 12, 1945.

Perlman asked me to tell Daniels that he has an upper berth to Portland at 1:30 a.m., June 9. He also gave me a message to pass on to Burton--Samples No. 99 and No. 161 have equal irradiation. This does not explain the discrepancies.

I played 18 holes of golf at Jackson Park with Winston Manning. (WM-116, GS-93.)

According to today's paper the U.S. launched a big B-29 daylight attack on Osaka, and American troops broke through Japanese defenses on Okinawa. At home there is mounting concern over the struggle for supremacy in Europe as Great Britain and Russia maneuver for the top position.

Saturday, June 2, 1945

I received a letter dated May 18 from President Robert Sproul in Berkeley. He explained that my letter of February 15, seeking the University's agreement that we assign all rights of Case 61 to the Government, has remained unanswered this long a time because the information it gave was at variance with representations earlier made to the Regents of the University of California. The Regents had understood that we were to assign all of our interests in the patents and seek no personal reward. Sproul went on to say that they have reconsidered their position and have decided not to agree to the proposals in my letter, at least until more information is available.

I sent a copy of Sproul's letter to Kennedy at Site Y. I explain I received it just about five minutes ago and have not yet formed any opinion. I ask for reactions from him, Emilio, and Art to this new information.

Albaugh completed preparation of Chapter 8, Volume 15 of the Project Record, entitled "Decontamination Cycles."

In the early afternoon, I attended a meeting about a request for suggestions on the postwar future of nucleonics, received from the panel composed of Compton, Fermi, Lawrence, and Oppenheimer. This group of men has been appointed as the scientific panel to the Interim Committee, which has been created as an advisory committee to the Secretary of War. Others present were Bartky, Cole, Daniels, Dempster, Franck, Hogness, Howe, Hughes, Jacobson, Mulliken, Nickson, Stearns, Szilard, Warner, Wigner, and Zinn.

"Big U.S. Gains on Okinawa!" reads today's top headlines. Also in the paper is the text of the President's message to Congress yesterday, outlining his plan to bring the war in the Pacific to an end.

Sunday, June 3, 1945

I played 18 holes of golf at Silver Lake Golf Club, south course, (147th Street and 82nd Avenue, Orland Park, Illinois) with Stan Thompson, Steve Lawroski, and Winston Manning. (ST and SL won a "low ball plus low total" match, 6 and 4. ST-108, SL-97, GS-101, WM-119). We then played nine holes on the North Championship Course. Stan and I won a match. (ST-53, SL-57, GS-52, WM-69.)

Jaffey left in the evening on a trip to the Mallinckrodt plant in St. Louis. This is in connection with our assignment to develop improved analytical methods for their use in the uranium extraction process.

The Chicago Sun's headline this morning says, "Russia Touches Off Crisis at Parley." Moscow is demanding a complete Big Five veto in the United Nations' charter at the discussion in San Francisco.

Monday, June 4, 1945

Jaffey is still visiting the Mallinckrodt plant in St. Louis.

I issued an organization chart of Section C-I as of June 1, 1945. This chart shows that the following men are leaving my section on or before July 1: Abraham, Bradt, Davidson, Dixon, Dorsey, Egan, Fields, Greenlee, Hellman, Hopkins, Howland, Hufford, Krueger, La Chapelle, Larson, McLane, Phipps, Post, Reinhardt, Simon, Winner, and Wolf. We are also losing Mildred Bolden, a secretary; seven technicians; and two laboratory assistants.

Organization of Section C-I
(As of June 1, 1945)

Glenn T. Seaborg - Section Chief
Ruth P. Rogers - Secretary to Seaborg

Winston M. Manning - Associate Secion Chief
Jane Horwich - Secretary to Manning

Thomas O. Jones - Assistant to the Section Chief
Mary Williams - Secretary[1]
Beatrice Greenberg - Typist[2]

Thelma Hays - Typist, on loan from Information Department
Norma Shaw - Draftsman, from Information Department

Section C-I, Separation Processes

Frederick W. Albaugh - Assistant Section Chief
Dorothy Black - Secretary to Albaugh

Group 3, Process Development,
Gilbreath, James R. - Group Leader
Blaedel, Walter J. - Research Associate
Bradt, Rexford - Research Associate
Post, Roy [SED] - Research Assistant[2]
Sedlet, Jacob - Research Assistant

Walling, Mathew T. - Research Assistant
Winner, Bernard - Research Assistant[2]
Bolden, Mildred - Secretary[2]
Boykin, Pearline - Technician[2]

Group 4, Solvent Extraction,
Lawroski, Stephen - Group Leader
Egan, C. J. - Assistant Group Leader[2]
Schaffner, Irwin J. - Research Associate
Simon, Wilbur - Research Associate[2]
Hyman, Herbert H. - Research Associate
Ader, Milton [SED] - Research Assistant
Hausman, Eugene A. [SED] - Research Assistant (half-time)
Reinhardt, Richard - Research Assistant[2]
Schraidt, John H. [SED] - Research Assistant
Dorcy, Dan J. - Draftsman
Giacchetti, Olga - Technician (half-time)
Murray, Betty - Technician[2]
Krinsky, Jerome - Technician[2] (part time)
Summers, Mildred - Technician[2]

Sub-section II, Basic Chemistry and Service

Burris B. Cunningham, Assistant Section Chief

Group 5, Basic Dry Chemistry,
Simpson, Oliver C. - Group Leader
Davidson, Norman R. - Assistant Group Leader[2]
Abraham, Bernard - Research Associate[3]
Fried, Sherman - Research Associate
Phipps, Thomas E. - Research Associate[4]
Seifert, Ralph - Research Associate
Westrum, Edgar R. - Research Associate
Erway, Norman - Glassblower
Thomson, Helen - Technician

Group 6, Basic Wet Chemistry,
Hindman, James C. - Group Leader
Howland, Jerome J. - Research Associate[7]
Morgan, Leon - Research Associate
Ames, Donald [SED] - Research Assistant
Dixon, Jonathan - Research Assistant[2]
Florin, Alan E. - Research Assistant
Greenlee, Roy W. - Research Assistant[2]
James, Ralph A. - Research Assistant
La Chapelle, T. J. - Research Assistant[5]
Magnusson, Lawrence - Research Assistant
McLane, Keith - Research Assistant[2]
O'Connor, Paul - Research Assistant
Peterson, Sigfred - Research Assistant
Mokstad, Betty - Technician[6]

Group 7, Recovery,
Stewart, Donald C. [SED] - Group Leader
Anderson, Herbert H. [SED] - Research Associate
Asprey, Larned B. [SED] - Research Assistant
Britain, J. W. [SED] - Research Assistant
Fields, Paul - Research Assistant[2]
Fineman, Phillip [SED] - Research Assistant
Giacchetti, Olga - Technician (half-time)

Group 8, Instruments and Physical Measurements,
Ghiorso, Albert - Group Leader
Jaffey, Arthur H. - Assistant Group Leader
Krueger, Albert C. - Research Associate[8]
Robinson, Herman P. - Research Associate
Crawford, John A. - Research Assistant
Dorsey, John - Research Assistant[8]
Hufford, Duane - Research Assistant[8]
Scott, Benjamin - Research Assistant
Walsh Patricia - Research Assistant
Weissbourd, Bernard [ERC] - Research Assistant
Billington, Hubert - Technician
Towle, Virginia - Technician[8]

Group 9, 23 work
Katzin, Leonard I. - Assistant Section Chief
Hagemann, French T. - Research Associate
Hellman, Nison N. - Research Associate[8]
Larson, Raymond G. - Research Associate[8]
Studier, Martin [ERC] - Research Associate
Wolf, Michael - Research Associate[8]
Hyde, Earl - Research Assistant
Kelley, Alec [SED] - Research Assistant
Van Winkle, Quentin - Research Assistant
Pinckard, Marian - Technician[8]

No Group Assignment
Thompson, Roy C. - Research Associate
Thompson, Stanley G. - Research Associate
Hopkins, Horace [SED] - Research Assistant
Hausman, Eugene A. [SED] - Research Assistant (half-time)
Malm, John G. - Research Assistant

Service Group
Florin, Kathleen - Supervisor
Freeman, Elsie May - Laboratory Assistant[8]
Porter, Lillie May - Laboratory Assistant[8]

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[1]Leaving June 16, 1945
[2]Leaving June 30
[3]Leaving June 25
[4]Leaving June 23
[5]Leaving June 9
[6]Leaving June 10
[7]Leaving June 15
[8]Leaving July 1

I was appointed to two of six committes formed at the Met Lab to prepare suggestions for the postwar future of nucleonics--the Program Committee chaired by Zinn and the Committee on Social and Political Implications chaired by Franck.

There was a Chemistry Seminar in Room 251, Ryerson Laboratory. Franck spoke on the principles of radiation chemistry.

Top news today is again from Okinawa where U.S. troops have cut off the Chinen Peninsula - the Southeast portion of Okinawa.

Tuesday, June 5, 1945

The CW-2 samples arrived from Site Y by truck. These are "Farmer's special" samples of U^{235} and Pu^{239} irradiated in the Hanford reactor. They have received about six times the exposure of CW-1 samples.

An organization chart of the scientific divisions of the Met Lab as of June 5, 1945 was issued by Gore.

Organization Chart

Physics
A. J. Dempster, Director
E. P. Wigner, Associate Director

Sections

P-I Instrument
W. P. Jesse, Section Chief

P-IV Crystal Structure
W. H. Zachariasen, Section Chief

P-V Mass Spectroscopy
A. J. Dempster, Section Chief

P-VII Theoretical
Gale Young, Section Chief

P-VIII- Properties of Solids
Gale Young, Section Chief

P-IX Engineering Physics
L. A. Ohlinger, Section Chief

P-XIII
L. Szilard, Section Chief

Health
R. S. Stone, Director
L. O. Jacobson, Associate Director

Sections

H-I Clinical Medicine and Medicinal Research
K. S. Cole, Section Chief
C. L. Prosser, Associate Section Chief
A. Brues, Assistant Section Chief

H-III Medical Industrial Hazards and Health Physics
J. J. Nickson, Section Chief
J. E. Rose, Associate Section Chief

Advisory Committee

William Bloom	Paul Hodges
Alexander Brunschwig	C. J. Watson
W. R. Harrison	Sewell Wright

Chemistry
T. R. Hogness, Director
J. Franck, Associate Director
F. Daniels, Associate Director

Sections

C-I Separation Studies and Basic Chemistry of the Heavy Elements
G. T. Seaborg, Section Chief
T. O. Jones, Assistant to Section Chief
W. M. Manning, Associate Section Chief
B. B. Cunningham, Assistant Section Chief
L. I. Katzin, Assistant Section Chief
F. W. Albaugh, Assistant Section Chief

C-II Radiation Studies
M. Burton, Section Chief
A. O. Allen, Associate Section Chief

C-III Chemistry of the Fission Products
W. Rubinson, Associate Section Chief

C-IV Analytical Services
J. I. Watters, Section Chief
M. S. Fred, Associate Section Chief

Metallurgy
F. Foote, Director (Acting)

Sections

M-I Metallurgy
F. Foote, Section Chief

M-II Corrosion
E. W. Brugmann, Section Chief

Technology
J. C. Stearns, Director

Section
T-IV Optics
G. S. Monk, Section Chief
W. H. McCorkle, Associate Section Chief

"Chemical Research - Separation Processes for Plutonium" (Seaborg, Section Chief; Manning, Associate Section Chief, Albaugh, Assistant Section Chief) Report for Month of May 1945 (CN-3000) was issued today. It contains the following information of interest.

Solvent Extraction - Research (Gilbreath, Group Leader).
Redox Process operating conditions. Walling and Post drew up operating conditions and a complete flowsheet for semiworks column tests on the basis of batch equilibrium data on the various systems involved. The columns have been designed to give 98-99 percent recovery of plutonium.

Decontamination obtained by the Redox Process. Winner, Walling, and Sedlet carried out batch extraction experiments giving MDF's (minimum decontamination factor) of 10^7-10^8 for beta particles and greater than 10^6 for gamma-rays. This indicates that adequate decontamination should be obtained.

Distribution of cerium, zirconium, ruthenium, and neptunium tracers in the Redox Process. Blaedel, Malm, Hopkins, and Reinhardt carried out tracer experiments that indicate cerium, zirconium, and ruthenium should separate quite efficiently from plutonium in the extraction column. It has also been shown that neptunium will follow plutonium quite completely through the process.

Effect of radiation upon the Redox Process. Hopkins, Penneman, and Blaedel found that exposure of a two-phase system, simulating that expected in the first column, to beta radiation five times that of production plant levels produced no noticeable effect on physical charecteristics, plutonium extraction, or decontamination.

Reducing agents for the Redox Process. Winner, Post, Blaedel, Reinhardt, and Walling did further work on a substitute for hydroquinone and found that a combination of Fe(II)-urea or Fe(II)-hydroxylamine should be satisfactory.

Solvent Extraction (Lawroski, Group Leader).
Redox solvent extraction process-development. Egan, Schaffner, Schraidt, and Simon. The two 3-inch diameter columns were used to test the Redox Process with inactive solutions--considerable precautions must be taken to prevent decomposition of the hydroquinone in the acid hexone scrub solution for the second column. Materials are being ordered for the construction of five smaller columns for testing the process through two cycles with active solutions.

Report CS-3009, Chemistry Division Summary Report for May 1945 was also issued today by the Chemistry Division Director's Office. All Section C-I information in this report appears in the summaries prepared by Albaugh, Katzin, and Cunningham for my use at the Project Council Information Meeting on Chemistry on May 16.

In addition, the Metallurgical Laboratory Report for May 1945 was prepared and issued by the Laboratory Director's Office. The summary section of the report noted (1) The job of selecting the scientific personnel who will remain after July 1 has been completed. Many of those leaving will be working at other DSM laboratories. (2) More than 50 percent of the total shop time is being devoted to work on the Thunderhead job for Site Y. The shop at New Chem is being enlarged to take care of all the work required by the Chemistry Division. (3) The Chemistry Division is starting construction of the "hot laboratory" and is building the equipment necessary to complete the solvent extraction study.

The war in the Pacific continues on several fronts--new marine landing on the west coast of Okinawa, most of Chinen Peninsula seized by infantry, and a big fire bombing of Kobe.

Wednesday, June 6, 1945

The Council of Section C-I met at 8:30 a.m. in my office. The meeting was attended by Albaugh, Cunningham, Davidson, Egan, Manning, Simpson, Stewart, R. C. Thompson, and S. G. Thompson, I opened the meeting by initiating a discussion at Dr. Nickson's request that we consider having two alpha counts per day for those handling large amounts of alpha-emitters as is being done at both Site X and Site Y. After comments by Simpson, Katzin, and Ghiorso, I said that I will recommend that such procedure be set up.

I pointed out that I have received less than 10 percent of the letters of appraisal for the personnel reports and said "get them in." I then mentioned we are keeping all our present rooms until September 1.

I brought up the subject of recommendations on the scientific, political, and social future of our project in atomic power, pointing out that I am on a Committee on Social and Political Implications. After summarizing the general statement prepared by the local committee about the political aspects of atomic power, I asked the opinions of the men about the following possible alternatives in the use of the atomic bomb: (1) not use the weapon in this war but simply announce at the end of the war that we have it; (2) tell the world immediately that we have it and give a demonstration at which representatives of all countries, including Japan, are invited to see it; (3) make bombs as fast as possible and use them on Japan. I added that the committee's opinion is to follow the second procedure.

In the discussion which followed, one man said he would like to see no. 3 used on the basis that we should treat it as just another weapon. I asked if there was not a certain moral aspect that we must consider. If we are the first to use the weapon and then want to build up a world organization that would never allow its use again, our position would be very much weakened. I also mentioned that it might be important to give a demonstration from the point of view of keeping our allies' good will; this would be particularly important in the case of Russia who is our most likely future antagonist in view of Russia's great future potentialities as an industrial power.

The division of opinion of the people present was mostly between the possibilities of no. 2 and no. 3. Most people agreed that a strong international organization is ncesssary but that some precaution must be taken to prevent capturing the whole available supply of fissionable material by that organization. Some expressed pessimism to the possibility of forming a sufficiently strong organization to control the problem, but everyone agreed that it was essential to make the effort.

Lavender sent me a letter dated June 4 from Washington enclosing a new draft of the proposed agreement giving the Government (1) a non-exclusive and royalty-free license to the inventions covered by Case 52, and (2) all rights to inventions covered by Case 61. He has incorporated some of the changes suggested by the inventors which I transmitted to him with my April letter. Lavender has also sent copies of the letter to Kennedy, Segrè, Wahl, and Metcalf.

Report CN-3001, "Chemical Research - Basic Chemistry of Plutonium" (Seaborg, Section Chief; Manning, Associate Section Chief; Cunningham, Assistant Section Chief), Report for Month of May 1945 was issued today. It contains the following:

Basic Dry Chemistry Group (Davidson, Assistant Group Leader).
Observations on the reaction of PuF_3 with oxygen. Abraham and Davidson attempted unsuccessfully to measure the free energy change for the reaction 4 $PuF_3 + O_2 = 3\ PuF_4 + PuO_2$. (The work was reported because there will be no opportunity to do further work on the problem.)

Concerning the existence and stability of anhydrous $PuCl_4$ and $PuBr_4$. Davidson summarized the evidence for and against the existence of anhydrous $PuCl_4$ and $PuBr_4$. He argues that while solid $PuCl_4$ is unstable relative to decomposition into $PuCl_3$ and chlorine, $PuCl_4$ molecules exist in the vapor phase when chlorine is passed over $PuCl_3$ in the temperature range of 600°C-800°C.

Some reductions of uranium, thorium, and cerium compounds. Abraham and Davidson carried out a series of high temperature high vacuum reductions on these compounds with the aim of extending the most interesting to compounds of plutonium; subsequent developments prevented completion of the latter part of the program. The most interesting reactions discovered were:

(1) $UOS + C \xrightarrow{1900°C} US + CO$;

(2) $ThOS + C \xrightarrow{1900°C} ThS + CO$;

(3) $CeF_3 + 11/4\ Si \rightarrow 3/4\ SiF_4 + CeSi_2$.

Recovery Group (Stewart, Group Leader).

Separation of uranium and plutonium in Hanford dissolver solution. Britain has developed a method for separating the major portion of the uranium away from the plutonium and has processed a 1-ml sample of dissolver solution. The method made use of diuranate and plutonium(IV) hydroxide with ammonia.

Plutonium(IV) sulfates. Anderson has prepared mixed salts of Pu(IV) sulfates with ammonium sulfate and with alkali sulfates.

Plutonium(IV) compounds. Asprey has prepared a number of "plutonates," but the apparent complexities of the systems involved make definite identification of the salts impossible.

Plutonium(III) sulfates. Asprey has prepared a Pu(III) sulfate as well-formed violet crystals. Analysis would indicate the formula to be $Pu_2(SO_4)_3 \cdot 7H_2O$.

Instruments and Physical Measurements Group (Ghiorso, Group Leader; Jaffey, Assistant Group Leader).

n,γ cross section of Pa^{231}. Jaffey and Van Winkle have measured this cross section using material extracted from uranium ores by Van Winkle and Larson. Ten micrograms as the hydroxide were sealed in a quartz tube and bombarded for 14 hours along with some foil neutron flux monitors in the CP-3 pile. The bombarded sample was dissolved, and aliquots mounted on quartz plates and counted in a mica window Geiger counter. The cross section was computed at 290 barns. This may have a large error since the decay scheme is not known, and von Grosse's value for the half-life may not be accurate.

n,γ cross section of Io^{230}. Jaffey and Hyde have determined the cross section of Io^{230} by bombarding a 100 microgram ionium sample in CP-3 in a manner similar to the Pa^{231} sample. Depending on the decay scheme assumed for the UY and whether gold or Th^{232} is used as the flux monitor, they calculated the cross section to be from 31 to 112 barns. An experiment is planned to determine the cross section more accurately by bombarding the Io^{230}-Th^{232} mixture for a sufficiently long period to form U^{233} and Pa^{231} in countable quantities. Absolute alpha counting is much simpler to perform.

At 7:45 p.m. I attended a meeting in Room 209, Eckhart Hall of the Separation Processes Sub-section I. Others present were Ader, Albaugh, Ames, Asprey, Blaedel, Egan, Fields, Gilbreath, Greenlee, Hagemann, Hyman, Jaffey, Jones, Katzin, Larson, Lawroski, Manning, McLane, Morgan, Peterson, Reinhardt, Schaffner, Sedlet, Studier, R. Thompson, S. Thompson, Van Winkle, Walling, Winner, and others. Stan Thompson reported on recent developments at Site W, indicating that losses in the plant have been averaging about 7 percent, with most of the loss being rather evenly divided among the extraction step, the LaF_3 by-product step, the LaF_3 product step, and the metathesis step, each of which has been running between 1 and 1-1/2 percent. Modifications incorporated in the process have been designed primarily to permit reduction in the volumes of process solutions. The laboratory work at Hanford has been directed mainly toward modification and improvement of the process, and an auxiliary concentration scheme has been developed and tested on a laboratory scale. It involves dissolution of a bismuth phosphate product precipitate (probably from a third cycle) in an aqueous solution of KOH and glycerol and reprecipitating the plutonium by heating to 50-75°C.

The next speaker was Blaedel who reported on a series of experiments performed in an effort to estimate the decontamination that can be obtained with the Redox Process under the recommended conditions of operation. On the basis of batch extractions it appears that decontamination should be satisfactory.

The question of veto power in the international security conference in San Francisco developed into a world crisis of far-reaching implications. There is disagreement over what was agreed to at the discussions at Yalta on the veto question.

Thursday, June 7, 1945

The Heavy Isotopes Group met in my office at 8:30 a.m. The meeting was attended by Cunningham, Florin, Ghiorso, Hagemann, Hindman, Hopkins, Hyde, Jaffey, James, Jones, Katzin, Magnusson, Manning, Morgan, O'Connor, Peterson, Scott, Studier, R. C. Thompson, S. G. Thompson, Van Winkle, and Weissbourd, After I covered a number of general matters, Ghiorso talked about x-rays from 100 gt Np^{237}, which he was unable to find.

Weissbourd discussed work on a 95^{241} and 96^{242} mixture and a search for U^{232} in the sample from the uranium plus deuterons bombardment.

Hagemann described Th^{229} and Th^{228} from the U^{233}-U^{232} mixture. A number of the other men reported on their work. There was also much discussion about the efficiency of x-ray counting in Geiger gases and walls.

At the next meeting Florin will review his work.

Jones sent a memo to Furney asking him to arrange for the proper disposal of a very large number of waste solutions and materials containing plutonium too dilute to justify recovery. The total plutonium involved is about 347 mg.

"Big Jap Air Field Captured" reads today's headline; this refers to the completion of the capture of Okinawa's Naha air field.

Friday, June 8, 1945

In fulfillment of my responsibilities as a member of the Program Committee to prepare recommendations for use for the scientific panel advising the Interim Committee, I prepared and sent to Zinn and Cole a ten-page summary of the chemical aspects of postwar research and industrial development. My proposed program encompasses eight areas (1) general investigation of the heavy elements, (2) pile investigations, (3) isotope separation, (4) military uses, (5) methods of analysis and instruments, (6) use of tracers, (7) applications of fissionable material to useful atomic power and other purposes, (8) fundamental chemistry.

With regard to (1), I propose preparation of all conceivable heavy isotopes by bombarding the following elements with positive ions such as protons, deuterons, etc.: Th^{230}, Th^{232}, Pa^{231}, U^{233}, U^{234}, U^{235}, U^{236}, U^{238}, Pu^{239}, Pu^{240}, 95^{241}, etc. I also suggest preparation of all conceivable heavy isotopes by intense neutron bombardment in piles of such elements as Pu^{239}, Pu^{240}, Pu^{241}, Np^{237}, 95^{241}, 96^{241}, 96^{242}. I call for determination of nuclear properties of individual isotopes and the basic chemical and physical properties of elements 89 to 96. I also ask for the development of natural sources--new and more efficient means for extraction of uranium and thorium from ores, the recovery of radium, protactinium, ionium, actinium, mesothorium, etc., from ores, and a search for new isotopes of transuranic elements in nature. Under "pile investigations," I cover construction and operation of piles, converter piles, breeder piles, and other new piles. Under "isotope separation," I propose chemical development connected with the electromagnetic method, the diffusion method, and the thermal diffusion method. I also suggest the isotopic separation of elements other than uranium using the electromagnetic method, and propose the investigation of new methods for the separation of U^{235} including solvent extraction and adsorption methods, competing chemical reactions, fractional electrolysis, and centrifuge methods.

I cover development work on the present type of device, the use of light elements, and the use of fission products under the heading "military uses."

Under "fundamental chemistry" I discuss solvent extraction, radiation chemistry, and photochemistry, less known elements of importance to atomic power, mechanism of co-precipitation, and natural radioactive families.

Manning and I sent a memo to Hogness on salary changes and reclassifications of academic personnel. It lists the academic personnel of Section C-I in three categories: (1) those remaining in Section C-I after July, (2) those leaving Section C-I after July 1 but remaining with the Project in Chicago, and (3) those leaving the Chicago Project July 1. We made no recommendations for those men whose salary is $5,000 or more.

Jones requested of Stearns about 5 g of plutonium from a large shipment of intermediate level product received recently by the Project Office in Hanford and an equal amount of higher gt material. This first material is to be used to replace the main portion of our present working stock of very low gt material produced at Clinton (which is actually irreplaceable).

I have had conversations with several of the men as a result of our discussion at Wednesday's Section C-I Council meeting about the future of the Project. As an example, today I received a memo from Roy Greenlee who, in expressing concern about an arms race, says, "There exists, however, one thin ray of hope in the possibility of isolation and control by the International Organization of the sources of raw material, supply, and careful surveillance of a certain type of industrial activity."

The top headline today reads "Reds Give Up Fight on Veto;" Russia has surrendered in the veto controversy that has stalled progress of the world security conference for nearly three weeks.

Saturday, June 9, 1945

T. J. La Chapelle transferred to the Mound Laboratory in Dayton, Ohio, today.

I read a memo (MUC-GTS-1739) dated June 8 from Davidson about the existence of solid $PuCl_4$. He refers to Dr. Zachariasen's report that he (Zachariasen) has identified the hitherto unknown substance $PuCl_4$ on the basis of its x-ray diffraction pattern. Davidson reports that the chemical evidence indicates that the diffraction pattern Zachariasen observed is primarily UCl_4. While Davidson is certain that the tetrahalide phase is not primarily $PuCl_4$, he is not absolutely certain that a small amount of $PuCl_4$ is not isomorphously incorporated in the UCl_4 lattice.

Davidson concludes by saying that this is not the first occasion in which Zachariasen has dogmatically and prematurely announced as certain, results which are highly questionable; neither the personal estimate of the work of other scientists nor the expression by Zachariasen of his personal satisfaction in the (premature) announcement of a new result is in accordance with the traditional mode of expression in scientific communication.

Allison wired me clarifying the types of samples Segrè wants. He wants two aliquots from the CW-2 plutonium sample derived from Farmer's Special No. 2 (Met Lab sample 49NG). One sample is to be taken from the undecontaminated solution of the bulk of the yield and one from the washings in which some aluminum is dissolved in the attempt to get all the plutonium.

Manning completed recommendations for promotions for the following men: Ralph James from Junior Chemist to Associate Chemist with an increase in salary ($265 to $300 per month); Sigfred Peterson from Junior Chemist to Associate Chemist with a salary increase (from $275 to $300 per month); Herman Robinson from Associate Chemist to Chemist with no increase in salary. Hogness signed these requests.

Fighting continues on Okinawa while superforts attack aircraft factories in Nagoya, Naruo, and Akashi.

Sunday, June 10, 1945

I played 18 holes of golf with Helen and Herman Robinson at Westgate Valley Golf Club (Ridgeland Road at 131st Street, Worth, Illinois). Herman scored 125, and I scored 109.

The top headline in today's Chicago Sun reads "Carriers Blast Japan Again, Last Okinawa Line Cracks." Admiral Halsey caught the Japanese off guard.

Monday, June 11, 1945

The report of the Committee on Political and Social Problems [Franck (chairman), Hughes, Nickson, Rabinowitch, Seaborg, Stearns, and Szilard] is being issued today. It consists of I. Preamble, II. Prospects of Armament Race, III. Prospects of Agreement, and IV. Methods of International Control. I believe that it is a significant report, and I quote the Summary:

> The development of nuclear power not only constitutes an important addition to the technological and military power of the United States but also creates grave political and economic problems for the future of this country.
>
> Nuclear bombs cannot possibly remain a "secret weapon" at the exclusive disposal of this country for more than a few years. The scientific facts on which their construction is based are well known to scientists of other countries. Unless an effective international control of nuclear explosives is instituted, a race for nuclear armaments is certain to ensue following the first revelation

of our possession of nuclear weapons to the world. Within ten years other countries may have bombs, each of which, weighing less than a ton, could destroy an urban area of more than ten square miles. In the war to which such such an armaments race is likely to lead, the United States, with its agglomeration of population and industry in comparatively few metropolitan districts, will be at a disadvantage compared to nations whose population and industry are scattered over large areas.

We believe that these considerations make the use of nuclear bombs for an early unannounced attack on Japan inadvisable. If the United States were to be the first to release this new means of indiscriminate destruction upon mankind, she would sacrifice public support throughout the world, precipitate the race for armaments, and prejudice the possibility of reaching an international agreement on the future control of such weapons.

Much more favorable conditions for the eventual achievement of such an agreement could be created if nuclear bombs were first revealed to the world by a demonstration in an appropriately selected uninhabited area.

In case chances for the establishment of an effective international control of nuclear weapons should have to be considered slight at the present time, then not only the use of these weapons against Japan, but even their early demonstration, may be contrary to the interests of this country. A postponement of such a demonstration will have in this case the advantage of delaying the beginning of the nuclear armaments race as long as possible. If, during the time gained, ample support can be made available for further development of the field in this country, the postponement will substantially increase the lead which we have established during the present war, and our position in an armament race or in any later attempt at international agreement would thus be strengthened.

On the other hand, if no adequate public support for the development of nucleonics will be available without a demonstration, the postponement of the latter may be deemed inadvisable, because enough information might leak out to cause other nations to start the armament race, in which we would then be at a disadvantage. There is also the possibility that the distrust of other nations may be aroused if they know that we are conducting a development under cover of secrecy, and that this will make it more difficult eventually to reach an agreement with them.

If the government should decide in favor of an early demonstration of nuclear weapons, it will then have the possibility of taking into account the public opinion of this country and of the other nations before deciding whether these weapons should be used in the war against Japan. In this way, other nations may assume a share of responsibility for such a fateful decision.

To sum up, we urge that the use of nuclear bombs in this war be considered as a problem of long-range national policy rather than of military expediency, and that this policy be directed primarily to the achievement of an agreement permitting an effective international control of the means of nuclear warfare.

The vital importance of such a control for our country is obvious from the fact that the only effective alternative method of protecting this country appears to be a dispersal of our major cities and essential industries.

I read a copy of a letter from Hilberry to Wayne Johnson dated June 9. He enclosed the rating sheet for Isadore Perlman requesting the maximum merit increase, thus raising his salary from $450 to $500 per month. Hilberry stated that I would send a supporting memo of Perlman's research ability. He went on to say that as far as his own contacts with Perlman are concerned, he cannot recommend him too highly. Hilberry stated Perlman has worked with him at Hanford since last fall under extremely difficult conditions and has done an excellent job; he has not only carried through the necessary liaison tasks smoothly and effectively, but he also has made a very real contribution to the actual scientific problems faced at Hanford.

In my letter to Wayne Johnson supporting Hilberry's request for the maximum increase for Perlman and giving my rating of his research, I state that I rate him as the best all-around man whom I have had associated with my chemistry section at Chicago. He is a highly original and an ingenious experimenter, a very rapid worker who displays the best of judgment in the planning and interpretation of his experiments. I say further that if I were given the choice to have only one man assist me in my experiments I would choose Mr. Perlman. I point out that Perlman has had a unique role in the development of the separation processes in that he has participated in the decisive development at all three sites; and that at Chicago, Clinton, and Hanford he has made decisive, crucial contributions to the conception, laboratory development, intermediate stage development, and production stage of operation of the separation processes.

Warren Johnson wrote a highly supportive letter to Compton about Perlman's qualifications; this was dated May 10.

Thetford will have to request Wayne Johnson to pay Ruth Rogers time and one half for the extra 0.4 hour she worked at my request today.

In response to a query of Spedding, Manning said our present situation with the control analysis program for Mallinckrodt is such that we cannot plan to take on any additional men. He indicated that if there should be any change of plans in the immediate future that would make it possible for us to take on additional people, he will let Spedding know.

Fighting is heavy in Okinawa, according to today's paper, and it is reported that Australian troops have landed on North Borneo.

Tuesday, June 12, 1945

Today Arthur Compton transmitted to Secretary of War, Henry Stimson, (attention George Harrison) the report of the Committee on Political and Social Problems. Compton states that "The main point of this memorandum is the predominating importance of considering the use of nuclear bombs as a problem of long-range policy rather than for its military advantage in this war. Their use should thus be directed primarily toward bringing about some international control of the means of international warfare." He then says that we suggest that a technical but not military demonstration be made in order for the United States to be in a position to support the outlawing of the military use of atomic weapons.

Morgan has found that the element 95 fraction he has isolated from target 49DE (100 mg plutonium + 20 Mev deuteron Berkeley bombardment) has decayed sufficiently to count the entire sample in a Geiger counter--he had measured the electromagnetic radiations on an aliquot of the sample on April 6. He now finds four gamma and x-ray components: 635 kev, 87 kev, 38 kev, and 12-15 kev. Morgan observes that this selection of energies seems to be quite different from those obtained from the aliquot measured on April 6. He concludes that the latest results are probably more reliable because of the greater statistical accuracy from the higher counting sample.

Manning sent Brown a list of the suggested subjects for basic collected papers for Volume 16 of the Metallurgical Project Record. Authors suggested for the various papers were James, Miller, Turk, Knox or Halperin, Cunningham, Willard, Thompson, Perlman or Kohman, Brown, Dam, and Koshland.

The Japanese commander on Okinawa is being asked to surrender unconditionally, according to today's headline.

Prime Minister Mackenzie King of Canada has won a majority in Parliament.

Wednesday, June 13, 1945

The Council of Section C-I met at 8:30 a.m. in my office. Attending were Albaugh, Cunningham, Davidson, Egan, Ghiorso, Gilbreath, Hindman, Jaffey, Jones, Katzin, Lawroski, Manning, Seaborg, Simpson, Stewart, and R. C. Thompson. I mentioned that the meetings this month will be of a different type, that we will not report work, as has been done in the past two years. We still will need a monthly abstract for the Division report. Albaugh indicated he probably would not have a regular monthly report, but Katzin and Cunningham said that they will.

There was a discussion about reporting after July 1. I indicated that all future reports will probably come out as MPR papers. These can be issued as reports immediately. I again mentioned the letters of appraisal. I said that I know it is a distasteful task, but it has to be done.

I discussed with Jaffey the present state of radium analysis. Martin has called from Mallinckrodt and wants to know whether we could take samples soon; he would like five days notice to cut off the Bureau of Standards hook-up. Jaffey said he hopes to have tested a radium solution by tomorrow, but he does not want to promise anything until there had been at least one run. I told him to tell me his status sometime tomorrow.

Lawroski gave a report on the Redox Process. He indicated that most of the equipment for the five smaller columns has been received, and the room is completed with the concrete shield in place.

Gilbreath discussed problems his group has had with the reducing agent needed to reduce plutonium to the +3 state. They had planned to use hydroquinone and hydrazine but this combination was found to decompose. They also tried ferrous ion and urea unsuccessfully. They are now trying ferrous-hydroxylamine and ferrous-hydrazine.

I next reviewed the writing in the Survey Volume 14 and went over the status of the collected papers.

Katzin brought up a question about the proposed licensing of chemists in Illinois. The licensing requirements are rather severe. There was the general feeling that there seems to be some ulterior motive in passing the bill. I promised to look into the matter so we can consider further action.

I received a report dated June 11 from Jaffey about his visit to the Mallinckrodt plant on June 4. He made the visit to learn the essential problems involved in their uranium extraction process. After reviewing the extraction process used, Jaffey indicates that one of the chief problems is to develop a rapid analytical method for determining the amount of radium in either precipitates or filtrates in order to run a large number of analyses on short order for the St. Louis people. Jaffey believes that a procedure similar to a

Bureau of Standards procedure should be developed immediately for the rapid analysis necessary for the pilot plant. He thinks that it might be possible in the long-range work to develop a simple procedure involving a separation of radium and simple alpha-counting; the pulse analyzer may be useful in determining that radium is the only alpha-emitter present in the final precipitate.

I sent Ernest Lawrence at Berkeley my opinions and suggestions on the course to be taken for nuclear weapons in the immediate future and on the question of the postwar future for nucleonics. I indicate that the opinions I express are shared almost unanimously by the people associated with me in Section C-I of the Chemistry Division here. My major points are:

(1) The basic facts concerning the successful release of nuclear energy and its immense destructive possibilities should be made public very soon, both to the general public and to scientific channels. I propose withholding only information with respect to the actual detailed designs of the major manufacturing installations.

(2) With respect to use in the present war, we propose not to use the weapon on Japan without warning but to demonstrate the weapon in the presence of all leading countries including Japan.

(3) On postwar control, I indicate we favor free research in nucleonics throughout the world and control through an international organization. Probably the best method of control lies in the control of the raw materials. Perhaps the only method of maintaining these controls would involve world-wide pooling to form a stockpile of fissionable material to be used by the international organization for policing purposes. I mention that, as suggested by Szilard, perhaps control could be effected by denaturization, i.e., by mixing it with suitable isotopes to spoil its use for explosive purposes without interfering too much with its use for research purposes such as power pile developments.

(4) With respect to the organization of postwar research in nucleonics in this country, I state that it would be a good idea to establish, with government aid, about four large research laboratories at four of the major universities. These laboratories should form a sort of a foundation for the country's research program and should include men who are able and willing to advise outlying laboratories as to the research program. The outlying laboratories might consist of government laboratories working on the more practical aspects of the field, and also regular university and industrial laboratories supported by government contracts or grants-in-aid.

I wrote a thank-you note to Captain McKinley of the Area Engineer's Office for Bureau of Standards reports on radium determination which he has forwarded to me. In addition I ask him to procure two Rochester reports on this subject. On another matter I ask him to follow up on Hewett's request of last October for information on determining small amounts of uranium and thorium in rocks. An answer to Hewett's request has been delayed because of the secrecy aspects.

The Basic Chemistry, Recovery, and Instrument Groups met at 7:45 p.m. in Room 209, Eckhart Hall. Present were Asprey, Cunningham, Dixon, Egan, Florin, Fred, Ghiorso, Hagemann, Hindman, Hyde, Jaffey, James, Jones, Katzin, Manning, McLane, Morgan, Nickson, Peterson, Phipps, Robinson, Seaborg, Seifert, Simpson, Templeton, R. Thompson, S. Thompson, Van Winkle, Westrum, and others. I opened the meeting, then turned it over to Cunningham. Simpson spoke first on effusion measurements and electrical conductivity calculated from optical reflectivity. He described the method of making effusion measurements that involves placing the sample in a tantalum container with an orifice so small that the rate of effusion is small compared with the rate of evaporation. The vapor pressure is related to the size of sample collected and is determined by the diameter of the orifice, the size of the collimater (if used), and the distance of the collecting plate from the orifice. Simpson then went through the calculations involved using the hypothetical mixture PuO_2, PuO. I asked if the effusion method had been used before for obtaining thermodynamic data. Simpson replied that it had not to his knowledge, although some English workers have determined the vapor pressures of some metals by this method. Simpson also described the measurement of the optical reflectivity of a plutonium mirror and the calculation of the electrical conductivity of the massive metal from this measurement. The mirror had been formed on the inside of a pyrex tube comprising part of a vacuum system.

The next speaker was Westrum who has constructed an apparatus for measuring heats of solution of thorium and plutonium compounds. The calorimeter is in a brass submarine container immersed in a thermostat controlled within ~ 0.001°C. The calorimeter consists of a 200 cc dewar flask in which is mounted a platinum stirrer supported by a glass rod. Temperature measurements are made with a copper resistance thermometer in a thin platinum envelope. Westrum then described the determination of the heat of solution of $PuCl_3$, prepared by sublimation in a straight quartz tube with a closed end. The sublimed salt collected in three zones, the first two of which were green in color and the third, which was farthest from the heated end of the tube, was tan to brown. This latter portion has been variously identified as anything from stopcock grease to $PuCl_4$. The results of two determinations showed heats of solution of 22.54 and 22.14 kcal/mole.

Peterson then talked about the work McLane and he have done on actinium. He indicated that their emphasis has been on the separation of actinium from lanthanum. The most promising method found so far is the use of IR-1 (cation exchange) resin. He briefly described the previous work with this resin and indicated that Morgan and Stan Thompson are now investigating the possibility of using this method for separating elements 95 and 96 from lanthanum. Peterson then described the experiments that involve the adsorption of lanthanum and actinium tracer on the column and elution with a citrate buffer. He found that, although lanthanum was supposed to be the least readily eluted of all the rare earths, actinium is even less readily eluted. I asked how much activity was used in this experiment, and Peterson replied that the total was about 60,000 c/m. Cunningham then adjourned the meeting.

The deadlock over the reorganization of Poland was broken today with the announcement that representatives from the Polish provisional government will meet with representatives of the "Big Three" (Russia, Great Britain, and the United States).

Thursday, June 14, 1945

The Heavy Isotopes Group met in my office at 8:30 a.m.; the meeting was attended by Britain, Cunningham, Florin, Ghiorso, Hagemann, Hindman, Hyde, Jaffey, James, Jones, Katzin, Magnusson, Manning, McLane, Morgan, O'Connor, Peterson, Scott, Studier, R. C. Thompson, S. G. Thompson, Van Winkle, and Weissbourd. I discussed some general matters, including the present uranium plus helium ions bombardment, the handling of Farmer's No. 2 (Pu^{239} and U^{235} irradiated in Hanford pile), and the work on the actinium series.

Florin reviewed the work on the last uranium plus 40 Mev helium ions target (sample TAC) and presented the following table which shows what has been found so far in each of the four layers machined off the target:

	thick.	wt.	Mev	39/38	rel. 93/g	β c/m/g
I	.0605	.755	40	12.5	1	$2.2 \cdot 10^8$
II	.0248	.333	36.5		.68	$7 \cdot 10^7$
III	.0705	1.215	34.8		.23	$1.9 \cdot 10^7$
IV	.128	1.720	27.8		.064	$6.3 \cdot 10^6$

α %	3.7 cm	4.1 cm	4.3 cm	total α c/m/ gm U/1000 μahr.
I	11.7	59.5	28.8	170,000
II	18	51	31	86,500
III	15	68	17	18,600
IV	23	58	19	5,400

I 5,150 c/m 48 expected from 38

153 c/m 49 expected from 39

Total 21,000 c/m α found

James received 20 mg of about 180 gt plutonium from Stewart. He will look for U^{237} and then milk out the 95^{241} in order to determine the half-life of Pu^{241}.

In the evening I played twelve holes of golf at Jackson Park with Tom Morgan before dark. (TM-50, GS-45 for nine; TM-68, GS-63 for twelve.)

An announcement was made today that there will be a Big Three meeting in Europe, sometime before July 17, which will be concerned with the peace conference.

Friday, June 15, 1945

Today was Jerome Howland's last day at the Met Lab. He is transferring to the Mound Laboratory in Dayton.

U.S. troops continue to move in Okinawa, and B-29s hit Osaka again, according to today's paper.

Saturday, June 16, 1945

I received and read the final report dated June 11 of the Research Program Committee (Daniels, Wigner, Seaborg, Jacobson, Cole, Howe, Zinn). The introduction reads:

> The field of nucleonics has reached the stage where it is fairly clear as to what should be the next immediate steps. The section of this report dealing with the social and political problems indicates several choices which face the nation concerning the future of this field. It is believed that irrespective of the choice made and irrespective of whether

or not the point of view is one which envisages an armament race or is one which happily contemplates a peaceful development, the immediate problem is unaltered. The time scale, however, on which the work is carried out will be very much affected by the realities of international policies.

The quantity production of fissionable isotopes is now realized only at the expense of a raw material which is not at all abundant in our country and exists in rather limited quantities in the world as a whole. At the present rate of destruction of this material it cannot be imagined that a wide-spread use of fissionable isotope will be permitted. The wasteful utilization of this raw material is forced upon us by the necessities of war, but the advisability of continuing present methods of production far into the future even for military needs can be doubted. It follows that research directed towards a more efficient utilization of the primary material is the main problem of the nucleonics program.

If a sizeable percentage of the isotope uranium-238 could be burned and if, in addition, useful fissionable isotope could be realized from thorium, then the whole future of the development would be on much more secure grounds. It is of paramount interest, therefore, that the fundamental research necessary to the design and construction of "breeder" piles be undertaken vigorously. It is only by demonstrating the practicability of the "breeder" principle that a sufficiently ample supply of fissionable material can be produced to permit the nucleonics program to proceed on the scale indicated by the benefits to be derived. It is also necessary that research and development leading to the useful utilization of the power from nuclear burning be vigorously supported. The ultimate goal here would be to operate machines which produce useful power and which at the same time produce as much as or more fissionable isotope than is consumed. Under these circumstances no real limit could exist to the application of nucleonics to all phases of our scientific and industrial life.

The accomplishment of the goal set in the previous paragraph is not possible without an extensive program for basic research in physics, chemistry, and metallurgy. The present state of the art is such that new designs of reactor units invariably suggest situations about which no information is available. For the long term development of the field the research in the basic sciences will be of more importance than any immediate pile design or engineering. It is possible that the real future of atomic power does not involve the burning of uranium but rather other elements at the light end of the periodic table. Progress in this direction can only come if all possible freedom and support is given for investigations concerning the nucleus.

In the following paragraphs are indicated some of the immediate questions which should be investigated. The problems listed are meant to illustrate the type of work required for a sound development of the field and in no way can be considered a program for anything but a beginning. New information and new discoveries will rapidly alter any planned program of research.

For the purpose of preparing this report the subject matter has been divided into the fields, physics, chemistry, metallurgy, biology, medicine, health protection, and pile development. Experience has shown, however, that this division is artificial and that no sharp boundaries exist. Inasmuch as many of the benefits to be derived from the nucleonics program result from the application of the radioactivity and radioactive tracers to the fields of biology, medicine, chemistry, engineering, etc., there is also included a partial program of studies which should be continued and initiated in these fields. The auspices and arrangements under which this work might be carried out may be somewhat different than the nuclear program itself, but it is no less important that these studies be given full support in order that the new discoveries may become a boon to the public as rapidly as possible.

The report covers: I. Pile Development (Breeder Piles and Power Piles); II. Physics Research (Nuclear Reactions, Reaction Cross Section, Fission, New Elements, Nuclear Structure, Radiation Physics, Pile Physics, Theoretical Physics, Classical Physics, Nucleonics Standards, Instruments, Separation of Uranium Isotopes, Separation of Other Isotopes, Military Weapons); III. Chemistry Research and Industrial Development (General Investigation of the Heavy Elements, Basic Chemical and Physical Properties of Elements 89-96, Development of Natural Sources, Chemical Problems of Pile Development, Fundamental Chemistry, Methods of Analysis and Instruments, Use of Tracers, Isotope Separation, Military Uses); IV. Program of Research and Development in Metallurgy (Pure Metals, Alloys of Special Materials, Properties of Metals and Alloys, Liquid Metal Coolants, Oxide Systems, Effect of Fission upon Properties, Metallographic and Testing Techniques, Use of Radioactive and Radiation Techniques in Metallurgy, Mining and Metallurgy, Metallurgical Handling Problems); V. Biology and Medicine (Uses of Biology in Nucleonics, Use of Nucleonics in Biology); VI. Health Protection; VII. Industrial Program.

Mary Williams, secretary to T. O. Jones, left the Met Lab. She will work in Kimpton's office at the University of Chicago.

President Truman will go to Berlin for the Big Three meeting, but the date has not yet been announced.

Sunday, June 17, 1945

I played 18 holes of golf at Oak Hills Golf Club (76th Ave. and 131st Street, Palos Park) with Stan Thompson, Steve Lawroski, and Foster York (GTS and FY won a "low ball plus low total" match, 1 up. ST-103, SL-109, GS-100, FY-102.)

Two Polish leaders arrived in Moscow yesterday, according to today's newspaper, to discuss setting up a new government in Poland.

Monday, June 18, 1945

James has completed the isolation of a possible uranium fraction from the 20 mg of 180 gt plutonium he started working up on June 14. He finds beta-particle activity and concludes that if it decays at all rapidly, it is undoubtedly U^{237}. James has also been working on the isolation or "milking" of the 95^{241} from this batch of plutonium.

Jones sent a memo to Hilberry requesting the loan of 2 grams of U^{235} from the special sample product U^{235} fluoride that was received by the Project Office from Y-12 on June 1. He explains that the material will be used to prepare several targets for special alpha and deuteron bombardments on the 60-inch cyclotron at Berkeley.

E. O. Lawrence is in town. I met with him during the morning to discuss the contents of my letter of June 13 which had not yet arrived in Berkeley when he departed on his latest trip. The letter contains my suggestions on the course of action to be taken for the control of nuclear weapons in the near future and the postwar future for nucleonics.

At 7:45 p.m. there was a Chemistry Seminar in Room 251, Ryerson Laboratory. Milton Burton spoke on radiation chemistry.

U.S. troops are moving on Okinawa still; and four cities were blasted with incendiary bombs, catching the cities by surprise.

Tuesday, June 19, 1945

My sister Jeanette Hendricks arrived by train from Los Angeles to visit us. She took a taxi from the station to our home.

At 9:00 a.m. I attended a Chicago conference on new piles. The following talks were given: (1) Eugene Wigner on the fast neutron uranium-plutonium breeder, (2) Nordheim on the heavy water-thorium-U^{233} breeder, (3) Weinberg on resonance neutron uranium-Pu^{239} breeder, moderated by such coolants as fluorocarbons, (4) Fermi on the uranium-Pu^{239} fast breeder system, (5) Soodak on the uranium-Pu^{239} fast breeder system, (6) Daniels on a version of his high temperature pile.

In the afternoon H. S. Brown talked about the chemical aspects of homogenous piles, and M. C. Leverett discussed the engineering aspects of such piles.

Before dark I played 13 holes of golf at Jackson Park with Stan Thompson. (ST-55, GS-47 for nine; ST-76, GS-67 for 13.)

Admiral Nimitz announced the capture of Okinawa in effect but did not say it was secured as yet.

Wednesday, June 20, 1945

In the morning I attended the last session of the Chicago conference on new piles. This session covered heterogeneous piles. From 11:00 a.m. to 11:45 a.m. I talked about chemical separations processes for such piles. I discussed the necessity for high yields in the separations processing and indicated that hope lies in solvent extraction. I reviewed Calvin's TFA process and the Redox Process.

At 7:45 p.m. I attended a meeting in Room 209, Eckhart Hall, of the Separation Processes Sub-section. Others present were Ader, Albaugh, Blaedel, Egan, Gilbreath, Hagemann, Hindman, Hyman, Katzin, Lawroski, Manning, Perlman, Peterson, Schaffner, Schraidt, S. Thompson, Van Winkle, and Walling. The first speaker was Egan who discussed the two attempts to test the Redox Process in the 3-inch extraction column. Difficulty has been encountered with the reducing agent employed in the second column. Both hydroquinone and ferrous-urea mixtures were found to decompose. Egan also described the status of the five 1-inch columns being constructed as a pilot plant for the Redox Process. The first cycle columns have been mounted. Because these two columns have the highest activity levels, they are housed in a cell having 12-inch thick concrete walls. As an additional safety measure a blower has been installed to exhaust fumes from a cellulose acetate shield that will surround these two columns. He hopes they will be ready for use by July 15.

Blaedel spoke on Redox Process research. Work of late has been devoted almost exclusively to a search for a satisfactory reducing agent. Somewhat encouraging results have been obtained in column tests with ferrous ion and hydrazine mixtures. They plan to evaluate both hydrazine and hydroxylamine alone as reducing agents, even though batch evidence does not indicate great promise for either of them.

President Truman flew to Olympia, Washington, today from the nation's capital--the first cross-country plane trip ever made by a President--in 12 hours and 18 minutes.

There was a Project Council Policy Meeting at 2:05 p.m. in Room 209, Eckhart Hall, attended by Allison, Bartky, Chapman, Chipman, Compton, Daniels, Dempster, Franck, Fermi, Hamilton, Harrell, Hilberry, Hogness, Howe, Jacobson, Johnson, Latimer, Leverett, McKinley, Mulliken, Murphy, Nordheim, Spedding, Stearns, Stone, Tracy, C. J. Watson, W. W. Watson, Wigner, Wirth, and Zinn. Compton announced that an "Interim Committee" has been appointed within the last few days as an advisory committee to the Secretary of War. He explained that in addition to the committee there are a series of panels, and that on the scientific panel are Fermi, Oppenheimer, Lawrence, and himself. The function of this panel is to advise the committee from the technical or scientific point of view. In answer to a question from Spedding, Compton indicated that the names of the Interim Committee members are secret. In answer to another question from Spedding, Compton indicated it is up to the Cabinet and Congress to decide whether the Manhattan District will continue until V-J Day.

Compton then discussed changes in the Met Lab Project, mentioning that Clinton will operate under Monsanto as of July 1, with the Monsanto representative in charge being C. A. Thomas. Whitaker is to remain the Director of the Laboratory, and the staff insofar as possible will remain unchanged. Compton also mentioned that Stearns has asked to be released on the basis that he has completed the assignment he was brought in for, and that he, Compton, has been asked by General Groves to take over the directorship of the laboratory, a matter which is still under consideration.

Compton then took up the matter of the coordination of the program. It has been decided to hold monthly meetings at Chicago one month and the next month at Clinton in order to reduce the number of people who have to travel. The next meetings will be on Tuesday and Wednesday, July 17 and 18, with planning sessions on the morning of July 17 and a policy meeting in the afternoon on July 17. The meetings on Wednesday, July 18 will be Information Meetings.

Mulliken reported on the status of the Metallurgical Project Record. About one-half of the overall job is done; two of the Chemistry volumes are expected to be done by July 1. Allison mentioned that at Site Y there is a historian who is writing a record of the factors that influenced the changes in program from time to time and asked if anything like that is being considered in Mulliken's write-up. Mulliken indicated that they have written a brief history in the introduction but have not gone into it in a very detailed manner. Compton observed that at present there are two histories that have already been prepared. One, by Smyth for the General, and one for the Manhattan Project.

The meeting adjourned at 4:15 p.m.

Thursday, June 21, 1945

At 8:30 a.m. I held a meeting in my office of the Heavy Isotopes Group, which was attended by Britain, Cunningham, Florin, Ghiorso, Hagemann, Hindman, Hyde, Jaffey, James, Jones, Katzin, Magnusson, Manning, McLane, Morgan, O'Connor, Perlman, Peterson, Scott, Studier, R. C. Thompson, S. G. Thompson, Van Winkle, and Weissbourd. I reviewed Hamilton's schedule for the Berkeley 60-inch cyclotron bombardments.

Cunningham and Britain discussed the status of the work on Farmer's No. 2 samples (Hanford-irradiated plutonium and U^{235}), and Ghiorso reported on his fission measurements at Site A.

Ghiorso carried out an alpha pulse analysis on the 95^{241} James has isolated from the 20 mg of 180 gt plutonium. He reported to James that the sample (51D-11) had 12,000 c/m of 51 (±25 percent).

R. C. Thompson proposed to me, in a memo dated June 21, the term "hoypaloy," code name for protactinium. He goes on to state:

> This name continues the suffix tradition set by tuballoy and Myrna Loy (or, if you prefer, myrnalloy). In addition, it has the subtle distinction of incorporating as its middle, unaccented syllable the conventional chemical symbol for protactinium. As a symbol for the element, Hp would suggest itself. The confusion which might arise between this symbol and the abbreviation for horsepower should add considerably to its desirability.
>
> I trust that this memo will be given the attention which it deserves.

In a letter to Hamilton I give him, as he requested, information useful for comparing the relative merits of the cyclotron and chain-reacting pile as methods for the manufacture of radioactive isotopes for tracer purposes. I indicate that the following radioactive isotopes are produced by transmutation in cyclotron bombardment but are not produced by any primary neutron reaction: Be^{7}, V^{48}, V^{49}, Mn^{52}, Co^{56}, Co^{57}, Ga^{67}, Y^{86}, Cb^{91}, I^{124}, and Bi^{207}. I further indicate that the following radioactive isotopes are produced in rather small yield by the n,2n reaction in pile bombardment: Na^{22}, A^{37}, Sc^{44}, Mn^{54}, Fe^{55}, Co^{58}, Ge^{69}, As^{74}, Y^{88}, $Cd^{107,109}$, Re^{184}, and Tl^{202}, although we have only estimates for the cross sections for the production of these isotopes.

Manning sent specifications to Jesse for the radiation alarm instruments to be used in the hot laboratory now being constructed in Room 12.

Hogness made an announcement about security to some of the staff. Hogness feels that we should be extremely careful about keeping quiet outside the Lab, as there is evidence that information has been pieced together about the existence of product and its intended use.

In the evening I played 13 holes of golf in Jackson Park with Manning and Lawroski. We had to stop at 9:20 because of darkness. (WM-54, SL-53, GS-42 for nine; WM-78, SL-71, GS-62 for 13.)

Today's newspaper reports that the San Francisco conference will end next Thursday, June 26. President Truman will address the final plenary session.

Friday, June 22, 1945

The Council of Section C-I met at 8:30 a.m. in my office. The meeting was attended by Albaugh, Cunningham, Davidson, Egan, Ghiorso, Gilbreath, Hindman, Jaffey, Jones, Katzin, Lawroski, Manning, Perlman, Seaborg, Simpson, and R. Thompson. I mentioned that Stoughton will be here from June 25 to June 29 and probably will devote most of his time to Volume 17 of the Metallurgical Project Record and writing problems.

I then asked Perlman, Manning, Cunningham, and Hindman to meet with Nickson at 3:00 p.m. on Monday to discuss the Health Division's method of analyzing urine for plutonium.

I reiterated Hogness' notice about security, and I urged the men not to discuss their work outside the building. I then said that we probably should not have any more Wednesday night meetings during the summer until September; there will be just one more this month. I asked for opinions about the time for the Chemistry Division meetings next fall; Hogness has stated a preference for afternoon meetings. The consensus of the Council is that if our Wednesday night meetings occur only once every two weeks and if the Chemistry Division meetings occur at the same intervals, it may be feasible to have one evening meeting a week.

I noted again that the letters of appraisal are coming in very slowly and urged speeding them up as they have to be in before July 1.

There was a general discussion on whether the present schedule of daily cleaning of the laboratory should be continued. It was decided that Hindman, Manning, Thetford, and Slattery should get together to try to modify the present cleaning procedures without reducing it too much.

At the conclusion of the meeting I mentioned that after July 1, there will be no more group leaders although this may have to be modified somewhat to take care of the signing of passes and seeing that special jobs like the building of a hot lab are completed.

James re-counted the "uranium fraction" sample he has isolated from 20 mg of 180 gt plutonium on June 18. He finds the decay rate corresponds to a half-life of 8.7 days. This is close to 7-day U^{237}. His conclusions are that 94^{241} decays by alpha emission as well as by beta emission. He calculates the branching ratio α/β to be <0.036 percent.

Katzin sent me a memo dated June 22, in which he refers to Roy Thompson's important contribution of June 21 on the nomenclature of protactinium. He indicates that he has some minor points of disagreement with the suggestion. In particular, the term "hoypaloy" bears a phonetic resemblance to the Greek phrase hoipolloi, and there may be serious objection to the use of this foreign phrase of suspicious origin and definitely subversive connotation. He goes on to propose the adoption of the code name "hypaloy," stemming from the roots hypo and alloy. He points out that derivation of the former is from the meaning "less than the ordinary" and comments that he is sure that all who have had the experience of working with it will agree that there is less of it than anything. He closes his memo by saying that he can do no better than to quote his distinguished colleague and hopes that this memo will be given the attention it deserves.

In a memo to Wayne Johnson, Hogness recommends a merit increase of $25.00 per month be given Mr. Glenn T. Seaborg as of July 1, raising his salary from $7,200 to $7,500 a year. He goes on to state:

> If it were not for the restriction in the total amount of increase granted to the Chemistry Division, I should propose a greater increase than this since I believe that Mr. Seaborg is by far the most important man in the whole division; in fact, he is the kingpin of the whole division. I am sure that most people who are acquainted with the work on this project are aware that if it had not been for Mr. Seaborg's work before this war there should not have been any Metallurgical Project. In this respect, he is in the same category with Mr. Fermi. I should be pleased if you could find some way of making a further increase to the amount which I am hereby recommending.

In another memo Hogness asks Branch to find some means of giving Don Stewart the signature authorization of a group leader. The business office has opposed giving such authorizations to SED men, but Stewart's duties are such that this is a great inconvenience for us. Hogness offers to underwrite Stewart if such a statement is necessary.

"Complete Frisco Charter" reads the top headline in today's paper. This comes after eight weeks of difficult negotiations.

Saturday, June 23, 1945

James prepared a sample of two grams of 87 percent enriched U^{235} to send to Hamilton in Berkeley.

I received a June 19 letter from Kennedy at Site Y, stating that Segrè, Wahl, and he are not entirely agreed as to what should be done about the May 18 letter from Sproul which indicates that the Regents of the University do not wish to agree to our proposal regarding the handling of Cases S-52 and S-61. Kennedy says that his feeling, in which he thinks Art concurs, is that if the matter cannot readily be straightened out by correspondence with Sproul, we should ask Lavender to go ahead without the waiver of rights by the University if he wishes. Kennedy also says that if Lavender is not willing to do this, then he would propose we forget the whole thing. He thinks Segrè might be willing to do this provided he could be assured he would not be subject to suit by the University for such action.

Kennedy also indicates that they have studied the memorandum of agreement and explanatory letter received from Lavender and that they feel that most points have been satisfactorily corrected.

Katzin sent Farrington Daniels the Problem Abstracts for the period May 15 through June 15, 1945, for Group 9, Section C-I.

Manning, in a firm memo to Budd Gore, states,

> Failure to provide paper towels in the washrooms of the Filtered Air Section of New Chemistry adds an unjustifiable obstacle to the already difficult program of protecting personnel from product and other radiation hazards. Office workers as well as laboratory workers may accidently acquire contamination. The office workers should not be discouraged from frequent hand washing, nor should they be required to go into laboratories with the attendant contamination hazards when they do wish to wash their hands. Facilities for hand cleaning in the washrooms as well as in the laboratories should also be provided for the laboratory worker.

The newspaper today reports that the Senate will not give a speedy ratification to the United Nations Charter. It is estimated that it will take until mid-September.

It is hot here today reaching into the 90's.

Sunday, June 24, 1945

I played 18 holes of golf at Indian Wood Golf and Country Club (Western Avenue and Sauk Trail) with Perlman, Thompson, and Lawroski. (GTS and SL won "low ball plus low total" match, 7 and 6. IP-122, ST-114, GS-104, SL-99.) Helen and my sister Jeanette were with us, and they played seven holes on the back side. We later had a picnic lunch, including the others who were there.

The top headline in the Chicago Sun today reads "Japs Attack Okinawan Fleet," but Japanese casualties on Okinawa increased.

Many Chicagoans flocked to the lake as the beaches officially opened today.

Monday, June 25, 1945

Thomas E. Phipps terminated work at the Met Lab. He will return to the University of Illinois.

Stoughton is here from Clinton and will stay until June 29.

James received from Britain the plutonium-neptunium fraction from about 15 percent of sample 25NA (from Farmer's special U^{235} sample CW-2, which has been irradiated at Hanford). He intends to look for Np^{237} in order to determine the cross section for the reaction:

$$U^{236}(n,\gamma)U^{237} \xrightarrow{\beta} Np^{237}.$$

At 3:00 p.m. Kohman, Cunningham, Hindman, and Nickson discussed methods of analyzing urine for plutonium.

The President will fly tomorrow to San Francisco from his vacation spot on the Olympic Peninsula to address the United Nations conference.

Tuesday, June 26, 1945

I received a memo from Katzin dated June 23, summarizing the work done for the month of May 15 to June 15, 1945, by Group 9, Section C-I. It contains the following information of interest (1) About 70 kg of carbonate residues have been processed through the semiworks by means of the alkaline MnO_2 procedure with yields approximately as expected. (2) Specific activity determinations have been made on some pure Pa^{231}, and the most reliable determination gives a value for the half-life of about 36,000 years. Pulse analysis shows that the alpha radiation from Pa^{231} consists of two groups, one with a range of 3.23 cm and the other with a range of 3.58 cm. Measurements have been made, using some of the purified protactinium, which indicate the presence of gamma-rays with an energy of about 300 kev, corresponding to values found in the literature. This value also corresponds to the energy difference between the two ranges of alpha particles. (3) The present picture of the radiations of U^{233} is summarized by the following table:

Probable type of radiation	Energy (kev)	α-Disintegrations per minute per GM count, 100% geometry (argon), extrapolated to zero absorber
gamma	305	5×10^5
gamma	85	2×10^5
gamma	40	9×10^4
L-x-rays	17.0 12.5	1.9×10^3 1.1×10^3
M-x-rays	3.25	15
Conversion electrons (?)	-	715

In a memo to Hilberry, Hogness urges that Franck be retained as a consultant in order that he can advise on the problem of the considerable amount of energy stored in the Hanford graphite; the sudden liberation of this energy could lead to disastrous results.

Budd Gore sent out an official notice that W. M. Branch will replace him as Chief Administrative Officer on July 1.

Wednesday, June 27, 1945

The Council of Section C-I met at 8:30 a.m. in my office. Present were Cunningham, Davidson, Egan, Ghiorso, Gilbreath, Hindman, Jaffey, Jones, Katzin, Lawroski, Manning, Perlman, Stewart, Simpson, R. Thompson, and S. G. Thompson. I announced that this is the last meeting of the Council, and then proceeded to discuss some of the plans for after July 1. I indicated that people who have been group leaders will still be able to sign such papers as are authorized on their authorization cards; some will continue to operate like group leaders (Stewart for recovery, Jaffey on the St. Louis work, and Lawroski on the solvent extraction work).

Stewart then discussed the new set-up for product control. The plutonium will be charged to certain people who are "heads" of particular programs since there will no longer be group leaders; Ghiorso's material will be assigned to Britain and Ghiorso; Hindman's material will be assigned to Hindman, James, and O'Connor; Redox material will be assigned to Lawroski; Westrum and Simpson will be on separate accounts. In answer to a question from Lawroski, Stewart indicated that the high gt material will be distributed as soon as the 2 gt material is returned.

I explained that we have to keep records on Section C-I's material. Katzin, who takes care of the U^{233} and thorium, should give Stewart a record of the U^{233}; Hindman will inventory the neptunium; Gilbreath will handle uranium; Roy Thompson will keep track of the protactinium; Stewart will maintain records for the Q material (depleted uranium), U^{235}, and special tracers. I added that Stewart will be authorized to send materials out for bombardment.

I again called for the letters of appraisal and asked about the status of the writing. Davidson said he is starting on the purification material for Volume 17A but will be leaving the lab soon after he starts writing; he has finished everything else.

We discussed arrangements for procuring a small continuous centrifuge from Hanford; this would be a boon to Katzin's protactinium work. Manning announced that the hot lab will be finished before the end of July.

Albaugh, in a memo dated June 26, summarizes the work of Sub-section I (written by Gilbreath and Lawroski) for the period May 15 to June 15, 1945. The report contains the following information (some of this material was discussed at last Wednesday's Sub-section meeting): (1) Reducing agents found satisfactory for the Redox Process on a laboratory scale are Fe(II)-hydrazine, Fe(II)-urea, and Fe(II)-hydroxylamine. A final decision as to which of these is the best reducing agent will be made when they are compared under continuous countercurrent operation. (2) The construction of the five glass columns to be employed in testing the Redox Process with active solutions has been started. It is estimated that all the construction connected with the first three columns, comprising the first cycle of the process and the uranium recovery, will be completed and ready for operation on or before July 15.

I read B. B. Cunningham's summary dated June 27, covering the work of Sub-section II, Section C-I, for the period May 15 to June 15, 1945. The first work reported is the helium ion bombardment of natural uranium. A preliminary investigation has been made of isotopes produced by bombardment of natural uranium metal with 40-42 Mev helium ions for 162 µahr in the Berkeley 60-inch cyclotron. Metal in the target area was removed in four successive shavings, and the distribution of activities determined separately. The results are extremely interesting in that: (1) The ratio of 93^{239} activity to 93^{238} activity in the first layer from this sample is 12.5, whereas in a previous bombardment with 20 Mev deuterons the ratio was found to be 3.5. Hence, some 93^{239} must have arisen from a reaction of uranium with helium ions since all of it could not have come from deuteron contamination of the beam. (2) The amount of 92^{237} produced is of the same order of magnitude as that previously found in a 20 Mev deuteron bombardment of natural uranium; deuteron contamination of the helium ion beam could not therefore account for all the 92^{237} found. It would appear probable that at least part of the 92^{237} was produced by a reaction of uranium with helium ions. (3) Slow neutron fission measurements show that the amount of fission found in a sample of the top layer 94 fraction is about 40 percent of that expected if all the 3.7 cm range alpha activity is attributed to 94^{239}. (4) In order to account for the observations, the following reactions are suggested as possibilities: $U^{238}(\alpha,3n)Pu^{239}$; $U^{238}(\alpha,p2n)Np^{239}$; $U^{238}(\alpha,\alpha n)U^{237}$; $U^{238}(\alpha,2n)Pu^{240}$; $U^{238}(\alpha,p3n)Np^{238}$; $U^{238}(\alpha,4n)Pu^{238}$; $U^{235}(\alpha,n)Pu^{238}$; $U^{235}(\alpha,3n)Pu^{236}$; $U^{235}(\alpha,2n)Pu^{237}$; and $U^{235}(\alpha,p)Np^{238}$. All of these reactions are energetically possible.

Studies of the alpha decay in Pu^{241} were continued with the 150 γ plutonium now available. The evidence now shows that U^{237} grows in this material. This indicates a branching decay of Pu^{241} by alpha-particle emission. The fraction of Pu^{241} which decays by alpha-particle emission is very small.

Three preparations of neptunium metal on the 50-microgram scale were carried out by the vapor phase reduction of NpF_3 with barium. All three preparations gave silvery, malleable globules of neptunium metal in high yield. The pycnometric density was measured at 17.6-17.8. Zachariasen's x-ray examination indicates that neptunium metal has a complicated structure not yet elucidated. Reaction of the metal with hydrogen at atmospheric pressure at slightly above room temperature forms a black, flaky hydride whose formula is somewhere between $NpH_{2.5}$ and $NpH_{3.5}$.

The following sulfate salts of plutonium(IV) have been prepared: $K_4Pu(SO_4)_4\cdot$(1 or 2)H_2O, $Rb_2Pu(SO_4)_3$?, cesium plutonium(IV) sulfate. The low solubility of certain of these sulfates might be used as the basis of a relatively simple non-fluoride oxidation-reduction cycle with thorium or zirconium being used as carrier.

Vapor pressure measurements by the effusion method have been carried out on PuO_2. Thermodynamic constants at 2,000°K are $\Delta F° =$ 83 kcal/mol, $\Delta H° =$ 192 kcal/mol, and $\Delta S° =$ 54.5 cal/mol deg.

The optical reflectivity of a plutonium pyrex second surface mirror deposited on the inside of a pyrex tube by volatilization has been measured and found to be 0.53 at a wave length of 0.665 microns. The Drude expression for the reflectivity of metals gives an electrical resistivity of plutonium of 96 (+12, -6) microhm-cm. This value is in remarkable agreement with the measured value of the resistivity of plutonium in a high temperature form of approximately 110 microhm-cm.

A preliminary value of the heat of solution of sublimed $PuCl_3$ in 6 M HCl to form a solution about 3×10^{-4} M in Pu(III) is 22.1 kcal/mol.

The molar heat of solution of $ThCl_4$ has been measured to be 57.4 kcal in 1 M HCl, 44.2 kcal in 6 M HCl, and 34.9 kcal in 9 M HCl.

Modification of the procedures for the preparation of actinium tracer has resulted in Ac^{228} activity of 99.95 percent purity. Carrying of actinium on lanthanum compounds has been further investigated. It has been found that lanthanum oxalate carries 99 percent of the actinium, whereas carrying by lanthanum phosphate ranges from 42 percent to 81 percent. Separation of actinium from lanthanum through the use of IR-1 resin has been attempted with moderate success.

Zachariasen, in a June 26 memo to Stearns, retracts his results on $PuCl_4$ which he reported in MUC-FWHZ-137. He says that Davidson in his June 8 memo (MUC-GTS-1739) shows his results to be incorrect. Zachariasen said, "I was wrong and I therefore retract my erroneous results on $PuCl_4$."

Dempster sent me a memo, dated June 25, suggesting that there is a good probability that the magnetic analysis of plutonium isotopes separated by Ghiorso, Florin, and Weissbourd from the sample prepared by Hamilton would give enough data, in addition to that which I now have, to allow a deduction of the fission cross section of Pu^{240}. He says he has not made any deposits of plutonium isotopes yet, but it is likely that the hot anode source would give a good efficiency of the order of 1 in 30,000. A special alpha-ray plate would be put in contact with the deposits to record the alpha-ray tracks from the individual isotopes.

He also suggests that a redetermination of the half-life of Pu^{240} could probably be obtained from deposits of separated isotopes of the sample containing Pu^{239} + 0.7% Pu^{240}. Here the alpha-ray tracks from each isotope would be counted on a transfer to an alpha-ray plate.

Stearns issued a memo to Division Directors and Section Chiefs stating that letters, reports, and documents containing the numerical values of nu and eta for U^{233} will be classified "secret" but will be handled in the same manner as if they were classified "top secret" when transmitted from one site to another. Numerical values of the nuclear physical constants of thorium and U^{233} and their relative importance, as compared with other elements, are to be made available only to those persons who are directly concerned with their application.

It is hot today with a high of 92.6°F at four this afternoon.

<u>Thursday, June 28, 1945</u>

The Heavy Isotopes Group met in my office at 8:30 a.m. The meeting was attended by Britain, Cunningham, Florin, Ghiorso, Hagemann, Hindman, Hyde, Jaffey, Katzin, Manning, McLane, Morgan, O'Connor, Perlman, Peterson, Scott, Studier, R. C. Thompson, S. G. Thompson, and Weissbourd. I announced that from now on the regular meetings of the group would be scheduled for 8:28 a.m. on Tuesdays in the conference room.

I then discussed in detail the plans for Farmer's No. 2 samples (Hanford-irradiated Pu^{239} and U^{235}).

Morgan prepared two samples from the CW-2 plutonium sample for Segrè (Farmer's special--plutonium sample irradiated at Hanford). Sample 1--49NG.S1--was obtained from the plutonium mechanically separated from its aluminum irradiation container. Sample 2--49NG.S2--came from the dissolved portion. It contains appreciable aluminum.

Allison sent me a teletype requesting that I wire him the estimated date of return of the material from CW-2. He also asks if I can return the shipping containers. Allison told me that a third set of irradiated samples has been pushed at Site W and that we will soon receive from him one U^{235} and one Pu^{239} sample from this set. He asks if we have any 95^{241} sample suitable for spontaneous fission counting at Site Y.

I responded immediately and indicated to Allison that we will send him the oxide of U^{235} from CW-2 about next Monday, Segrè's seven undecontaminated milligrams of Pu^{239} at about the same time, and the other two items from the Pu^{239} portion of CW-2 about two weeks from now. I add that I am surprised to learn CW-3 has already been pushed from the Hanford pile. I say we will be glad to handle them, which will be fairly easy, after the experience gained with CW-2. I also tell Allison we can provide him with about 20 million dpm of 95^{241} with 1/10 g of lanthanum immediately and samples with less lanthanum later; I say we will send this unless advised to the contrary.

Cunningham sent Farrington Daniels the Problem Abstracts for the period May 15 through June 30, 1945, for Sub-section 2.

Hamilton called me at 11:00 a.m. from Berkeley. I told him that the platinum backing plate for bombardment 6b (U^{235} plus helium ions) left here yesterday by airmail, special delivery. He informed me that bombardment 5b, 890 microampere-hours of 44 Mev helium ions on uranium metal, left Berkeley last night on the Overland Limited, and will arrive here Saturday morning. Bombardment 6a (U^{238} plus helium ions) now has over 50 microampere-hours. Bombardment 6b (U^{235} plus helium ions) will arrive by Saturday night. He may combine bombardment 7c (87 percent U^{235} plus deuterons) and bombardment 7b (Th^{232} plus deuterons). Bombardment 7b (Th^{232} plus deuterons) will come to us between July 2 and July 4 by the "City of San Francisco." He will bombard all night July 1-July 2 on the cyclotron. He may do bombardment 7a (U^{238} plus deuterons) before he does bombardment 7b (Th^{232} plus deuterons). Our bombardment 7b (Th^{232} plus deuterons) will be the last bombardment he will do for us for some time while he performs bombardments for other people, but he will be ready to bombard for us about July 15-August 1. Therefore we should get our next items out to him. In the case of bombardment 6b (U^{235} plus helium ions) he will collect extra helium ions on a uranium plate and ship that to us also--this may have 100 microampere-hours on it. The log on bombardment 5a and bombardment 5b is in the mail.

In a brief note to Ed Orlemann in Knoxville, I tell him that I have just heard of an opening in analytical chemistry at Boston University which he may be interested in investigating.

"Stettinius Quits Cabinet," reads today's headlines, but President Truman will appoint him to represent the United States in the United Nations.

Friday, June 29, 1945

John Willard wrote me from Seattle that several micrograms of element 95^{241}, isolated by Werner at Hanford, are being brought to Chicago by courier. He also informs he will arrive in Chicago on July 14. [Because the letter had to be written on an unclassified basis, Willard used an impromptu code.] (See Figure 2.)

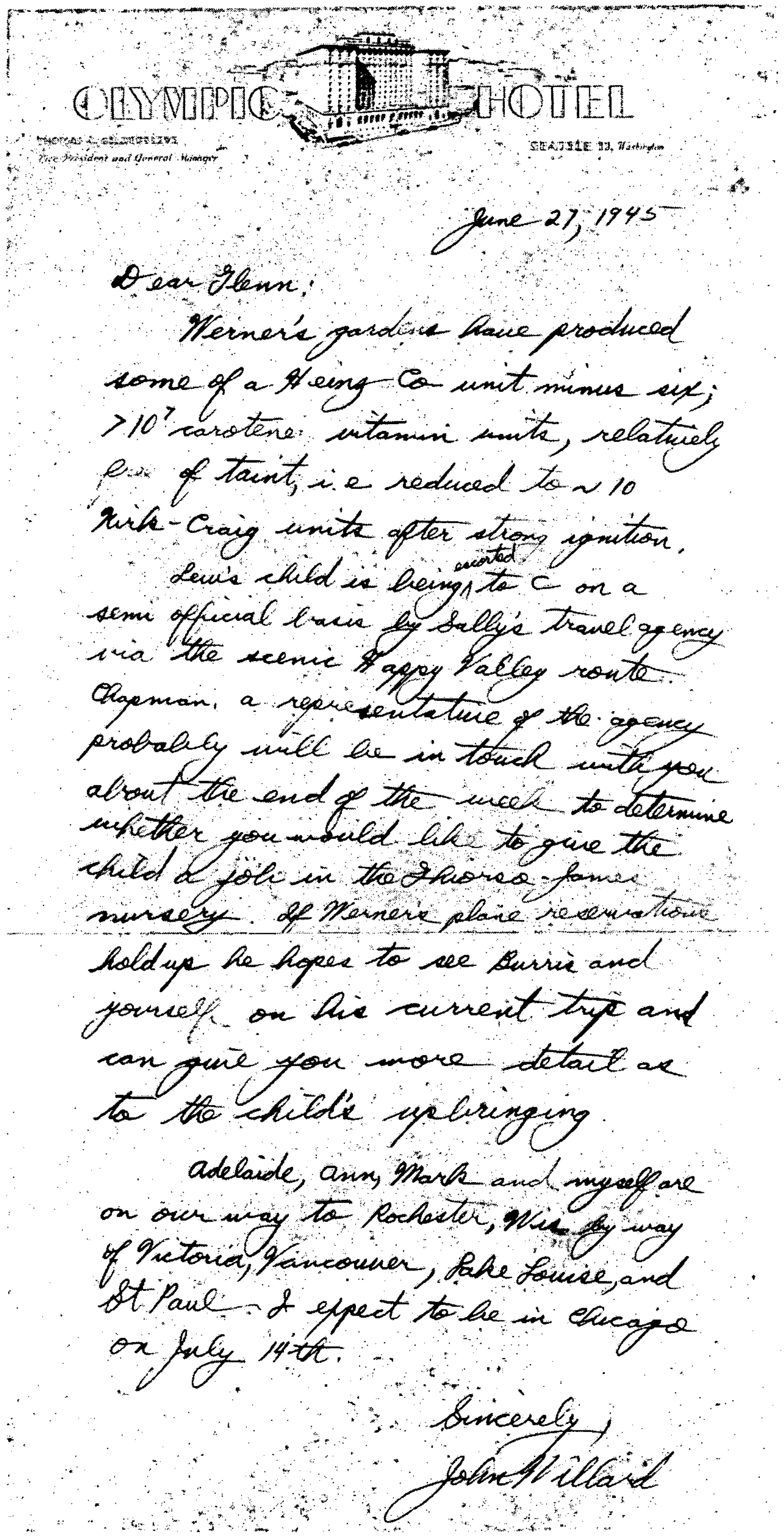
Lawrence, who is visiting the Met Lab, and I had a conversation in which I told him I have practically decided to return to the University of California. We discussed the matter at some length.

Manning proposed to Hogness that we replace our present extensive list of problem assignments with the following five general tables: (1) chemical and nuclear properties of isotopes of the heavy elements important to the atomic power program, (2) research on isotopes of importance in the U^{233} program, (3) control analysis methods for use with solvent extraction process for recovering uranium from ores, (4) services for other sections of the Met Lab and for associated projects, (5) development of "Redox" solvent extraction process for separating plutonium and uranium from fission products and from each other.

Fighting in the Pacific continues to be heavy and on many fronts.

Saturday, June 30, 1945

Today was the termination date for the following people of Section C-I: Norman R. Davidson (to RCA Labs), Jonathan S. Dixon (to Site Y), Raymond G. Larson, Albert C. Krueger, Rexford H. Bradt, John M. Dorsey (to Zenith Radio in Chicago, TV division), Nison N. Hellman (Northern Regional Research Lab, Peoria); Bernard M. Winner, Roy W. Greenlee, Michael J. Wolf (to Department of Agriculture), Richard A. Reinhardt, C. K. McLane (SAM Laboratories), Wilbur Simon, Paul R. Fields (to Site M). In addition, Ralph Seifert and T. O. Jones


OLYMPIC HOTEL
SEATTLE 11, Washington

June 27, 1945

Dear Glenn:

Werner's gardens have produced some of a Heinz Co unit minus six; >10^7 carotene vitamin units, relatively free of taint, i.e. reduced to ~10 Kirk-Craig units after strong ignition.

Lew's child is being escorted to C on a semi official basis by Sally's travel agency via the scenic Happy Valley route. Chapman, a representative of the agency probably will be in touch with you about the end of the week to determine whether you would like to give the child a job in the Ghiorso-James nursery. If Werner's plane reservations hold up he hopes to see Burris and yourself on his current trip and can give you more detail as to the child's upbringing.

Adelaide, Ann, Mark and myself are on our way to Rochester, Wis. by way of Victoria, Vancouver, Lake Louise, and St Paul. I expect to be in Chicago on July 14th.

Sincerely,

John Willard

XBB 775-4814, XBB 775-4815

Figure 2. Letter from John Willard received June 29, 1945. Code: Werner's gardens - work of Louis Werner in Willard's Process Chemistry Section at Hanford; Heinz Co. unit minus 6 - 57-6 = 51 or 95^{241}; $>10^7$ carotene vitamin units (vitamin A) - alpha particle counts per minute; relatively free of taint - relatively carrier-free; 10 Kirk-Craig units - weight is 10 micrograms as weighed on an ultramicro balance developed by Kirk and Craig; Sally's travel agency - an officer named Sally is arranging for shipment to "C" (Chicago) on "scenic Happy Valley route" (Great Northern Railway); Ghiorso-James nursery - investigations of Ghiorso and James on properties of 95^{241}.

are transferring to other sections of the Met Lab effective July 1; Seifert is joining the new piles section under Hogness, and Jones is going to the Patent and Information Division.

James Franck resigned as Associate Director from the Chemistry Division effective today. He will continue to serve the laboratory on a consultant basis.

The uranium metal plus 44 Mev helium ions Berkeley bombardment 5b arrived this morning on the Overland Limited. It has received 886.9 microampere-hours.

I prepared an account of the discovery and early study of U^{233} for the use of William L. Laurence of the New York Times who has been brought into the Project by General Groves to represent the news media.

New developments in the Pacific war include the occupation of the small island of Kume (located north of Okinawa) without resistance.

<u>Sunday, July 1, 1945</u>

A chart prepared last Thursday shows the organization of Section C-I as of today as follows:

Glenn T. Seaborg - Section Chief
Ruth P. Rogers - Secretary to Seaborg
Kathleen Florin - Clerk

Winston M. Manning - Associate Section Chief
Isadore Perlman - Associate Section Chief
Jane Horwich - Secretary
Frederic W. Albaugh - Assistant Section Chief (leaving July 15)
Dorothy Black - Secretary (transferring July 15)

Group 1, Heavy Isotopes - No group leader
Group 2, Control Analysis - Arthur H. Jaffey, Acting Group Leader
Group 3, Services - Donald C. Stewart, Group Leader
Group 4, Solvent Extraction - Stephen Lawroski, Group Leader
James R. Gilbreath, Assistant Group Leader

Group 1, Heavy Isotopes
Cunningham, Burris B. - Research Associate
Ghiorso, Albert - Research Associate
Hindman, Clark J. - Research Associate
Katzin, Leonard I. - Research Associate
Kohman, Truman P. - Research Associate
Morgan, Leon - Research Associate
Robinson, Herman - Research Associate
Simpson, Oliver S. - Research Associate
Studier, Martin [ERC] - Research Associate
Thompson, Roy C. - Research Associate
Thompson, Stanley G. - Research Associate
Westrum, Edgar F. - Research Associate
Florin, Alan E. - Research Assistant
Hausman, Eugene A. [SED] - Research Assistant
Hopkins, Horace H. [SED] - Research Assistant
Hyde, Earl - Research Assistant
James, Ralph - Research Assistant
Magnusson, Lawrence - Research Assistant
O'Connor, Paul - Research Assistant
Peterson, Sigfred - Research Assistant
Scott, Benjamin F. - Research Assistant
Van Winkle, Quentin - Research Assistant
Walsh, Patricia - Research Assistant
Erway, Norman- Technician
Thomson, Helen - Technician (half-time)

Group 2, Control Analysis,
Jaffey, A. H. - Acting Group Leader
Anderson, Herbert H. [SED] - Research Associate
Ames, Donald P. [SED] - Research Assistant
Fineman, Phillip [SED] - Research Assistant
Sedlet, Jacob - Research Assistant
Weissbourd, Bernard [ERC] - Research Assistant

Group 3, Services,
Stewart, D. C. - Group Leader
Asprey, Larned B. [SED] - Research Assistant
Britain, J. W. [SED] - Research Assistant

Group 4, Solvent Extraction,
Lawroski, S. - Group Leader
Gilbreath, J. R. - Assistant Group Leader
Blaedel, Walter J. - Research Associate
Hagemann, French T. - Research Associate
Schaffner, Irwin J. - Research Associate
Ader, Milton [SED] - Research Assistant
Gaarder, Sidney - Research Assistant
Goeckermann, Robert - Research Assistant
Kelley, Alec [SED] - Research Assistant
Post, Roy [SED] - Research Assistant
Schraidt, John H. [SED] - Research Assistant
Hyman, Herbert H. - Research Assistant

Boykin, Pearline - Technician
Giacchetti, Olga - Technician

Today Monsanto Chemical Company is taking over the responsibility for operation of Clinton Laboratories as a replacement for the University of Chicago.

James completed isolating the Np^{237} from the plutonium-neptunium fraction of sample 25NA, received from Britain on June 25 and June 28. (Sample 25NA is part of Farmer's special U^{235} sample CW-2, which has been irradiated at Hanford.) On the basis of the yield he calculcated the cross section for the reaction: $U^{236}(n,\gamma)U^{237}$ to be 2.2 barns.

Helen, my sister Jeanette, Steve Lawroski, and I went by I.C. R.R. to Calumet Country Club to see the last round of the Chicago Victory National Open. Byron Nelson won the open with 275, "Jug" McSpaden, and Ky Lafoon tied for second 282. Afterward we all has dinner at our apartment.

President Truman nominated James F. Byrnes to become Secretary of State; his name will go to the Senate tomorrow.

Monday, July 2, 1945

Truman Kohman, Robert Goeckermann, and Sydney Gaarder returned to the Met Lab from Hanford. Kohman is joining the Heavy Isotopes Group, while Goeckermann and Gaarder will be in the Solvent Extraction Group.

Quentin Van Winkle began his vacation; he will return Monday, July 16.

I received a letter dated June 27, from Hamilton in Berkeley. He enclosed a day-by-day log of the bombardments of uranium metal targets with helium ions.

He also told me the U^{238} as U_3O_8 went on that afternoon (June 27) and if everything goes well, the U^{235} target would be put on sometime Friday, June 29, and the thorium target on Monday morning, July 2. The three samples will then be put aboard the streamliner Monday afternoon (today), and are scheduled to arrive in Chicago on Wednesday morning.

Perlman sent his comments to Russell about the proposed method of analysis of urine by $BiPO_4$-LaF_3 precipitation.

Daniels sent Compton a summary of the tentative research program for the Metallurgical Laboratory from July 1, 1945, to June 30, 1946. It shows that the chemistry research would consist of: (1) chemical and nuclear investigations of the heavy elements 89-96, (2) control analysis in the extraction of uranium ores, (3) development and test of a solvent extraction process to separate plutonium, uranium, and fission products, (4) fission product studies, (5) radiation chemistry, (6) high temperature oxide piles, (7) analytical chemistry.

The manpower distribution in the Chemistry Division, as prepared by Hogness, is as of July 1, 1945:

	Heavy Isotopes		23
Section C-I	Jaffey, Control Analysis		6
Seaborg	Stewart, Services		3
	Lawroski, Solvent Extraction		11
	Administration - Seaborg, Manning Perlman, Albaugh (until July 15)		4
		Total	47
	Daniels, Pile Research		5
Section C-II	Allen, Radiation		4
Hogness, Acting	Rubinson, Fission Products		5
Section Chief	Watters, Analytical		10
	Administration - Hogness, Willard		2
		Total	26
		Grand Total	73

7/2/45 (cont.)

The following is the organization chart of the Chicago Metallurgical Project as of July 1, 1945.

U. S. ARMY

UNIVERSITY OF CHICAGO

CHICAGO METALLURGICAL PROJECT
Director: A. Compton
Assoc. Dir.: W. Bartky
Assoc. Dir.: N. Hilberry

Metallurgical Laboratory
Director: F. Daniels
Assoc. Dir.: J. Howe
Asst. Dir.: R. Lapp

Argonne
Acting Dir.:
W. Zinn

Information
R. Mulliken

Patents
H. Metcalf

Physics and Metallurgy
A. Dempster
F. Foote

I-Mass Spectroscopy
A. Dempster

II-Crystal Structure
F. Zachariasen

III-Metallurgy
F. Foote

Chemistry
T. Hogness
J. Willard

I-Heavy Elements
G. Seaborg
W. Manning
I. Perlman

II-New Piles
T. Hogness
J. Willard

Health
R. Stone
L. Jacobson

I-Clinical Medicine
L. Jacobson

II-Biological Research
K. Cole
C. Prosser
A. Brues
R. Zirkle

III-Medical Industrial Hazards
J. Nickson
J. Rose

War Services
Administrator:
J. Simmons

I-Shops
T. O'Donnell

II-Treatments
W. Jesse

III-Optical
G. Monk

IV-Glass Blowers

Business Manager
University of Chicago
W. Harrell

Administration
W. Branch
B. Gore
J. Wilkinson

I saw Hogness and told him of my discussions with Lawrence on Friday. I said that I probably shall return to the University of California.

Harold Urey gave a lecture at the Metallurgical Laboratory which I attended. He described the theory and chemical exchange reactions involved in the production of enriched N^{15} and enriched C^{13}. He also explored the possibilities of separating heavier isotopes, such as those of iron, by partition between water and organic solvents and the use of iron carbonyls.

At 7:45 p.m. Cunningham spoke on the chemistry of Pu^{239} at the chemistry Seminar in Room 251, Ryerson Laboratory.

The U.S. has adopted a policy for dividing Germany by the Allies into regions to decentralize and to produce militarily impotent districts, according to today's paper.

Tuesday, July 3, 1945

I attended a meeting at 8:28 a.m. in the New Chemistry conference room of Groups 1 and 3 of Section C-I. It was attended by Asprey, Britain, Cunningham, Florin, Ghiorso, Hindman, Hopkins, Hyde, Jaffey, James, Katzin, Kohman, Magnusson, Manning, Morgan, O'Connor, Perlman, Peterson, Robinson, Scott, Simpson, Stewart, Studier, R. Thompson, S. G. Thompson, and Westrum. I opened the meeting by discussing the future organization of the section. I indicated Groups 1 and 3 will meet on Tuesday morning and Groups 2 and 4 on Thursday morning. I pointed out that in the absence of group leaders, it will be essential for each man to keep Manning, Perlman, and me informed as to what he is doing rather than to leave it to us to ask him about it. We want the results whether they are good or bad. I also described the organization of the Metallurgical Project, as presently planned.

Perlman asked what happened to the 25NA sample (Hanford-irradiated U^{235}, Farmer's special No. 2). Britain indicated that 19.5 mg are ready for us, 168 mg are ready to ship away, 10 mg (undecontaminated) are left in solutions and plates, and 20 mg are reserved for call by Site Y. I observed that this corresponded to a good recovery. Britain mentioned that the absorption curve on 1 mg of the sample shows a curve that is very much like the 92^{237} curve in the files.

I questioned James about the results of the measurements of the Np^{237} from the 25NA sample, and then I proceeded to list the series of reactions that could take place during irradiation, starting with U^{235} and leading to Pu^{238}. I pointed out that from a number of these yields the U^{236} cross section could be determined. I discussed the ambiguity in the U^{236} cross section because of a discrepancy in the estimate of the total flux--Fermi estimates that the total flux on this (Farmer No. 2) is six times the total flux on Farmer No. 1, whereas the yields of reactions of known cross sections give a ratio of 15 or 18. There was a discussion of various ways of estimating the ratio of the fluxes, including measurement of one of the fission products. I referred to Ghiorso's search for U^{236} alpha particles in which he found twice as much activity at the U^{235} peak than could be accounted for by the fission counts. I added that measurements have been made on only one sample and will have to be checked. Using the high value for the flux, Ghiorso has calculated the half-life of the U^{236} to be 8×10^7 years; when he uses the lower flux value, he finds the half-life to be 3×10^7 years.

I asked O'Connor about the status of the 49NG sample (Hanford-irradiated plutonium from Farmer's No. 2). He has extracted plutonium, neptunium, and uranium into the ether layer and will next carry out a lanthanum fluoride cycle followed by a sodium plutonyl acetate cycle. Morgan indicated he intends to work on a small part of the water phase with lanthanum fluoride to make an accurate determination of the Pu^{239}/Pu^{242} ratio, then concentrate the 95 and 96 fraction. Perlman asked whether Thompson's column could be used to try to separate the 95 and 96; however, Thompson indicated his column is not quite ready yet. I suggested that Morgan and Thompson divide up the bulk of the water layer, each man taking what he thinks he can handle.

Hindman's specific activity measurements give preliminary results on the 170 gt material that show a 4.7 percent increase in specific activity; he will work on O'Connor's plutonium as soon as it is decontaminated further. Kohman pointed out that Hoekstra obtained only 2.3 percent increase on 160 gt material. I observed that I think the higher value may be wrong but that I did not want to discourage Hindman from getting the right answer.

I asked that a sample be made for Ghiorso, free of elements 95 and 96, so that he could measure the growth of 95^{241} on top of the 94^{238} peak; O'Connor should provide the material for Scott to electrolyze. In this connection I assigned to Britain, as his next problem, the increasing of the electrolysis yield. I also asked that he try, when he has time, to work out an idea for using solvent extraction methods for separating neptunium, plutonium, and uranium in the bombarded targets.

Ghiorso reported on fission measurements made at Argonne of sample 49NG. The results indicate that, compared with the Pu^{239} standard, the decrease in the ratio of fissions to alpha particles was 14.6 percent--if Pu^{240} does not fission, this indicates the sample contains 7.3 percent of Pu^{240} out of the total plutonium, if the half-life of Pu^{240} is 8,000 years.

James told us what has been done with the target of uranium plus helium ions. He indicated that it had been machined off in five layers of 0.002 inches each; I explained that Hopkins, James, and Florin will try to get the neptunium, plutonium, and uranium from each layer.

I alerted the men to the fact that two other targets are coming from Berkeley in a few days--depleted uranium and 87 percent enriched uranium; these have received 50 to 100 microampere-hours of helium ions. The primary purpose of these bombardments is to enable the making of isotopic assignments by knowing that the target is of essentially pure isotope. I assigned the work on these targets to James and the others--in their spare time. I also mentioned that a target of thorium plus deuterons is coming in for Jaffey and Hyde, who will look at the protactinium primarily and then examine the uranium for beta particles. They will also examine the actinium.

Roy Thompson talked about the protactinium package from Agruss and said that 110 mg of dry materials have been received. So far six mg of protactinium have been found, if he assumes all the alpha activity is protactinium. In answer to another of my questions, Thompson indicated that the semiworks group is working on the last barrel and that we may end up with a total of 20 mg of protactinium.

Asprey discussed the status of the 10 g of high gt material and said that it is practically free of lanthanum and has been sitting since June 27. Asprey suggested the possibility of milking it after a week to make sure the lanthanum is out. I agreed this would be a good idea and observed it would also keep the men in practice.

Ghiorso reported on the fission cross section for Th^{229}. He told us that Hagemann has given him about 5,000 counts of Th^{229} and that the number of fissions in the sample corresponds to 5 c/m of U^{233} impurity. He indicated that, if there is no impurity, the fission cross section of Th^{229} is only 1/10 that of U^{233}.

Peterson discussed his experiments on carrying of actinium by zirconium iodate; the purpose is to investigate the fundamental chemistry of actinium to compare it with the behavior of 95, for example. He has found the iodate concentration very critical. He has also found that the IR-1 resin seems to give good separation of lanthanum and actinium tracers.

Stan Thompson told us that he has run a synthetic mixture of 95 and the rare earths through his column and will analyze them as soon as possible.

Magnusson said that we have about 6 mg of neptunium in the laboratory. I announced we will probably receive a neptunium extraction run at Site W in August, which may yield 500 mg of Np^{237}. I initiated a discussion of the possibility of putting 1 mg of pure Np^{237} into the Clinton pile for Pu^{238} manufacture rather than waiting longer to have it irradiated in the Hanford pile; the consensus was that it should be sent to Clinton, so I asked Magnusson and Jaffey to prepare the sample.

I asked Simpson to report on his future program. He identified three programs: (1) continue the work on PuO_2 to find what volatilizes out of the oxide and also to measure thermodynamic data, (2) measure the vapor pressure of uranium metal and uranium compounds, (3) measure the vapor pressure of BeO and other refractories for Daniels' high temperature pile. I suggested a fourth program--volatilize some high gt material to see if the 95 concentrates in a particular fraction. This would be for the purpose of determining the variation of vapor pressure across the actinide series.

Perlman, Katzin, and I discussed plans for work on protactinium chemistry.

I mentioned we are planning to measure a number of heats of formation of plutonium compounds and asked Westrum to describe his program. He told us that he has the data necessary to determine the heat of formation of $PuCl_3$ and that he has run the oxychloride and should have its heat of formation by the end of the week. Next, he intends to determine the heat of formation of PuO_2 and PuF_3.

We discussed the degree of error in the specific activity of plutonium; Westrum and Hindman will get together and decide the best value of the specific activity since our value is the standard used for all other laboratories.

Kohman asked what decisions have been made about the naming of elements 95 and 96. There was some discussion on this point, but the secretary (Jaffey) was too tired to take notes. The meeting ended at 10:45 a.m.

Hogness sent a memo dated July 2 to Daniels giving the Met Lab's contribution to the program to be held at Clinton July 17 and 18. I am to give a 30-minute talk on the "Effect of Heavy Isotopes on the Operation and Products of Converter and Breeder Piles."

In a memo to Jesse, Manning requested two methane-filled proportional counters, one for the solvent extraction program and the other for heavy isotopes research.

Hogness indicated to Daniels that the estimated budget of the Chemistry Division for the coming fiscal year is $244,082.

Hutchins has become the chancellor of the University of Chicago in an administration reorganization, and Ernest C. Colwell has become the new president. Hutchins will still be the executive officer of the University, Colwell responsible to him; and there will be three vice presidents: R. G. Gustavson (dean of the faculty), Wilbur C. Munnecke (business affairs), and Neil H. Jacoby (in charge of development).

Yesterday I told Hogness that I intend to return to Berkeley. Today I was asked to go down to Chancellor Hutchins' office. I conferred with Hutchins, Bartky, and Gustavson for over an hour. They explained in detail their plans for the Nuclear Institute and offered me a salary of $10,000 as a Full Professor to change my mind. (As an inducement to return to Berkeley I was recently promoted, in one large step, from Assistant Professor [salary $2,700 per year] to Full Professor [salary of $4,200 per year]). They are lining up a huge sum of money to support the work and spoke in terms of a large University budget in addition to any government support that might come. They are thinking of about twelve chemists of Assistant and Associate Professorial rank and mentioned Perlman as an example of one of these men.

This morning's paper reports on President Truman's message to the U.S. Senate asking them to ratify the new international security charter.

<u>Wednesday, July 4, 1945</u>

As usual the Met Lab is working today and is not taking a holiday.

Clark J. Egan terminated work at the Lab to return to the California Research Corporation in Richmond, California.

The following Berkeley bombardments arrived this morning on the streamliner: U^{235} plus helium ions (bombardment 6b); it received 67.6 microampere-hours between June 28 and July 2. Uranium disc placed behind U^{235} (bombardment 6b); this received 78.8 microampere-hours. U^{238} plus helium ions (bombardment 6a); it received 70.8 microampere-hours between June 27 and 28. All three targets were turned over to Ralph James who began separation of the various fractions.

A courier left for Site Y by train today, carrying two samples resulting from our work on CW-2 material (Hanford-irradiated U^{235}): (1) sample GTS-100, about 170 mg of uranium element in the form of U_3O_8; and (2) sample GTS-103, about 2×10^7 d/p/m of 95^{241} mixed with about 100 mg of lanthanum. Don Stewart and I talked to men at Site Y by phone to describe these samples.

I wrote to Jerry Howland in Dayton to say I am trying to arrange things so I will be in Dayton on Sunday, July 15, to see the finals of the PGA Golf Tournament while on my way to Clinton Labs. I suggested he might like to join me.

I sent Hamilton a radioautograph which I thought might be of interest since it shows a different distribution from the previous ones.

I replied to Mulliken's memo of June 22 to give him my evaluation of the values for the abundance of the isotopes of helium, oxygen, and rhodium which appear in my table. With regard to oxygen, I suggested the values used in the secret "Table of Isotopes" in the Project Handbook, III, c, 3.1, p. 5 are somewhat better.

Cunningham wrote to Koch at the University of Illinois, to tell him there will be a delay of six weeks in the preparation of a plate of U^{233} on which Koch is to make arrangements. Cunningham then asked how the measurements with Pu^{239} are progressing.

A. J. Dempster called me to say that he would like 0.5 microgram of plutonium in a cubic millimeter of solution (i.e., a concentration of a milligram per cubic centimeter). He asked that someone put it on the filament that Lewis will bring over.

Later I called Joe Hamilton at Berkeley who told me that bombardment 6a (U^{238} plus helium ions) was done on a whole backing plate: the total bombardment is 70.8 microampere-hours. He said bombardment 6b (U^{235} plus helium ions) was done on a probe, and he now has learned that the total bombardment is approximately 80 microampere-hours. The backing plate in this case received roughly the same amount of bombardment as the probe. Bombardment 7b (Th^{232} plus deuterons) had problems when he changed to deuterons. He will run bombardment 7a (U^{238} plus deuterons), bombardment 7c (87 percent U^{235} plus deuterons) (bombardment 7b will be last). Hamilton will give us notice of a week or two as to when they will be finished.

Bombardment 7b (Th^{232} plus deuterons) and bombardment 7c (87 percent U^{235} plus deuterons) will be done together, for a total of 500-1,000 microampere-hours. Joe said that the two old beryllium targets that he has been bombarding for Daniels will have a total of approximately 20,000 microampere-hours over a period of 1-1/2 years. This target also contains some zinc, cadmium, argentum, as hard solder. He mentioned he will start a bombardment of uranium with deuterons for Stone (two weeks exposure) after he finishes two weeks of thorium plus deuterons for Friedell. He will process Stone's uranium for him.

Jeanette took the "400" train to Ishpeming where she will visit our relatives until July 27.

Today's paper reports that Harry Hopkins is resigning as a White House aide because of poor health.

Also on the front page there is an article from Decatur, Illinois, about the wearing of shorts by women and girls on the streets. The police chief of that city stated that the wearing of "abbreviated shorts in public places will not be tolerated by the forces of law and order." The chief defined abbreviated shorts as any that fail to come at least to the knees. So far the rules have been ignored, and more shorts are being seen than before.

Thursday, July 5, 1945

At 8:28 a.m. I held a meeting in the New Chemistry conference room of the Solvent Extraction and Control Analysis Groups of our section. It was attended by Ader, Ames, Blaedel, Fineman, Gilbreath, Goeckermann, Hagemann, Hausman, Hyman, Jaffey, Kelley, Kohman, Lawroski, Manning, Perlman, Post, Schaffner, Schraidt, Sedlet, and Weissbourd. I opened the meeting by explaining that the new meeting procedure is somewhat as follows: on Tuesday mornings there will be a meeting devoted to heavy isotope chemistry and on Thursday mornings, a meeting devoted to solvent extraction and control analysis. The question of whether to hold the Thursday meetings weekly or biweekly is still uncertain.

Blaedel reported that three activities are under way in the Solvent Extraction Group: (1) construction by the group of five columns for complete testing of the Redox process on a one-inch column scale, (2) batch experiments on the stability of reducing agents in the second column, and (3) experiments using a single one-inch column as an intermediate step between batch experiments and final multiple column tests. In answer to a question of mine Blaedel said that the designations given to the five columns are IA for the first column where the plutonium and uranium are extracted from the feed, IB to the next column where these two elements are separated, IC to the uranium recovery column, IIA to the second cycle oxidizing column, and IIB to the final column.

There was next a lengthy discussion of the problems involved in finding a suitable reducing agent for the second column (Column IB). The work has been concentrated on hydrazine and hydroxylamine, and plans are under way to study the effect of various changing conditions on the stability of reducing agents, especially ferrous-hydrazine.

Schaffner reported on the status of the column work construction. Columns IA, IB, and IC are now in place. The men have encountered some trouble in locating a suitable gasketing material between glass sections. Answering a question from Manning, Gilbreath indicated that the columns will accomodate up to about 0.1 Hanford level of activity without excessive exposure.

Jaffey described the work of the Control Analysis Group. By way of background he explained that the Mallinckrodt Chemical Company has been making pure uranium materials for the Project by starting with relatively pure oxide, dissolving it, and then ether extracting it. Now they are working with the crude ores instead. At the present time, the radium in the ores is not needed, but it is too valuable a constituent to lose in the process. As a result, it is necessary to analyze all waste materials to find out where the radium is going and set aside for later recovery. This has been the first problem assigned to the Control Analysis Group. Other problems will involve the setting up of a control laboratory to operate in routine fashion and to determine the existence of any health hazard from other radioactive species that appear in any concentration in the various fractions in the process. Jaffey and Weissbourd then talked about the work being done on the radon evolution method of analysis.

Two additional samples resulting from our work on CW-2 (Hanford-irradiated plutonium) left for Site Y by automobile today: (1) sample GTS-101, from the plutonium mechanically separated from its aluminum irradiation container; and (2) sample GTS-102, from the stock prepared by dissolving the plutonium sticking to the walls of the container. The samples were prepared by Morgan on June 28.

I received a letter dated July 2, from Hamilton giving the bombardment history for the samples of U^{238}, U^{235}, and uranium metal backing discs received yesterday. He told me that he has not been able to get adequate bombardment on the thorium disc but will try to get it in for me whenever he can arrange for a courier to take the sample east. He also hopes that the metallurgy people have made progress in making plutonium metal targets since he does not like to use compounds in the cyclotron.

I read a copy of a July 4 memo from Compton to Division Directors and Section Chiefs about the composition of the Scientific Panel to advise the Interim Committee. He explains that it is not possible to comply with the suggestions to add Urey to the panel because of the necessity to limit its size. He notes that the suggestions made to the Scientific Panel about the program organization and other aspects related to the future of the Metallurgical Project have been called to the attention of the Interim Committee and are now being discussed.

I answered Kennedy's letter of June 19 about Patent Cases S-52 and S-61. I explain I have delayed answering him because I have been at a loss as to what to suggest. I go on to say that I do not want to concur in his proposal that we forget the whole thing, if he means by this that we should not even take out the patents in our names at all but just let Lavender's office get along as best it can without help and cooperation. I then suggest that perhaps we should write to Lavender, alluding to the attitude of the University of California and suggesting that he send the last page of the agreement draft of June 2 to the University asking them to sign it. I also propose that, in the letter to Lavender, we say that the inventors now accept the agreement in the form of the draft of June 2, just as it is.

I mention that it might be desirable to write a letter to Sproul as well as Lavender expressing our belief that the inventors do have the right to proceed according to the plan outlined in my letter to Sproul of February 15. That plan suggests that the control of non-governmental applications remain in the hand of the inventors in one of the cases (Case 52). I say that I would indicate in the letter that I would be willing to enter into a written agreement with the University of California to let them control my share of any benefits which might accrue from this arrangement.

I explain to Kennedy that I do not want to enter into any argument with the University because I think it is quite likely I will return there. I add that, under such conditions, I believe it will be quite likely that my one-fourth of the proceeds would be put into the type of research that I would suggest in the somewhat unlikely event that an appreciable sum of money were ever to derive from this sort of thing. I assure Kennedy that I will do my best to support the plan of action for which the majority of the inventors vote although I would be willing to have both cases handled on the same basis now suggested for Case 61. I ask for his comments.

Darrell Osborne called me from Maryland to say that he cannot join the Met Lab now but hopes to be able to make it by September 1. I told him we could take him then. He said he could come for an interview any time except the week of July 29. We agreed to arrange for the interview around July 26. He gave me his address as Allegheny Ballistics Laboratory, Box 120, Cumberland, Maryland.

Farrington Daniels informed Captain J. H. McKinley, by memo, today that a panel meeting will be held on the evening of July 12, on Social Implications. Daniels told McKinley, "Scientists have long been accused of taking no responsibility for the social and political upheavals resulting from their discoveries, but the members of our scientific staff are keenly aware of their responsibility in our present program." Daniels also says that he believes Dr. Compton agrees that scientists in the Met Lab should be encouraged to think of the social and political implications of this work.

I played 15 holes of golf at Jackson Park with Ghiorso and Morgan (AG-49, TM-56, GS-48 for nine, AG-95, TM-107, GS-94 for 15).

"All Philippine Isles Freed" reads today's top headline, and the Philippine campaign is completed, says General Douglas MacArthur.

<u>Friday, July 6, 1945</u>

I sent Allison at Site Y a description of samples GTS-100, 101, 102, and 103 that were sent to him on July 4 and 5. I then go on to say that we have obtained a number of interesting results from our work on these samples. I say that in sample CW-2 we found U^{237}, Np^{237}, and Pu^{238} in such amounts that we feel confident they are produced by reactions:

$$U^{235}(n,\gamma)U^{236},\ U^{236}(n,\gamma)U^{237},\ U^{237} \xrightarrow{\beta^-} Np^{237},\ Np^{237}(n,\gamma)Np^{238},$$

$$\text{and } Np^{238} \xrightarrow{\beta^-} Pu^{238}.$$

I say these all lead to a value for the cross section of the reaction $U^{236}(n,\gamma)U^{237}$ of a few barns, but we feel there is a discrepancy between what we have heard is the estimated nvt for this sample, namely, about six times that of CW-1 and the value that we seem to find. On the basis of the yield of Pu^{239} from the U^{238} in CW-2-L we feel the nvt of the sample must have been higher.

I wrote to Luther Arnold to thank him for his letters and to tell him that I have written Ed Orlemann about the opening at Boston University. I also add that I have been playing golf regularly.

In a letter to A. Q. Butler of the Mallinckrodt Chemical Works, attntion G. L. Martin, Jaffey encloses analyses of some of the samples Martin has sent us. Jaffey mentions that Truman Kohman is taking over this work.

Manning sent a complaint to Captain McKinley that a teletype delivered to me this morning was sent by Hamilton on July 2. It described a change in plans concerning the shipment of a sample. It was only through a fortunate circumstance that we avoided serious difficulties as a result of the sample arriving before the teletype.

Dempster sent Daniels the program of the Physics Division for the next year. He indicates that the Mass Spectroscopy Section plans the following investigations with Section C-I: (1) determination of the masses of the radioactive isotopes of elements 95 and 96, (2) measurement of the percentage of Io^{230} in a sample that is mixed with the Th^{232}, (3) comparison of the half-lives of Pu^{240} and Pu^{239} by means of alpha-ray tracks on transfer plates from separated isotopes, (4) slow neutron fission cross section of Pu^{240}, deduced from the magnetic analysis of Hamilton's sample prepared by α,2n reaction on uranium.

Secretary of Treasury Morgenthau resigned yesterday as a result of rumors that he was going to be released.

Great Britain and the U.S. have recognized the Warsaw government of Poland, cutting adrift the exile regime in London. Both governments said that this is the beginning of the fulfillment of the Yalta "Big Three" agreements.

Saturday, July 7, 1945

Howland wrote to me from Dayton that he wishes he could have been out watching the Victory Open with me last Sunday. He goes on to say that while the city of Dayton is okay from his standpoint, he is very dissatisfied with the working conditions. There is little fundamental research going on, and people are not well informed about other project work. He refers to the laboratory building as a "hole" and says the general level of contamination must be a hundred times higher than New Chem ever was--"sitting in the library a few hours his hands often send the monitor off-scale." He did indicate that a health unit is growing rapidly. He has concluded that the general atmosphere of an industrial laboratory is not much to his liking and that he would be much happier if he could get connected with a first-rank university. In a P.S. to the letter he says that if Site Y should happen to ask him to go out there, he would not hesitate to go.

I received a letter from Bill Watson in Montreal, who asked for additional information about the 10 mg sample of U^{233} we prepared for irradiation at Site X for Goldschmidt last January. I asked Katzin to look into the matter.

I returned to Jesse some reports he sent me many months ago on the determination of thorium and uranium content of ores. I expressed regret that our work carried on in Katz's group, on the ether extraction of such ores, did not yield any positive results.

Wayne Johnson and I talked by phone, and I told him about Osborne. Johnson asked me to phone Osborne and invite him to visit us. We will pay for meals, hotel bills, and lower-berth Pullman accomodations, but he must get his own reservations.

Mulliken transmitted to F. W. Aston (Cambridge, England) for the "International Table of the Isotopes," information I gave him Wednesday about the abundance of helium, oxygen, and rhodium.

President Truman has appointed Fred M. Vinson as the new Secretary of the Treasury, says this morning's paper.

Sunday, July 8, 1945

I played 18 holes of golf at Timber Trails Golf Course (Plainfield and Wolf Roads, La Grange) with S. Thompson, York, and Lawroski (GS and SL won a "low-ball plus low total" match, 4 up; ST-105, FY-92, GS-97, SL-100).

A headline in today's Sun reads "Vast Air Fleet in Marianas; Kyushu Blasted by U.S. Fliers." The U.S. flag was raised on Okinawa.

Monday, July 9, 1945

Morgan began experiments to determine the neutron flux that samples 25NA and 49NG received. He will measure the content of Ba^{140} formed in fission. Samples 25NA and 49NG are the U^{235} and Pu^{239} samples irradiated at Hanford as Farmer's special No. 2, or CW-2 samples.

Howland wrote me a letter dated July 6 giving the arrangements he is making for us to watch the PGA tournament on the 15th when I hope to be in Dayton.

I wrote a long letter to Latimer in Berkeley telling him of my conversation with Lawrence on Friday, June 29, when I told him of my decision to return to Berkeley. I also report to Latimer of my subsequent discussion with Hutchins about the University of Chicago's plans for an Institute and the salary offer of $10,000 they made to me in an effort to change my mind. I say I am worried as to whether we, at Berkeley, are thinking in broad enough terms and that I believe we should think of University support for this program. I ask if it would be a good idea for Lawrence, him, and me to go over the matter with Sproul. I observe that it certainly would be too bad to allow it to come to pass that the University of California play a secondary role in this field.

I mention I have received my appointment from Underhill (Professor of Chemistry at $4,200 per annum, on leave of absence for the year 1945-1946) and signed and returned the letter of acceptance.

In closing I indicate my plans to include visits to Berkeley in August with a side vacation trip to Los Angeles.

In another letter I tell Lawrence of the vigorous reaction of the University of Chicago to my decision to return to Berkeley. I mention the visit with Hutchins and the salary offer made to me. I also say that the University of Chicago is planning in terms of a large University budget in addition to any Government support that might come, that a number of University appointments of physicists have already been made, and the University of Chicago is speaking in terms of about a dozen chemists of assistant and associate professorial rank.

I mention that I saw Zinn last week and he is definitely interested in Berkeley; however, the University of Chicago has made him a very attractive offer here--an associate professorship at a salary of nearly $7,000.

I tell him of my concern, as I expressed in my letter to Latimer, that we are not thinking in broad enough terms at Berkeley, especially in connection with the extent of University support. I also repeat the suggestion I made to Latimer that we talk this over with President Sproul.

I submitted the plan for the research program of section of C-I for the period July 1945 to July 1946. I subdivide the program into four areas, as follows:

I. Chemical and Nuclear Investigations of the Heavy Elements. The program will include the chemical, physico-chemical, and nuclear investigation of all of the heavy elements.

II. Control Analysis in Connection with Uranium Extraction. The program includes the development of methods of analysis and the performance of routine analyses in collaboration with the chemical plant for the extraction of uranium from ores being operated by the Mallinckrodt Chemical Company in St. Louis. The isotopes radium, protactinium, ionium, and actinium constitute health hazards for the operation of such a chemical plant. It is our task to determine for the company the distribution of such isotopes for each of the steps of their process.

III. A complete Solvent Extraction Process. The program covers the development and semiworks scale operation and demonstration of a complete solvent extraction procedure for the processing of plutonium formed by irradiating uranium in the pile.

IV. Services. It is anticipated that our program will include the preparation of a number of special samples and the processing of a considerable amount of special irradiated material for other parts of the Project.

I wrote to Howland to inform him that my plans have been changed and I shall no longer be able to make it to Dayton next Sunday. I explain that the meetings down south have been called off and, as he may have heard, the transportation situation is now nearly impossible; apparently there will be no more Pullman travel between Dayton and Chicago. I also say that I should like to talk to some Monsanto official about Howland's comments on the condition of the Mound Laboratory, perhaps C. A. Thomas, but I would like to go over it with him (Howland) first. I ask him to let me know when he plans to come to Chicago, saying that I shall be away from Chicago most of August and a few other days on short trips.

I called Osborne in Maryland to invite him to visit us here between July 26 to 29. He will telegraph me the exact dates as soon as he knows. I said I also will telegraph him if my plans change so that I will not be here on his dates. I told him that a salary of $5,400 will be okay.

Report CS-3072, Chemistry Division, "Summary Report for June, 1945" was issued today. The material on Section C-I has been covered in meetings held during the month and includes such work as reducing agents for the Redox process, the Berkeley helium ion bombardment of natural uranium, the alpha decay of Pu^{241}, the chemistry of neptunium, of plutonium, of thorium, and of actinium. The report also includes a report on U^{233}.

According to today's paper the Japanese have announced that 200,000 refugees are going to be moved from Honshu Island to Hokkaido to remove them from the path of American planes. Admiral Nomura announced that the U.S. demand for unconditional surrender will result in costing the U.S. higher casualties.

Tuesday, July 10, 1945

I attended a meeting at 8:28 a.m. in the New Chemistry conference room of Groups 1 and 3 of Section C-I. Other attendees were Asprey, Britain, Florin, Ghiorso, Hindman, Hopkins, Hyde, James, Jones, Katzin, Kohman, Magnusson, Manning, Morgan, O'Connor, Peterson, Perlman, Robinson, Scott, Simpson, Studier, R. Thompson, S. G. Thompson, and Westrum. I opened the meeting by asking James to discuss the work in which he, Florin, and Hopkins are engaged. James explained that three uranium bombardments have been received--one is a natural uranium target bombarded with approximately 890 microampere-hours of 44 Mev helium ions (Berkeley bombardment 5b, received at Met Lab June 30); another a U^{238} oxide sample (ratio of 238 to 235-3000) that received a 50-100 microampere-hour bombardment (Berkeley sample 6a, received at Met Lab July 4); and an 87 percent U^{235} oxide sample that also received a 50-100 microampere-hour bombardment (Berkeley bombardment 6b, received at Met Lab July 4). The natural uranium target was milled to remove successive layers, with a ratio of alpha counts between the layers in order of depth being 43 to 7 to 3 to 1.5 to 1. Further data are being obtained. The various fractions from the U^{238} and U^{235} bombardments are in the process of being decontaminated from fission products.

I explained that the information that we hope to obtain from these experiments is the relative yields of the various α,n reactions, the α,p, and the α,pn reactions. The yields of d,n and d,p reactions are also to be determined since there was deuteron contamination of the cyclotron beam.

There was considerable discussion as to the amount of Pu^{240} that would be obtained from this material, and James suggested the use of Scott's electrolysis method to put approximately one-tenth of the Pu^{240} on a plate. I promptly vetoed this suggestion since present difficulties are being encountered with the electrolytic deposition method. It was decided that 0.2 mg of lanthanum be used to prepare the final plate for Ghiorso. I emphasized that the completion of this plate should be given first priority.

I then inquired as to the status of work on the 49NG sample

(Hanford-irradiated plutonium from Farmer's No. 2). O'Connor reported he has been reworking the original salt layer to get out more plutonium that has resisted ether extraction; he now has about 105 mg total plutonium from this sample. There was considerable discussion as to the disposition of this material. I made slide rule calculations on the spot that indicated, on the basis of previous percentage assignments, 54 mg should be sent to Site Y as metal, another 26 mg to Site Y as thoroughly decontaminated material, and 23 mg kept for our use. We decided that the entire quantity of thoroughly decontaminated material (75 mg) should be used for metal production and any amount of metal produced in excess of the 54 mg scheduled for Site Y would be retained here. O'Connor was given the assignment of converting the 75 mg now in nitric acid solution to the oxalate, Florin will make the fluoride, and Westrum will convert it to the metal.

Morgan reported that yields of fission barium in the 49NG material have been determined, but his results are uncertain and he will check them again before reporting.

Hindman reported that preliminary values on the specific activity of the plutonium from the 49NG sample indicate approximately an 11 percent increase in specific activity. I made some calculations (which few present seemed to understand) showing that this value agrees very well with Ghiorso's measurements on the change of fissionability of the materials upon neutron bombardment. It also checks the value of 8,000 years for the half-life of Pu^{240}. We agreed, after some discussion, that Farmer's No. 2 sample must have received about ten times the bombardment received by Farmer's No. 1. The value of five, calculated by Fermi seems quite improbable.

Scott and Britain reported on the preparation of plates by electrolytic deposition; they have encountered difficulty in getting adequate yields of material on the plates. I emphasized the electrolytic deposition process is now becoming the bottleneck of the whole section program and that work on the method should be pushed.

In response to a question from me, Britain said that the U^{237} from the 25NA sample (Hanford-irradiated U^{235} from Farmer's special No. 2) is decaying with approximately the correct half-life.

I mentioned that Dempster is going to study the uranium and plutonium from the 49NA and 49NG samples in order to determine the isotopic composition. Dempster is also going to separate some Pu^{240} from high gt Hanford product.

Hyde reported that he is preparing to handle two targets that should arrive soon--one is the thorium plus deuteron bombardment and the other a target of U^{233} plus helium ions. In the U^{233} bombardment, he intends to look for such things as Np^{233}, Np^{234}, Pu^{234}, Pu^{235}, and Pu^{236}.

Studier said that the bulk of the U^{233} has now been thoroughly purified and set aside for decay.

I brought up the matter of the 10.5 mg of high gt plutonium. It was agreed that this material should be milked for element 95 now and again in about two weeks; this will give Cunningham an opportunity to practice isolation. Asprey will prepare the material for Cunningham.

Stan Thompson reported on his work of the past week on the separation of element 95 from rare earth activities using a 6-foot IR-1 ion exchange column. Results were not quite so good as had been expected on the basis of small column results. Thompson has also worked on the use of organic reagents for the separation of 95.

James said he has not checked the decay of element 95 recently but would do so as soon as time permits. A check now should enable one to place a lower limit of 100 years on the half-life if no appreciable decay is noted.

Peterson reported that his work of the past week has consisted mainly of attempts to remove actinium tracer from large amounts of lanthanum with IR-1 columns.

Roy Thompson said the final result of the assay of the protactinium purchased from Agruss was approximately a disappointingly small amount.

The recalculation of Westrum's calorimetric data indicates a specific activity for Pu^{239} of 70,700 ± 100 counts per minute per microgram. There was considerable discussion as to the best value for the specific activity at this time and whether some official proclamation of this best value should be made.

Robinson reported that the new model of the pulse analyzer has been completed but not been thoroughly tested.

Manning then announced that the hot lab should be finished within another six weeks--maybe. I then told the group that we will have a visitor, Yaffe, from Montreal toward the end of the month. He will be mostly interested in seeing our hot lab facility.

Katzin supplied me with some information to use in my reply to Bill Watson's questions. I say that the weight of isotope 23 in the sample, determined by radio-assay, was about 11.0 mg and we estimate the isotope purity to be 96 percent. The actual weight of the sample was 14.56 mg. I also say we claim no great accuracy.

Last week I received a memo from R. S. Mulliken, Editor-in-Chief of the Metallurgical Project Record, expressing concern about the various values used for the half-life of radium. Today I wrote Dr. L. F. Curtiss of the Bureau of Standards to ask his opinion. I then informed Mulliken of this action. I say that my present inclination is to regard the value 1,686 years as a better value than the value of 1,590 years presently used in the Project Handbook.

In another letter I replied to Dreher's request for advice as to his future activities. I suggest that of the companies he named, the best from the standpoint of research work would be the California Research Corporation. I tell him of my competing offers from the University of Chicago and the University of California, both of which include fair-sized groups to work in this field. I say I do not know whether he would be interested in this sort of position but that it will probably be about six months and perhaps nearly a year before anything definite can be said. I then tell him of Helen's and my vacation plans for Los Angeles (lots of golf) in August.

Daniels, in a memo to Hogness about the budget for the coming year, explains he is trying to reduce, as far as possible, the amount of time spent in making reports. He indicates the Monthly Abstracts will be continued and will be due promptly in his office before the 23rd of each month. Final reports are to be written on any phase of the scientific work as soon as that phase is completed, but it will be unnecessary to continue the long monthly report on all phases of the work.

In an official memo Daniels sent out a notice that the committee of Nickson, Simpson, and Maurer will conduct interviews with the academic staff on social and political implications on Thursday evenings from 7:30 to 9:30, beginning July 12.

I talked with Hamilton in Berkeley by phone. He said they are having trouble with deuterons for bombardment 7a (U^{238} plus deuterons), bombardment 7b (Th^{232} plus deuterons), and bombardment 7c (87 percent U^{235} plus deuterons). I told him to go ahead even if we get only 100 microampere-hours on each. He will do bombardment 7b (Th^{232} plus deuterons) and rush it to us. The same courier will carry 15 milligrams of U^{233} for bombardment 8a (U^{233} plus helium ions--approximately 100 microampere-hours) back to Berkeley. Perlman will arrange this. Bombardment 7a (U^{238} plus deuterons) and bombardment 7c (83 percent U^{235} plus deuterons) will be worked in after bombardment 7b (Th^{232} plus deuterons), and perhaps before bombardment 8a (U^{233} plus helium ions).

Nearly 2,000 U.S. planes attacked Japan over many hours yesterday, according to this morning's paper.

Wednesday, July 11, 1945

Today I received a letter dated July 9 from Hamilton asking me to send one of the platinum disc targets that we made up some time ago. He would like such a target in order to avoid contamination from copper, etc. He said he has had a little difficulty these past few days operating on deuterons (he mentioned this in our phone conversation yesterday); it appears the cyclotron is well constituted to operate on helium ions but does not do so well on deuterons. He thinks this is caused by the presence of much greater circulating ion currents in the case of deuterons with the resultant heating of the deflector and exit strip which in turn makes efficient operation quite difficult.

I read a copy of a July 10 letter from Captain McKinley to Manning in which he indicates that the delay in transmission of the teletype from Hamilton occurred in Berkeley rather than in Chicago.

Manning sent Captain Chapman our analysis of the Pa^{231} content of the sample purchased from the Agruss Company.

A revised organization chart of Section C-I was issued. It shows Kohman replacing Jaffey as Group Leader of Group 2.

Report CN-3053, "Chemical Research Basic Chemistry of Plutonium. Report for Month of June 1945," was issued today. The report includes work on the equilibrium constants in the temperature range 530°C-700°C for the reaction $PuOCl + 2HCl = PuCl_3 + H_2O$. Davidson and Sheft estimate that

$$\Delta H_{298°}\ (\mathrm{kcal}) = -22.2 \pm 0.7,\ \Delta S_{298}(\mathrm{e.u.}) = -33.5 \pm 0.8.$$

It is observed that the equilibrium is much less rapidly obtained in this reaction than in the corresponding bromide reaction.

Bright, shiny, malleable neptunium metal has been prepared by vapor phase reduction of NpF_3 with barium (Fried) on the 50 microgram level. Similar reductions of NpF_4 failed evidently because the NpF_4 soaked into the crucible. The density of one piece of neptunium metal was measured as 17.8 and 17.6 gm/cc in duplicate determinations. Fried reacted a piece of neptunium metal with hydrogen at atmospheric pressure at 50°C to form a black, flaky, hydride $NpH_{3.6-3.8}$.

Fried and Davidson also report on attempts to prepare $NpCl_5$ or $NpCl_6$ by sublimation. These were unsuccessful. However, they did prepare Np_2S_3 by treatment of NpO_2 with an H_2S mixture at 1,000°C.

Hindman and Ames describe the spectrophotometric investigation of Pu(III) in solutions of perchlorate, nitrate, chloride, and sulfate. The investigation places the complex forming order, based on the perturbations produced in the spectrum with increasing anion content, as follows:

$$ClO_4^- < Cl^- < NO_3^- < SO_4^{--}.$$

In all cases the changes in the spectrum are small precluding the possibility of calculating complex ion formulae and association constants from the spectral data alone.

The report also includes studies of the sulfate of Pu(IV) and the mixed sulfates of Pu(IV) (H. H. Anderson and D. C. Stewart) and other work on Pu(IV) sulfates (H. H. Anderson).

After work I played 18 holes of golf with Perlman, Ghiorso, and Morgan at Jackson Park. (IP-110, AG-95, GS-96, TM-99.) We had trouble seeing our golf balls on the 18th hole because of the near darkness.

The Allies have agreed to joint control of Austria, and an allied commission will probably be set up next month, according to today's paper.

Thursday, July 12, 1945

At 8:28 a.m. I attended the meeting of Groups 2 and 4 in the New Chemistry conference room. Also present were Ames, Anderson, Blaedel, Gaarder, Gilbreath, Goeckermann, Hagemann, Hausman, Hyman, Kelley, Kohman, Lawroski, Manning, Perlman, Post, Schaffner, Sedlet, and Weissbourd. Hyman reported on batch experiments to test ferrous ion, hydrazine, and hydroxylamine as reducing agents in the Redox Process. None of the combinations tried was satisfactory. In response to a question from me, he indicated that his future work will be to investigate a precipitate that has been noticed in some of the batch runs and to study the preparation of sulfate-free ferrous solutions.

Blaedel talked about a short run made with the small column using ferrous ion alone. It showed ferrous ion to be quite unstable. Another run is planned in which three streams will be used. The aqueous stream containing ferrous ion and hydrazine will be introduced at the top. The hexone feed will be introduced in the center of the column, and a scrub hexone containing hydrazine will be introduced in the bottom of the column.

Schaffner reported for the Control Analysis Group. He stated that most of the past week has been spent in running samples and there have been no new developments to discuss.

Plans for future meetings were discussed. It was decided to split the meetings into two groups, with a Solvent Extraction Group meeting from 8:28 to 9:30 a.m. and the Control Group at 9:30 a.m. The next meeting will be July 26, two weeks from today.

Morgan completed the determination of the Ba^{140} content of samples 25NA and 49NG (Hanford-irradiated U^{235} and Pu^{239}--part of Farmer's Special No. 2 or CW-2 samples). He calculated that the flux the sample received was 2.37×10^{13} n/cm^2/sec for the Pu^{239} sample and 2.56×10^{13} n/cm^2/sec for the U^{235} sample.

Katzin, Studier, Manning, Cunningham, and I met to discuss the U^{233} for the Koch-Dancoff Experiment. Studier will clean up approximately 30 mg just returned from Zinn sometime before August 10. On approximately August 10, he will take 80 mg from the 280 mg (set up for decay) and extract the Th^{229} and clean it up and give approximately

110 mg to Cunningham or Manning to have Scott or Britain make the plate for Koch. Manning will arrange to have that plate delivered to Koch or Dancoff.

Osborne called me from Maryland to tell me that he thinks it will be difficult to get a release unless we do something about the urgency. R. E. Gibson agrees. They are worried about the draft position of their project. I suggested Osborne come here to visit, and we will discuss it then. I told him that Farrington Daniels is writing R. E. Gibson about the visit. I offered to have him stay in our apartment during his July 26 visit.

In the evening in Room 210, Eckhart Hall, a committee consisting of John Simpson for Physics, R. J. Maurer for Argonne, A. Brues for Health, and A. H. Jaffey for Chemistry, together with Daniels, interviewed members of the academic personnel who wished to express their views on social implications. Appearing before the committee were A. O. Allen, Jerome Brewer, G. R. Carlson, L. C. Furney, Kohman, Rabinowitch, Weissbourd, and L. G. Nierman. A number of others waited to express their opinions but left without being interviewed. Technical matters were not discussed. One man suggested international control of materials for new weapons through the new United Nations Assembly. Another suggested setting up a special corporation in a neutral country to control and develop materials for new weapons. Still another man felt that the United States should keep secret all new developments and continue with an intensive research program in order to keep ahead of other countries. One man suggested that scientists form an independent organization for the purpose of studying social and political implications of nucleonics. The interviews lasted from 7:30 until 10:30.

Tokyo radio says a British task force attacked Sabang off the northern tip of Sumatra on Wednesday, according to unconfirmed reports in today's paper.

<u>Friday, July 13, 1945</u>

I received a letter from Kennedy in which he says he has no objection to my proposal (as given in my July 5 letter), and he feels that Art and Emilio will have none. He thought I might as well write to Sproul since he would appear unwilling to sign the disclaimer without some further argument being presented. Kennedy added he is rather certain that no money will ever be involved and he cannot get worked up over it except to be mad at the other parties.

Doan called me from Clinton Labs to ask whether he should request our thorium carbonate cans. I told him to request "about 100 cans." He will try to put as many as possible in the pile.

I conferred with Dancoff about the betatron at General Electric in Schenectady which produces 100 Mev x-rays with an intensity of 3×10^6 photons/centimeter^2-second two meters from target. They have observed gamma-proton, gamma-deuteron, and gamma-alpha reactions but no fission of lead, bismuth, or thallium. Two Ph.D. physicists (graduates of University of Illinois) are using it. We discussed the Pu^{239}/Pu^{238} ratio in our sample 25NA. He thought the calculated cross section for the $U^{238}(n,\gamma)$ reaction approximately equal to 5 barns is reasonable.

I sent an estimate of our requirements for thorium to Daniels for the period from the present until December 1946. I ask for 500 pounds of metal and 100 pounds of oxide. This assumes that we will irradiate the metal at Site W to produce gram amounts of U^{233}.

I played 18 holes of golf at Jackson Park with Tom Morgan and Roy Thompson. (TM-106, GS-89, RT-100 for 16 holes.)

No startling news on Friday the 13th! Hearings are proceeding on the international charter in the Foreign Relations Committee, and the President is heading for Potsdam for the Big Three meetings.

<u>Saturday, July 14, 1945</u>

Today John Willard returned to work at the Met Lab from Hanford. He is joining Chemistry Section C-II under Hogness.

Fred Albaugh terminated work at the Met Lab. He and Edrey will travel to Los Angeles by car, and he will return to the Union Oil Company. Duane Hufford also terminated today. He is to report to Site Y on the 16th.

In a letter dated July 12, Howland expresses his disappointment that I will be unable to take in the finals of the PGA golf tournament on Sunday with him, Brody, and Niedrach. He gave me his new address and said he would let me know when he can pass through Chicago on his vacation.

Daniels told me that Wheeler is interested in my judgment on the certainty of the assignment of the recently discovered two-to-three year fission product element 61 to mass number 149. Today I wrote Wheeler to say I think it could be assigned to mass number 147 with about the same amount of justification.

The Foreign Relations Committee voted 20-0 to endorse the international security pact. "Fleet Shells Japan" reads the top headline today; U.S. battleships are shelling a steel center on Honshu Island.

Sunday, July 15, 1945

I played 18 holes of golf at Maple Crest Golf Club (Route U.S. 66 at Wolf Road, La Grange) with Winston Manning, Al Ghiorso, and Herman Robinson (GS and HR won a "low ball plus low total" match, 7 and 6. (WM-112, AG-101, GS-95, HR-109.) Ghiorso, Manning, and I then played another 18 holes. (AG-98, GS-90, WM-100.) See Figures 3 and 4.

According to today's Sun, warships and planes are still blasting Japan virtually unopposed.

Monday, July 16, 1945

At 5:30 a.m. mountain time, the first explosion of an atomic bomb occurred at Alamogordo, New Mexico, about 100 miles south of Albuquerque.

Later I read the account of this event that Joe Kennedy wrote in his diary:

> We got to the site around 3:00 a.m., and took our position roughly 20 miles north (?) of the spot. It had been raining, and was completely overcast, in spite of Jack Hubbard's prediction to Parsons (Sunday morning) that it positively would be clear overhead in the North and South directions. We didn't know whether they would fire; Bradbury who was to meet us was there, but didn't know he was to meet us; the radios didn't work except one in an MP car down the road. Finally we learned by MP radio that the shot would be delayed (due to weather) for perhaps one or two hours. Various ones tried to sleep on the ground, etc. CAT, Laurence, and I tried in the sedan, but L. snored awfully.
>
> Don aroused us at about 5:00 a.m., saying the shot would be within five minutes or not that day. Feynman had managed to get one plane, but not the ground station, on our radio. From it we finally guessed that 5:30 a.m. was the zero time. The plane gave signals at 10 minute and at 2 (?), also colored rockets were shot by soldiers at our camp but no one knew the code! A siren could be faintly heard, but again the code was not known.
>
> At zero, I fortunately was facing exactly in the right direction, and with my eyes well-shielded with the dark glass. A brilliant yellowish spurt of fire was first seen, about 1000 feet, in a direction just clockwise of exact vertical, where it mushroomed at the top. At the time of this mushrooming, the total brightness seemed greatest. The ball formed rapidly, grew

Figure 3. Glenn Seaborg, Maple Crest Golf Club, July 15, 1945.

Figure 4. A. Ghiorso, G. T. Seaborg, W. M. Manning, H. P. Robinson, Maple Crest Golf Club, July 15, 1945.

(in a matter of seconds) to one roughly 1000-2000 feet in diameter, and became increasingly less brilliant. At this time, with the ball looking like flame from burning oil, I removed my dark glasses. Before this, while the ball was forming and rising, I looked to the side around the dark glass and the ground and hill appeared as well illuminated as in mid-morning. Likewise, I viewed the sky above. The overcast of stratocumulus (?) clouds was pink on the underside and well illuminated, as at sunrise.

The red and black fireball grew rapidly to several times its size, and rose continuously, reaching about 14,000 feet in about 100 seconds. As it grew, it changed quickly from red and black flames to a mass of white, billowy smoke interspersed and surrounded (to an extent of about 1000 feet beyond the borders of the smoke) by a purple-blue glow, like the low pressure discharge in air in color, and reminding me of the blue glow around hot Po samples but on a much larger scale. As tens of seconds went by, the blue glow diminished and the white smoke cloud became more the prominent feature, until, when the blast wave took me by surprise at about 100 seconds, the blue glow was practically gone.

It was at this time well before sunrise, yet light enough for smoke to be easily visible. It formed a column, not entirely vertical, much narrower at the bottom than at the top, which was dome shaped (from the ball shape of the fireball). Soon after the blast and the end of the blue glow, the dome penetrated the thin cloud haze (at about 17,000 feet), and continued visible above the haze. It appeared to be drifting rapidly northeast (?) at the top. It was perhaps ten minutes later that we began to gather in the cars and buses, and everyone of the party was away shortly before 6:00 a.m.

Some high spots of the trip: (1) the rough "McKee workmen" appearance of the people in the party; (2) the rest stops along the road; (3) Teller, Bethe, and Thomas at about two minutes, applying sunburn lotion to their faces, with Teller saying "100 to 1 it's not needed, but what do we know?"; (4) the utter silence of the explosion until the sound wave reached us; (5) the quiet echoing of the sound in the mountains, audible for about five minutes; (6) the usual guess afterwards of 3-8 kilotons.

We got back to L.A. in the early afternoon, very much worn out.

I received and read Latimer's reply to my letter of July 9. He says,

> You have put your finger on our most important problems.

> In general the University departments of chemistry and physics have not kept pace with the tremendous developments which have occurred in these subjects. There has been no expansion in personnel in our department in the past twenty years. Now on top of this we have the developments in the nuclear field. Chancellor Hutchins at Chicago seems to have grasped the situation. However, when he is willing to increase his offer to you from \$8,200 to \$10,000 and wants to add a dozen new assistant and associate professors in chemistry, I can't help but be encouraged. I am confident that the logic of the situation is such that the proper presentation will achieve the same results here...

He adds that he thinks much of the present project properly belongs to the industrial field. Latimer also mentions that he will be in Tennessee on July 23 and hopes to see me then.

Quentin Van Winkle returned from vacation.

At 7:45 p.m. Katzin spoke on the U^{233} problem at the Chemistry Seminar in Room 251, Ryerson Laboratory.

"Big 3 Parley Begins Today" reads today's headline. Discussions will take place in Kaiser Wilhelm's former castle in suburban Potsdam.

<u>Tuesday, July 17, 1945</u>

The Chicago <u>Sun</u>, Tuesday, July 17, 1945, reports on page 3:

> SHELL DUMP EXPLODES
>
> Alamogordo, New Mexico, July 16 - (UP) - A magazine containing high explosives, gas and other shells blew up at the Army base here today with a pyrotechnic display visible 200 miles away. No one was injured.

Groups 1 and 3 met at 8:28 a.m. in the New Chemistry conference room. The meeting was attended by Asprey, Britain, Cunningham, Florin, Ghiorso, Hausman, Hindman, Hopkins, James, Katzin, Kohman, Magnusson, Morgan, O'Connor, Perlman, Peterson, Robinson, Scott, Seaborg, Simpson, Stewart, Studier, R. Thompson, S. G. Thompson, Walsh, and Westrum. I asked for a report on the status of sample 49NG (Hanford-irradiated plutonium, part of Farmer's Special CW-2) and learned that O'Connor has given 75-77 mg as oxide to Florin; Florin has converted it successfully to fluoride containing 68.5 mg of plutonium and turned it over to Westrum for metal preparation. Westrum said he would have the metal in 24 hours.

Scott reviewed the status of the work on the electrolytic deposition method, and we decided that we need more people to work on this problem. I pointed out that when James, Florin, and Hopkins finish with the alpha targets, some of them will be able to help.

Ghiorso reported pulse analyzer data that show that the plutonium formed in sample 25NA (Hanford-irradiated U^{235}, Farmer's Special CW-2) is 2.4 percent Pu^{240}, which compares with 5.5 percent Pu^{240} in sample 49NG.

Kohman asked whether spontaneous fission measurements have been made on 95^{241}. I replied that they have, but no counts were observed. I explained we are going to take 1/10 of the 95^{241} Cunningham is extracting from the 10 g of high gt plutonium and make a sample for measurements of spontaneous fission, slow neutron fission, and fast neutron fission. I added that after element 95 is again extracted from the 10 g of plutonium we will have plenty--the yield is about 10^7 c/m every two weeks.

I asked how the work on the alpha targets is progressing. James presented the following table giving the preliminary pulse analyzer range curves on the plutonium fractions from the three targets: he indicated the U^{237} and 95^{241} are being isolated and he should be through with nearly all measurements this week. I observed this should enable us to calculate a rough half-life for 95^{241} when the additional measurements of Pu^{241} beta particles are submitted; I calculated that the amount of 4.3 cm range activity in sample BAA agrees with that in TAD.

I mentioned that Hamilton has asked for more isotopes to bombard with helium ions and that we intend to bombard 95^{241} and perhaps plutonium enriched in Pu^{240} as soon as such samples are available.

		Total c/m	Pu^{240} and Pu^{239}	Pu^{238}	Pu^{236}
TAD-I	(1st 0.002 inch)	120,000	5.2%	72.8%	22%
TAD-II	U metal + He^{+2}	5,500	1.2%	75%	23.8%
TAD-III	U metal + He^{+2}	650	-	-	-
TAD-IV	U metal + He^{+2}	400	-	-	-
TAD-V	U metal + He^{+2}	250	-	-	-
QAA	U^{238} + He^{+2}	7,000	14.4%	83.1%	2.5%
BAA	U^{235} + He^{+2}	110,000	-	-	100

Hindman reported there is nothing new on the specific activity of Pu^{240}--the latest value is 8,000 years.

Cunningham next described the isolation of 95^{241} that resulted in 7.2 micrograms of material with a total alpha activity of 4.2×10^6 alpha counts per minutes. Correcting for the Pu^{239} present, he concludes that the half-life is certainly less than 1,000 years. I said I thought 100 to 200 years to be a better value. I suggested that a sample be set up to observe the decrease in the rate of growth of 95^{241}.

I asked if there was anything new on the chemistry of 95, and Thompson told us he has been working on the use of organic complexing agents in solvent extraction. He said it appears that lanthanum is less easily complexed by these organic reagents than are yttrium and 95; also, 95 appears to be more strongly complexed than lanthanum and probably more than yttrium. He said we might predict that the oxalate and phosphate of 95 will be more insoluble than those of the rare earths. Stan plans to try carrying with bismuth oxalate as a carrier from about 4 N acid solution.

Roy Thompson is continuing with the isolation of protactinium; he hopes to isolate 20 to 25 mg by the end of August.

Stewart informed us that the ionium and U^{233} targets are leaving today for Berkeley; I asked that he put a summary of all shipments on my desk.

Katzin suggested that I discuss the importance of measuring the cross section for the absorption of neutrons by Pa^{233} to form Pa^{234}. This loss may block the U^{233} pile if the reaction has a cross section as high as about 500 barns. We hope to settle this question by putting a sample of 10 mg of thorium in a Hanford pile for 30 days, then work up the sample within five days.

Fried discussed his unsuccessful attempt to convert protactinium oxide to the chloride at 550°C with CCl_4; he intends to repeat the experiment using a quartz tube at higher temperatures.

Morgan announced that by isolating barium from samples 25NA and 49NG, he has calculated the nvt of Farmer's No. 2 to be 9 to 10 times that of Farmer's No. 1.

I wrote to Saul Winstein at UCLA to tell him that Helen and I plan to spend our vacation in Los Angeles and that we hope to see him and Sylvia during the first two weeks in August.

In a letter to Latimer at Berkeley, I say that I will see him in Tennessee during the week of July 23. I shall arrive on the afternoon train that day, stay through all or most of Wednesday, July 25. Hogness and I want to try to return to Chicago by plane. I also suggest the possibility of a discussion with President Sproul when I am in Berkeley in August.

I notified Hamilton that we are sending some U^{233} and ionium which we would like to have bombarded with helium ions--about 50 to

100 microampere-hours on each. I tell him that the U^{233} sample (GTS-104) contains 15.7 mg of U^{233} and has an isotopic purity of about 95 percent. The ionium sample consists of about 7.8 mg of Io^{230}; the combined weight of the thorium and ionium dioxides is 35.5 mg. The samples are going by courier via Site Y and should arrive in Berkeley Thursday night.

I point out, in a memo to Daniels, that one of the secondary reactions in the thorium blanket of a breeder pile is the absorption of a neutron by Pa^{233} to form Pa^{234}. The cross section for this reaction is unknown and, if it should be as large as 500 barns, a breeder reactor of this type might not be feasible. I propose to measure this cross section by irradiating 10 g of thorium in one of the Hanford piles in a position of maximum neutron flux for 30 days. The sample should then be shipped to us as soon as possible so that the uranium fraction can be separated from the undecayed Pa^{233} at the earliest possible moment. This will keep the ratio U^{234}/U^{233} at a maximum and make it possible to detect the U^{234} in the presence of U^{233}.

I point out, by memo, to Mulliken that Dr. Curtiss of the Bureau of Standards states, in a letter I received yesterday, that an average of five most recent determinations of the half-life of radium gives a value of 1,636 years, a value that we should probably use.

Perlman sent Spof English at Clinton information for the procedures to follow in requesting a small, solid bowl, centrifuge from Hanford which Bohlmann wants.

Captain McKinley returned the merit increase request for Isadore Perlman to the Met Lab and asked that justification for the proposed increase be furnished.

Hilberry prepared a 24-page document of the suggestions to the Interim Committee for postwar organization. This document was the culmination of panel discussions with scientific personnel of the DSM projects. The report proposed the establishment of a Commission to be called the Commission on Atomic Energy, or possibly the National Nucleonics Commission, and would be a full-time working unit resembling an industry's executive committee. The Commission should consist of five or seven men appointed by the President for five or seven-year terms, with the appointments receiving Congressional approval. Of the five or seven men, two or three should be scientists, two or three should be representatives of the military authorities. The report also proposed an advisory panel of twelve members appointed by the President for six-year terms. Five would be scientists, three would be military, and four would be public representatives. The panel would advise the Commission on all matters of major policy, on budgets, on the appointment of a director, and on the appointment of divisional and special advisory panels. The report visualized the work of the Commission as being grouped in four divisions under a Director appointed by the Commission: (1) administration, (2) scientific and technical research and development, (3) social, political, and economic research, (4) industrial operations.

It was proposed that the research program be carried out by personnel at established universities or industrial laboratories through government grants or contracts. Regional cooperative laboratories should be encouraged, by government aid, for major tasks. The report recommended that industrial development be carried out in industrial laboratories and by the creation of two government-operated development laboratories. Military development should utilize the present ordnance laboratories. A laboratory under military and Commission control should be established.

At 4:50 p.m. I left on a trip to the General Electric Research Laboratory in Schenectady, New York, via NYC RR (Car 104, upper berth 6).

Today's top headline reads "Fleets Blitz Tokyo!" This is reported to be the greatest carrier striking force ever assembled.

Wednesday, July 18, 1945

I visited the General Electric Research Laboratory in Schenectady, New York. Ernest E. Charlton, one of the two assistant directors, is in charge of the laboratory. Mr. Baldwin and Mr. Klaiber described to me some of the work that they are doing on induced radioactivity with the gamma-rays from their electron synchrotron.

Dr. C. Guy Suits, Vice President for Research, described to me the company's plans for the future in the nuclear field and discussed the possibility of my joining General Electric in some responsible role after I leave the Metallurgical Laboratory. He said that they plan to add some 30 to 40 people to their expanded nuclear group, in addition to the 20 who are presently working at Schenectady. They want to try to add a strong theoretical group of about three people, and he has already made an offer to David Bohm. E. O. Lawrence and Hans Bethe will be consultants.

I left Schenectady to return to Chicago via NYC RR (bedroom C - shared, car 376).

Thursday, July 19, 1945

Back in Chicago I found a letter dated July 17 from Howland saying he is leaving on vacation in two weeks. He would like to see me in Chicago on Sunday, July 29. He suggested that he might join me at the Tam O'Shanter Golf Tournament.

I read a reply from Lawrence to my letter of July 9. It was received yesterday. He is not surprised at Chicago's offer to me and thinks it would be a good idea if we see President Sproul when I am in Berkeley. He says, "I have confidence that California will be able to do things on a scale commensurate with the importance of the work."

I also read the memo Kohman wrote on July 18 to Helen McIntyre in Personnel about the duties and abilities of Bernie Weissbourd, a member of the Enlisted Reserve Corp. Kohman said that Weissbourd would be difficult, if not impossible, to replace.

Wayne Johnson sent Captain McKinley a letter in support of a merit raise for Perlman. He cited the letters written by Hilberry and me as justification.

Ernest Lawrence telephoned me from Oak Ridge. He will meet me at his apartment in Oak Ridge at 5:30 p.m. on Monday, July 23. (Apartment 127, A-1, Efficiency Apartments, Tennessee Avenue.) I told him I plan to leave for California on August 2 by streamliner. Lawrence will be in Chicago on his way back to Berkeley on Wednesday, July 25; he plans to come east again around the middle of August.

W. P. Jesse called to ask if we could supply him with some standard samples of plutonium for calibration use. He would like samples of 200, 500, 1000, 3000, 10000, and 20000 disintegrations per minute deposited on one-inch platinum discs. I agreed to arrange for him to get such samples. Jesse said he would send me a formal request tomorrow.

The Committee on Social and Political Implications (Nickson, Jaffey, Maurer, and J. A. Simpson) interviewed interested members of the academic staff who wished to express new ideas for the Panel between 7:30 and 9:30 p.m.

"Bomb Jap Warships" reads the headline this morning. American carrier aircraft discovered Japanese ships hiding at Yokosuka naval base just 18 miles southeast of Tokyo.

Friday, July 20, 1945

Morgan attempted to verify the mass assignment of 96^{242} by determining the growth of Pu^{238} in the 95-96 mixture isolated from the rare earth fraction received from Site Y, Farmer's Special No. 1 (Hanford-irradiated plutonium). He put the sample through two LaF_3 oxidation-reduction cycles and measured the ratio between the alpha and beta-counting rates of the product and by-product precipitations. Morgan concludes that the behavior of the decay product is more like that expected from Pu^{238} than 95^{241} (Ghiorso's range analyses indicate a range of 4.1 cm in air, so the choice is probably between these two alpha emitters.) Although the evidence is inconclusive, he feels that it indicates the postulated mass assignment is correct for 96^{242}.

I wrote a note to Miss Kittredge at the Department of Chemistry at Berkeley. I ask her to send six copies of the "Table of Isotopes" to Dudley Chambers at GE in Schenectady. I also ask that she send us an additional 50-100 reprints since our supply is exhausted.

I sent a summary of the work of Section C-I for the period June 15 to July 15, 1945 to Hogness.

I first discuss the work we have done on the sample of plutonium weighing about 140 mg that was irradiated for a little over six months in one of the Hanford piles. It was very thoroughly analyzed for its heavy isotope content and Pu^{240}, Pu^{241}, 95^{241}, and 96^{242} were found, identified by alpha-particle range measurements on different chemical fractions. The purified plutonium from this irradiation, containing approximately 5 percent of Pu^{240} by weight, was used to determine its specific slow neutron fission rate; the experiments indicate that Pu^{240} does not undergo fission with thermal neutrons. We also used the sample to determine a value for the half-life of Pu^{240} of 8,000 years.

I describe the results of our analysis for heavy isotope composition of a 240 mg uranium foil (approximately 50 percent U^{235}) which received an irradiation of six months in the Hanford pile. We found evidence for the isotopes Pu^{240}, Pu^{241}, 95^{241}, 96^{242}, U^{236}, U^{237}, Np^{237}, and Pu^{238}. I give the reactions by which these isotopes are produced and the extent to which they took place. I give the evidence for U^{236} having a neutron capture cross section within the range of 5-25 barns and a half-life of about 2×10^7 years.

I discuss our work on uranium bombardment with helium ions, mentioning that chemical processing is in progress on a natural uranium target in order to determine the distribution of activities at different helium ion energies. I discuss our work on a target of 87 percent U^{235} and another target of depleted U^{238}. The results so far show that the 4.3 cm isotope, previously assigned to Pu^{236} or Pu^{237}, is formed by reaction on U^{235}, and not by reaction on U^{238}, in agreement with this previous assignment.

I review our work on the chemistry of element 95, discussing our efforts to isolate pure element 95 by carefully purifying about 10 g of 180 gt plutonium and setting it aside for decay to 95^{241} that was extracted after two weeks. Working with this sample, we have determined the half-life for 95^{241} to be less than 1,000 years; current measurements of the decay show that its half-life is greater than 35 years.

I describe the tracer experiments carried out in an attempt to separate 95^{241} from rare earths using Amberlite IR-1 in an adsorption column; separation from rare earths has been definite, but the decontamination factors have not been as high as desired. Some work also has been done on the use of organic reagents to separate element 95 from the rare earths.

I summarize the thermo-chemical measurements made on the heat of solution of plutonium and compounds of plutonium. Work on the investigation of the products produced in the irradiation of U^{233} is continuing.

With regard to the Redox Solvent Extraction Process, I say that work has continued on the reducing agent to be used in the reducing step, or second column. Ferrous ion--hydrazine probably will be satisfactory at the proper rate of flow of the aqueous reducing solution although it may be necessary to introduce additional hydrazine into the hexone feed, either directly into the feed or by means of a scrubber hexone solution containing hydrazine which would be introduced in the bottom of the column. I mention that construction of three of the five glass columns to be used in testing the Redox Process with active solutions has been almost completed. These three columns comprise the equipment necessary for the first cycle of the process plus the uranium recovery and will be put into use as soon as possible. I then describe the uranium ore control analysis work being done for the Mallinckrodt Chemical Company.

I wrote to Howland to tell him I will spend Sunday, July 29 at the Tam O'Shanter and will expect to see him at the golf course.

Hamilton called me from Berkeley. He told me that bombardment 7a (U^{238} plus deuterons) with planned exposure of 250-500 microampere-hours is now on the cyclotron. This will be followed by bombardment 7c (87 percent U^{235} plus deuterons) with a planned exposure of approximately 250 microampere-hours. The courier with the samples will leave Berkeley on the streamliner, "City of San Francisco," next Monday. This will be followed by approximately 100 microampere-hours on bombardment 7b (Th^{232} plus deuterons). He will use a uranium backing plate on bombardment 7c (87 percent U^{235} plus deuterons) and keep it in Berkeley for the present. He will then follow with bombardment 8a (U^{233} plus helium ions) and bombardment 9a (Io^{230} plus helium ions) approximately one week each. These will be sent to Chicago independently--with bombardment 8a (U^{233} plus helium ions) to come by air. A probe target (square block of uranium) present in the deuteron bombardments gets nearly as much exposure as the target. This may cover Stone's needs. Some easy-flo solder is mixed in with the bombarded target.

I played 18 holes of golf at Jackson Park after work with French Hagemann, J. M. Watters, and Steve Lawroski (FH-109, JW-105, GS-99, SL-97).

Helen has decided to go to California ahead of me in order to spend some time with her mother at 1637 S. Van Ness Avenue in Los Angeles and to visit other personal friends before I arrive in early August. She is leaving Chicago by Greyhound Bus tomorrow.

The Senate voted yesterday to join the World Bank. Majority Leader Alben Barkley led the administration forces in the passage of the bill. Senator Robert Taft opposed the bill saying that the bill would cause American costs of postwar aid to skyrocket and wreck this country.

Saturday, July 21, 1945

In a memo to Nickson, I report that Perlman and I do not feel the rooms along Corridor E can be kept free enough from alpha contamination to be used for the analysis of urine samples.

I sent Hamilton some additional ideas that have occurred to me since our phone conversation yesterday for the helium ion bombardments to follow the current deuteron bombardments 7a, 7c, and 7b. I say we would like to have a bombardment of pure thorium with helium ions worked into the schedule--this may be called bombardment 10a. I also ask that this bombardemnt and the Io^{230}-Th^{232} mixture be shipped to us before the bombardment of U^{233} with helium ions (bombardment 8a) because we want to be sure that absolutely no U^{233} will contaminate bombardments 10a and 9a.

I wrote to the Treasurer of the University of Chicago Quadrangle Club saying that I have learned that the high quarterly payments I have been making result from the fact that I hold what is known as a quarterly membership rather than a full membership. I ask that I be transferred to full membership standing and receive a refund for the difference in cost between this type of membership and the type which I have been holding for the period of my association with the Club.

I read a letter I received today from Paneth in Montreal about the naming of the 4n + 1 series. He notes that I seem inclined to call the whole series after its longest living member so far known, the Neptunium series. He hopes that the name remains. Paneth goes on to say,

> In view of the fact that the information imparted by the name should include the family relationship, the chemical element, and sometimes the atomic weight, it seems logical to designate every member of the series by its chemical character and by the prefixed name of the series. Thus the shorthand form would be NpRa, NpAc, etc., and we suggest the written-out and spoken form could be Nep-radium, Nep-actinium, Nep-uranium, etc.; whenever desired, the atomic weight could be added: $NpRa^{225}$, etc.

He believes, in the cases of 87 and 85, the names of virginium or moldavium and alabamine have to be deleted. He asks for my reaction to this terminological proposal. Paneth closes by mentioning that Durham University has asked him to return before the beginning of next term, so he expects to leave Canada in a month or so.

Hogness sent a strong protest to Daniels against a movement by the Army to take our SED men away from us. Any reduction in our staff now through the removal of SED men or otherwise would seriously hamper our program. Hogness points out that in the section on Control Analysis we have Anderson, Ames, and Fineman, all SED men and Weissbourd, an ERC man, together with one other civilian. The Solvent Extraction Group has SED men, Ader, Kelley, Post, Schraidt, and Hausman. The services group consists entirely of SED men--Stewart, Asprey, and Britain. Finally, Hopkins is with the group on Heavy Isotopes, selected because of his innate ability since this group has in it probably the best men in the whole division. Hogness concludes by saying he cannot see how we can spare a single one of these men and that if the Army persists in taking this group it seems to him that it is just about tantamount to asking us to close shop.

J. J. Nickson, J. A. Simpson, and R. J. Maurer of the committee on Social and Political Implications requested that Farrington Daniels provide a well-informed speaker to discuss the Vannevar Bush report. They would also like a second meeting on the organization of postwar nucleonics research. For security reasons they would (1) register all personnel at the door, (2) avoid all discussion of advanced types of piles, exact techniques, and fundamental constants, (3) emphasize organization of nucleonics research and <u>not</u> on the research itself, (4) invite intelligence personnel to attend.

Daniels immediately wrote to Compton asking whether the time has come for an open discussion of the Bush report and whether Hilberry could lecture to the academic personnel. Among other items Daniels also emphasizes that removal of the SED personnel would curtail the efficiency of the Laboratory.

Report CC-3056, "Chemical Research - Extraction and Properties of U^{233}," Report for Period Ending June 30, 1945, was issued today. The report contains a corrected version of the chain of decay products of U^{233}: Th^{229} (3,000-4,000 yr, alpha active); Ra^{225} (ca 26 day, very soft beta-activity); Ac^{225} (10 day, alpha active); 87^{221} (5 min, alpha); 85^{217} (very short, alpha); Bi^{213} (50 min, mostly beta); Po^{213} (very short, alpha); Pb^{209} (3.3 hr, beta); Bi^{209} (stable).

The radiation spectra of U^{233}, U^{235}, Pa^{233}, and Pa^{231} are described. Absorption curves under controlled conditions show the hardest gamma-ray of U^{233} to be 305 kev, with softer gamma rays of about 85 and 40 kev also present. The latter may be highly converted as indicated by the relative abundance of the L x-rays. A gamma-ray of about 170 kev energy has been found in (enriched) 87 percent U^{235} or in the accompanying 0.5 percent by weight of U^{234}. Careful absorption curves on Pa^{233} have given no indication of gamma-rays with energies over about 330 kev, such as a 1.5 Mev gamma-ray. Protactinium-231 has been shown to possess about 12.5 percent of an alpha particle with about 320 kev less energy than that of the main body of alpha particles. The corresponding gamma-ray has been found, and with its aid, a counting efficiency of about 0.5 percent for radiation of about 300 kev energy has been ascribed to the standard argon-alcohol filled Geiger-Müller tube used on the Project.

In other work a description is given of the preparation of thorium oxycarbonate with less than 0.01 ppm of uranium, starting with commercial thorium nitrate. Directions are given, using ceric oxidimetry, for the determination of small amounts of uranium present in large amounts of aluminum powder. Details are given of the preparations for a long-irradiation experiment for the measurement of the neutron capture cross section of ionium. On the basis of interrupted studies, information and predictions are given for the properties of thoria slurries.

The isolation and purification of protactinium from uranium ore residues is described. A refinement of the gel process used in the semiworks is reported. Also discussed is the abnormal chemical behavior of Pa^{231} in process solutions for its recovery from natural sources. The specific activity of protactinium and its half-life have been determined on the assumption that the formula of the ignited oxide is Pa_2O_5. The value obtained is about 35,000 years, slightly longer than the literature value. Crystal studies indicate the ratio of oxygen to protactinium atoms may be less than indicated, giving a longer half-life.

Five airfields in Shanghai are being hit for the second day, according to this morning's paper.

Temperatures in Chicago today reached a high of 94.3°F at 2:45 p.m.

<u>Sunday, July 22, 1945</u>

I played 18 holes of golf at Timber Trails with York, Thompson, and Lawroski. (GS and SL won a "low ball plus low total" match, 8 and 6. ST-105, FY-96, GS-97, SL-98.)

At 11:50 p.m. I left from the Englewood Station to go to Oak Ridge via PRR and L&N RR.

The U.S. has beamed a warning to Japan to surrender immediately to avoid utter destruction. There are also hints that Russia might enter the war against Japan.

Temperatures in Chicago today reached 90.8°F just after 2:00 p.m.

Monday, July 23, 1945

I arrived in Oak Ridge during the afternoon. At 5:30 p.m. Latimer and I conferred with Lawrence in Lawrence's apartment. I told Lawrence I will leave for California on August 2 by streamliner.

I spent the night at the Oak Ridge Guest House.

Tuesday, July 24, 1945

In Oak Ridge. During the morning I participated in a presentation of the Met Lab program to a committee composed of C. A. Thomas, Major Murphy, and A. H. Compton. I was the first speaker on the program, and I spoke for about 30 minutes on the results of the last three to four weeks investigation of heavy isotopes likely to occur in breeder pile operations. I said that two foils were bombarded at Hanford. One contained 120 mg U^{235} + 120 mg U^{238}. The other placed in an equivalent position as far as we know, contained 140 mg Pu^{239}. Thus they can serve as models for the processes in U^{235} and plutonium breeders. The total neutrons to which they were exposed was about 2 x 10^{20}, greater than ever used. At its termination 17 percent of the Pu^{239} was transmuted --5 percent into Pu^{240} and 12 percent into fission products. Eight percent of U^{235} was decomposed by fission. I mentioned the products expected to be formed and told the group what had been observed. I said that we found no Pu^{238} in the plutonium irradiation, showing that fast neutrons had no role in the transformations observed since Pu^{238} can be obtained, by means of slow neutrons, only from U^{235}. From our observations so far, Pu^{240} does not undergo fission by slow neutrons. Element 95^{241} shows no evidence of slow neutron or spontaneous fission. I said that, quoting from memory, the content of Pu^{240} rises from 0.03 to as much as 0.4, and that of Pu^{238} from 0.01 to 0.1 (alpha activity).

Weinberg asked whether new foils were to be irradiated to settle finally the question of thermal fission of Pu^{240}. I replied that we have two more foils that may have been removed too soon, but we hope to be able to answer that question.

Foote then talked about metallurgical problems, in particular, extrusion work. Zinn reported on delayed neutron emission--its period, energy, yield, and origin. His experiments utilized the 250 mg of U^{233} prepared by Clinton and extracted by my group.

In the evening I went to a buffet dinner at the Comptons', and I spent the night at the Guest House in Oak Ridge.

At 2:10 p.m. there was a policy meeting of the Project

Council at Clinton Laboratories, which was attended by Chipman, Compton, Daniels, Doan, Foote, Hogness, Jeffries, Johnson, Latimer, Leverett, Mulliken, Major Murphy, Nichols, Nordheim, Spedding, Thomas, Sinclair, Thumser, Tracy, Warner, Whitaker, Wirth, and Zinn. Compton said that he, Major Murphy, and Thomas were asked by Colonel Nichols to listen, as a committee, to the progress of the various laboratories. He then announced that Site Y is getting along well and, as yet, we have no indication that our product is not satisfactory.

A most important part of the meeting was Nichols' discussion of the release of information for publicity purposes. He mentioned they have assumed there must be a release of news sometime, whether it will be when the weapon is first used or whether it will be forced by some accident or leak of information. He stressed that no information be added to the official releases.

Mulliken reported that the Metallurgical Project Record is behind schedule. The meeting adjourned at 3:10 p.m.

Wednesday, July 25, 1945

In Oak Ridge. I again attended the meeting on breeder piles; Weinberg reported on the Clinton pilot breeder, and Shapiro discussed the catalyst problems. Later reports included English's on the chemistry of reactor solutions, Nordheim's on controls, Coryell's on the physical chemistry of pile solutions, Wollan's on absorption coefficients, Ward's on circulation and cooling, Lyon's on removal of gaseous fission products, and Peterson's on the removal of non-gaseous fission products from U^{235}.

In the afternoon I boarded an L&N train in Knoxville for Cincinnati where I will transfer to PRR to Chicago.

Thursday, July 26, 1945

I arrived back in Chicago at 7:15 a.m.

* * *

On Tuesday while I was in Oak Ridge, Groups 1 and 3 met at 8:28 a.m. in the New Chemistry conference room. Asprey, Cunningham, Florin, Fried, Ghiorso, Hindman, Hopkins, Hyde, Jaffey, James, Katzin, Magnusson, Manning, O'Connor, Perlman, Peterson, Robinson, Scott, Simpson, Stewart, R. Thompson, S. Thompson, Van Winkle, Weissbourd and Westrum attended. Manning opened the meeting by announcing that three Berkeley deuteron bombardments arrived yesterday: thorium

U^{235}, and U^{238}. He also mentioned that Farmer's No. 3 has arrived, but the sample probably will not be worked on until next Monday. Perlman estimated that Farmer's No. 3 has 7 to 8 percent of Pu^{240} and should be good material for helium ion and deuteron bombardments.

Simpson presented a summary of vapor pressure data for plutonium oxides. There is no final interpretation yet. He may rerun oxides with molybdenum rather than tantalum crucibles since the reduction power is less with molybdenum. In answer to a question from Perlman, he indicated Seifert is scheduled to make vapor pressure measurements on uranium and plutonium alloys.

Westrum reported that he has made no recent measurements of heats of formation, but he plans to measure the oxychloride, then trifluoride, and later the oxide.

Studier talked about the present status of work on the 4n + 1 series. He noted that the half-life for Ac^{225} is expected to be as low as 10 days, perhaps even lower. He also talked about the half-life values for Bi^{213}. The counts originally believed to be from emanation are counts arising from atomic recoil.

Manning announced that he has examined all of the 25NA data (Hanford-irradiated U^{235} from Farmer's Special No. 2) and found that the capture cross section of U^{236} is off by a factor of 5 to 6 compared with that calculated from the yield of Np^{237}

O'Connor reported that the decontamination of 49NG (Hanford-irradiated plutonium from Farmer's Special No. 2) is nearly complete.

James observed that further decay data on 95^{241} has placed the half-life at less than 100 years, perhaps 30 to 70 years. Asprey will milk additional 95^{241} on Friday or Saturday--his vacation starts on Monday.

Yesterday the following Berkeley bombardments arrived at 12:15 p.m. on the "City of San Francisco": U^{238} plus 22 Mev deuterons (Berkeley bombardment 7a, Met Lab 28DA), it received 264 microampere-hours between July 20 and July 21; Th^{232} plus deuterons (Berkeley bombardment 7b, Met Lab MDA) which received 100 microampere-hours on July 23; U^{235} (87 percent U^{235}) plus 22 Mev deuterons (Berkeley bombardment 7c, Met Lab 25DA) which received 71 microampere hours between July 21 and July 23.

The two uranium targets were turned over to James at 5:00 p.m. By 10:15 p.m. he completed, for both targets, the separation of the uranium from the neptunium and plutonium. The thorium target was given to Hyde.

My office received a teletype from Hamilton giving the bombardment logs of the three samples which arrived today. He mentioned that much of the U^{238} sample was lost inside the cyclotron so we have roughly 40 percent of the total amount of U^{238} bombarded.

* * *

I attended the meeting of the Solvent Extraction Group at 8:28 a.m. in the New Chemistry conference room. Also present were Ader, Blaedel, Gaarder, Gilbreath, Goeckermann, Hagemann, Hausman, Hyman, Kelley, Kohman, Lawroski, Manning, Perlman, Post, Schaffner, and Schraidt. Blaedel reported on attempts to find satisfactory operation conditions for column 1B (reducing column). Results of a run performed on the small column with ferrous ion and hydrazine as the reducing agent show that, in all probability, this agent will be adequate for use in the 1-inch columns. Blaedel suggested asking Berkeley to study the mechanism of transfer of Pu(IV) from the hexone to the aqueous phase and the rate of reduction of Pu(IV) to Pu(III). I said that I would be in Berkeley next week and asked him to write a memorandum outlining what should be done on the problem.

Hyman described further batch work with various combinations of reducing agents.

Schaffner reported that construction of the first three columns is complete; hexone-water systems will be tried later this week and uranium solutions will be tried next week.

Gilbreath said nine 100-day irradiated, 15-day cooled Clinton slugs will begin to arrive Saturday.

Lawroski indicated the second cycle columns will be finished in another three weeks. I commented that Hogness, Daniels, and I reported the process favorably to Compton and Nichols when we were in Oak Ridge earlier this week. If it is well established in October, there is a chance that its evaluation will justify installation at Site W, if only to replace the present process for a year. I added that, in the event this is not done, the experience gained will be of use in breeder pile processing--the 10^4 kw pile at Clinton could use a solvent process. I announced that there will be another meeting next Thursday in view of the rate at which the columns are progressing.

Cunningham finished isolation of a sample of element 95 (95^{241}) oxide that is fairly pure. It has a specific alpha activity of 3.6×10^6 d/m/μg.

Manning informed Chapman of our need for helium ion bombardments of U^{233}, ionium, and myrnaloy (thorium) in the Berkeley cyclotron as soon as possible. He stated that, after the bombardment of U^{233} is completed, we wish to have the material rushed to us by air in order that we may search for short-lived substances that may be formed.

I talked with Joe Hamilton by phone. He told me that aluminum foil of 0.7 mil thickness (equivalent to 300-400 kev energy) was placed around bombardment 7c (87 percent U^{235} plus deuterons) during the bombardment. He will go on a two-weeks vacation beginning Saturday. Bombardment 8a (U^{233} plus helium ions) will be performed immediately and airmailed to us next Monday. He has received a request from Chapman for 100 microampere-hours, but the sample may receive only 50 microampere-hours in order to finish in time. Bombardment 8a (U^{233} plus helium ions) may arrive on a platinum plate in a test tube.

Hamilton said that bombardment 9a (Io^{230} plus helium ions and bombardment 10a (Th^{232} plus helium ions) might follow a two-weeks bombardment for Warren. Bombardment 9a corresponds to Chapman's request for 100 microampere-hours and bombardment 10a to a Chapman request for 200 microampere-hours.

For bombardment 8a or 9a Hamilton plans to use a U^{238} or natural uranium backing plate. Then correspondingly for bombardment 9a or bombardment 8a, he will use a natural uranium or a U^{238} backing plate.

Captain Chapman phoned me to say that he will okay sending the U^{233} sample by air from Berkeley. It probably will be flown to Chicago on Friday or Saturday of this week. Chapman asked me to give him the schedule for the next few bombardments and the microampere-hours requested for each. This should be done coincident with shipment of the target material or sooner.

The Time Schedule Committee approved a work schedule change for Dorothy Black. She will now work 21 hours per week on an irregular schedule.

My sister Jeanette and Elsie Swanson, a cousin, took the train to Menominee, Michigan, today. Uncle Lawrence Seaborg is supposed to meet and have lunch with them. They will then go to visit Uncle Oscar Erickson (my mother's brother) in Wallace, Michigan.

Darrell Osborne is visiting the Met Lab today and has decided to join the Met Lab in September. In spite of a migraine headache, I had dinner with him and Lawroski at the Baroque Restaurant at 53rd and Lake Park Ave. Osborne is staying at the Miramer Hotel.

I completed a letter to Helen, who is now in California, before retiring.

The Japanese say that enemy troops have landed on Puket Island off the west coast of the Malay Peninsula; but, according to this morning's paper, there has been no U.S. confirmation.

The heat wave that started before I left Chicago continued with temperatures reaching the year's high of 99.4° F on Tuesday the 24th. On Wednesday morning it rained bringing the temperatures below the 90's and bringing relief prior to my return.

Friday, July 27, 1945

I received a letter dated July 25 from Howland. He said he hopes to arrive in Chicago early enough next Sunday to catch us before we leave for the golf course. He will phone when he arrives in town. Howland also mentioned that La Chapelle had his tonsils removed on Tuesday.

Radiation surveys by the Health Division for the week ending July 27, 1945, showed the following rooms to have high radiation levels: Room 4, Room 10, Room 11, Room 13, Room 30, and Room 37.

I conferred for one hour with Chancellor Robert Hutchins, President Colwell, and Vice President Gustavson of the University of Chicago. They urged me to accept a Full Professorship at the University at $10,000 per year.

Lawroski, Manning, Osborne, and I went to the Tam O'Shanter Golf Tournament. We followed Nelson who shot a 68.

Jeanette is visiting with our Uncle Oscar Erickson in Wallace, Michigan, until tomorrow.

Clement Attlee has become prime minister of Great Britain.

The Allies issued an ultimatum to Japan for its unconditional surrender.

Saturday, July 28, 1945

I read a July 20 memo from Perlman, suggesting that the contribution to the total Pu^{240} found in pile material by neutron capture by Np^{239} can probably be obtained by examination of Hanford material produced under varying conditions. He points out there is material from Site W available in which the flux values differ by 25 percent with irradiation time of about 150 days. In such material the amounts of Pu^{240} should differ by 8.5 percent if the capture cross section of Np^{239} is 5,000 barns, and by 1 percent if the cross section is 500 barns.

Perlman and I prepared a summary of the amount of heavy isotopes formed in pile irradiation:

Pu^{239} ppm in U (gt level)	Pu^{240a} alpha yield %	Pu^{240a} wt yield %	95^{241} alpha yield %	96^{242c} alpha yield %	Pu^{238} alpha yield %	Pu^{238} wt yield %
100	2.1	0.7	0.005	0.0035	0.3	6×10^{-4}
250	5.1	1.7	0.03	0.06	0.8	0.0015
400	8.2	2.8	0.08	0.2	1.2	0.025
550	11	3.8	0.15	0.6	1.7	0.0035
800	16	5.5	0.3	1.8	2.5	0.005
1000	21	6.9	0.5	3.5	3.1	0.0065
1500	31	10	1.1	12	4.5	0.0095
2000	41	14	1.9[b]	30[b]	6.1	0.012

[a]The half-life of Pu^{240} is about 8,000 years.

[b]If a 1,000 d irradiation is taken to reach 2,000 gt and is followed by a 60-day cooling period, the amount of 95^{241} activity will increase to about 5 percent while the 96^{242} activity will about double.

[c]The half-life of 96^{242} is taken as 6 months for these calculations.

Westrum sent Brewer in Berkeley information on the purity of the plutonium metal used in his measurements of the heat of solution. He also described the procedures he followed in handling the material to prevent oxidation.

Perlman has gone to Site W on Project business. He is scheduled to return to Chicago on August 3.

Robinson, Thompson, Ghiorso, and I went to the Tam O'Shanter Golf Tournament. We again followed Nelson, this time in a terrific rain. He shot a 33 for nine holes, and then the round was declared off. I had dinner at Stan Thompson's home.

Jeanette and Elsie are scheduled to travel back to Menominee today.

I wrote to Helen before going to bed.

Planes from British and American carriers have assaulted the Kure and Kobe area during the past two days, according to today's paper.

Sunday, July 29, 1945

Thompson, Howland, and I went to the third round of the Tam O'Shanter Golf Tournament.

Jeanette is now visiting our Uncle Lawrence Seaborg in Menominee. She will stay there until tomorrow.

According to today's Sun, the Senate has ratified the United Nations Charter, departing from its historic isolationist policy.

Monday, July 30, 1945

Asprey started his vacation today.

Two men from Chalk River, Leo Yaffe and Leslie Cook, are visiting the Met Lab. They are here to look at the counting room facilities and the hot lab, and to discuss measurement techniques. I introduced Yaffe to Art Jaffey, saying that the two are almost isotopic and virtually the same weight. Jaffey showed them the counting room facilities and described our measurment techniques. I took the two men to the Del Prado Hotel at the corner of Hyde Park and 53rd St. for lunch. It was a stifling hot day so I left my jacket in my office although Yaffe and Cook were more formally dressed. At the dining room I was told that I could not enter without a jacket. Fortunately I was offered a jacket belonging to one of the waiters in order that I might fulfill the dress requirement.

In the afternoon Cook talked to some of the members of the Solvent Extraction Group about Chalk River's work on the solvent extraction of plutonium to separate it from fission products. He is using triglycol dichloride as a solvent for plutonium. Unfortunately, for security reasons, we could not discuss our work in solvent extraction with him.

Hamilton teletyped me to say that the courier left on the streamliner yesterday with the thorium metal target that has received a total bombardment of 20-35 microampere-hours of helium ions.

Russell R. Williams at Clinton wrote to inquire about some early work of ours (Kennedy's and mine). He mentions that we reported that when Te^{127} or Te^{129} in telluric acid undergoes isomeric transition, it is reduced to tellurous acid in yields of approximately 100 percent. He has studied the Te^{129} isomer and has obtained no reduction greater than 55 percent. He asks for details of our experiments.

Jeanette arrived here from Menominee today to stay with me. This evening she ate an informal dinner at the Ghiorsos' home while I had dinner at the Mayflower Hotel with Yaffe and Cook, our visitors from Chalk River. Before I went to bed, I wrote a letter to Helen.

Japanese Premier Suzuki scorned as unworthy of official notice the allied Potsdam surrender ultimatum, according to this morning's paper.

Tuesday, July 31, 1945

The meeting of Groups 1 and 3 was held in the New Chem conference room at 8:28 a.m. It was attended by Britain, Cunningham, Fried, Ghiorso, Hindman, Hopkins, Hyde, Jaffey, James, Magnusson, Manning, Morgan, Peterson, Robinson, Scott, Simpson, Stewart, Studier, Roy Thompson, Stan Thompson, Van Winkle, and Westrum. I opened the meeting by announcing that Nickson wants everyone to take alpha hand counts for the week of August 6 for statistical studies.

Next, I suggested we discuss the work on CW-2 (Farmer's No. 2) and then discuss the plans for CW-3 (Farmer's No. 3). I asked about 49NG (Hanford-irradiated plutonium, part of CW-2), and Morgan said that 19.4 mg equivalents of decontaminated plutonium were sent to Site Y and 3 mg were kept here.

I then read the list of 21 problems that we have been considering for the CW-2 samples. We discussed them and decided to do essentially the same things on the CW-3 samples. I then summarized the plans for CW-3: little or no metal will be made and we may not decontaminate so large a sample--the same people will make the same measurements as they did on the CW-2 samples. In answer to questions I said there are no more Farmer's samples in the pile and that the gt level of CW-3 will be 1.5 times that of CW-2.

The next topic was the electrolytic preparation of samples. I suggested that all spare effort be put on this because it is holding up all other work and, if we cannot do the last step in the sample preparation, all our involved chemical procedures are wasted. After the ensuing discussion, it seemed that satisfactory progress is being made, so I told Britain and Scott to continue working until reproducible results can be obtained. I recommended that others learn the techniques because many people are going on vacation. We decided that James will set up an electrolytic apparatus in Room 28.

James then gave the results of absorption curves on the neptunium fractions from Berkeley deuteron bombardments on depleted uranium (Berkeley 7a, Met Lab QDA) and 87 percent enriched U^{235} (Berkeley 7c, Met Lab BDA). I also led discussions of the work on and further plans for samples TAD (uranium metal plus 44 Mev helium ions - 87 µah), 25AA (U^{235} plus helium ions - 70.8 µah), 49AB (plutonium plus 40 Mev helium ions - 63.1 µah).

I also talked about the target of U^{233} plus helium ions. This should arrive Wednesday or at least by Friday since it is to be flown here by an Army plane. Hyde will take the neptunium and plutonium fraction as quickly as possible for examination by Ghiorso and Robinson. I mentioned that in September bombardments will start on Np^{237}, Pa^{231}, and more ionium.

In answer to a question of Hindman, I announced that another Np^{237} run has been approved for Hanford, probably in late August.

I brought up the discussions we had with Yaffe and Cook about the use of triglycol dichloride as a solvent for plutonium and suggested consulting Lawroski, Blaedel, and Gilbreath on the possibility of doing some work with it.

There was a discussion of data and calculations about the Pu^{240} content of the plutonium from 49NG (Hanford-irradiated plutonium from Farmer's No. 2). I asked Ghiorso, Hindman, and Westrum to prepare a table showing their conflicting measurements on this material for my use in writing a letter to Allison. After I get their data, I said I will go into a trance in order to sort them out for Allison.

I then called on several of the other men to report and learned that (1) Van Winkle has isolated 900 mg of ionium. (2) Stan Thompson finds the best separation of 95 from yttrium is by precipitation of bismuth oxalate from alkaline solution of ammonium oxalate. When he uses the solvent extraction method, element 61 behaves very much like element 95. (3) Peterson has found IR-1 resins unsatisfactory for separation of actinium from macroscopic amounts of lanthanum. (4) Simpson will need at least 5 mg of 95^{241} to determine its vapor pressure relative to that of plutonium. (5) Fried has prepared a volatile chloride by treating protactinium hydroxide with carbon tetrachloride. At this point, the material for discussion and the attendees were exhausted; the meeting was adjourned.

Allison teletyped me from Site Y asking for more information on samples GTS-101 and GTS-102 than I provided in my letter of July 6.

I sent a teletype to Allison that we are sending him 17.5 mg of plutonium, identified as sample GTS-107. It will leave by courier tomorrow.

In a letter to Latimer, I ask whether any of his men would be interested in undertaking a somewhat long-range study of the kinetics of the second step in the Redox Process, which is not adequately understood at present. It involves the re-extraction of the Pu(III) into a salt solution containing reducing agents.

In another letter (airmail, unclassified) to Latimer, I announce that I am leaving here on the streamliner on Sunday, August 5, and will arrive in Berkeley on Tuesday morning, August 7. I say that I would like to stay in Berkeley until Saturday, August 11. Then I plan to go to Los Angeles. I ask that Mrs. Moquin or Miss Kittredge obtain reservations for me on the "Daylight" to Los Angeles for the 11th.

I also wrote to Melvin Calvin to tell him of my plans. I ask him to find a place for me to stay.

I then phoned E. O. Lawrence in Berkeley to tell him about my forthcoming visit.

James completed lead and aluminum absorption curves on the neptunium fractions from sample 25DA (U^{235} plus 22 Mev deuterons - Berkeley bombardment 7c) and from sample 28DA (U^{238} plus 22 Mev deuterons - Berkeley bombardment 7a). For both samples the lead absorption curves showed a hard gamma-ray of 1.5 Mev. The aluminum absorption curve was that expected from Np^{238}, Np^{239} mixtures.

Hilberry sent a letter to General Groves attaching the following compilation of reports submitted to his office in connection with the recent panel discussions of the field of postwar nucleonics. Hilberry indicates that Compton has requested the list be transmitted to the General for his use.

Organization by W. Bartky

Significant relevant facts by J. C. Stearns

Memo to W. Bartky from L. Szilard

Memo to W. Bartky from W. H. Zinn (MUC-WHZ-217,3A)

Memo to W. Bartky from T. R. Hogness (MUC-TRH-266,1A)

Memo to W. Bartky from A. J. Dempster (MUC-AJD-77,3A)

Letter to AHC from M. D. Whitaker - Organization and Program for Nucleonics, 1A

A National Program for Postwar Research and Development by R. L. Doan, 2A

Memo - Organization of the National Nucleonics Program - by M. D. Whitaker et al., #5

Memo to A. H. Compton from H. J. Curtis - Organization of the National Nucleonics, #14

Suggested Organization and Program for Nuclear Energy by Latimer (MB-WML-114,3A)

Letter to Hilberry from Spedding, 1A

Letter to Hilberry from Hamilton, 1A

Memo to K. S. Cole from A. M. Brues - Future of Metallurgy (MUC-KSC-493,1C)

Nucleonics Health Problems - A Summary - by R. S. Stone, 2A

Memo to L. O. Jacobson from J. J. Nickson - Organization of Health Protection Program for Work in Nucleonics (MUC-HG-1085,2A)

Memo to Medical Files from J. E. Wirth - The Need for a Health Organization, #2

Memo to Hilberry from Dempster, Hogness, Bartky, Stearns, Jacobson - Suggested Statutory Controls (MUC-ADJ-73,1A)

Information, Education, Publication and Security (MUC-RSM-420,2A)

Report of the Committee on Production Problems (MUC-JPH-345,1A)

Report of the Research Program Committee (MUC-WHZ-218,7A)

Memo to R. S. Stone from H. J. Curtis and R. E. Zirkle - Post War Biology Program, 1B

Memo to R. S. Stone from P. S. Henshaw - Suggestion for Nucleonics Biology, 2B

Suggested Postwar Nucleonics Program by H. J. Curtis, R. E. Zirkle, J. R. Raper, P. S. Henshaw, 2A

Proposed Postwar Program of Research in Nucleonics Biology by H. J. Curtis, 2B

Memo to M. D. Whitaker from J. E. Wirth - Proposed Work Outline for Health Division for 1945 and 1946, 2B

Research Program - Biology and Medicine (MUC-KSC-500,3A)

Some of the Problems Confronting Health-Physics by K. Z. Morgan, 2B

Letter by J. G. Hamilton, 2A

Report of Committee on Political and Social Problems

Memo to J. Franck from W. H. Zinn (MUC-WHZ-216,4A)

Memo to AHC from M. C. Leverett - Use of Atomic Power (MUC-JHP-346,4B)

Jeanette cooked dinner for us. We spent the evening at home talking about her trip to Menominee, Ishpeming, and Wallace, and all the relatives she saw on the trip.

According to this morning's newspaper, U.S. destroyers are only 80 miles southwest of Tokyo. They shelled the town of Shimizu.

It was 98.7°F at 4:45 p.m. today--a record high for this date--but a heavy rain and electrical storm cooled the atmosphere.

AUGUST 1945

Wednesday, August 1, 1945

The organization of Section C-I as of August 1, 1945, is as follows:

Glenn T. Seaborg - Section Chief
Ruth P. Rogers - Secretary to Seaborg
Kathleen Florin - Clerk

Winston M. Manning - Associate Section Chief
Isadore Perlman - Associate Section Chief
Jane Horwich - Secretary
Dorothy Black - Secretary, part-time from Information Office
Mildred A. Bolden - Secretary, on loan from Information Office
Norma D. Shaw - Draftsman, on loan from Information Office

Group 1, Heavy Isotopes,
No group leader
Cunningham, Burris B. - Research Associate
Ghiorso, Albert - Research Associate
Hindman, Clark J. - Research Associate
Hyde, Earl - Research Associate
Katzin, Leonard I. - Research Associate
Jaffey, Arthur H. - Research Associate
James, Ralph - Research Associate
Magnusson, Lawrence - Research Associate
Morgan, Leon - Research Associate
Peterson, Sigfred - Research Associate
Robinson, Herman - Research Associate
Simpson, Oliver C. - Research Associate
Studier, Martin [SED] - Research Associate
Thompson, Roy C. - Research Associate
Thompson, Stanley G. - Research Associate
Westrum, Edgar F. - Research Associate
Florin, Alan E. - Research Assistant
Hausman, Eugene A. [SED] - Research Assistant (half-time)
Hopkins, Horace H. [SED] - Research Assistant
O'Connor, Paul - Research Assistant
Scott, Benjamin - Research Assistant
Van Winkle, Quentin - Research Assistant
Walsh, Patricia - Research Assistant
Erway, Norman - Technician
Thomson, Helen - Technician

Group 2, Control Analysis,
Kohman, T. P.- Group Leader
Anderson, Herbert H. [SED] - Research Associate
Ames, Donald P. [SED] - Research Assistant

Fineman, Phillip [SED] - Research Assistant
Sedlet, Jacob - Research Assistant
Weissbourd, Bernard [SED] - Research Assistant
(inducted into Army 8/1/45)

Group 3, Services,
Stewart, D. C. [SED] - Group Leader
Asprey, Larned B. [SED] - Research Assistant
Britain, J. W. [SED] - Research Assistant

Group 4, Solvent Extraction,
Lawroski, S. - Group Leader
Gilbreath, J. R. - Assistant Group Leader
Blaedel, Walter J. - Research Associate
Hagemann, French T. - Research Associate
Hyman, Herbert H. - Research Associate
Schaffner, Irwin J. - Research Associate
Ader, Milton [SED] - Research Assistant
Gaarder, Sydney - Research Assistant
Goeckermann, Robert - Research Assistant
Hausman, Eugene A. [SED] - Research Assistant (half-time)
Kelley, Alec [SED] - Research Assistant
Post, Roy [SED] - Research Assistant
Schraidt, John H. [SED] - Research Assistant
Boykin, Pearline - Technician
Giacchetti, Olga - Technician

Eugene P. Wigner left the Met Lab to join Princeton University.

About noon, a Berkeley bombardment of thorium metal plus helium ions (20-35 microampere-hours) arrived in Chicago on the "City of San Francisco."

In a note entitled, "High Order Products from Intense Neutron Irradiation of Uranium Isotopes," Manning and I summarize the results of our work on a sample of uranium foil containing about 240 mg of uranium, composition about 50 percent U^{235} and 50 percent U^{238}, irradiated for a little more than six months in one of the Hanford piles (CW-2). We point out the data have direct applicability to the operation of high energy converter piles using such a mixture of uranium isotopes.

We report that alpha-particle range measurements, made with the pulse analyzer apparatus, of different chemical fractions, was the means of identifying some of the isotopes. The evidence for the isotope 96^{242} consists of the observation of alpha particles of range 4.75 cm assigned to the isotope 96^{242} in previous work. The amount of Pu^{240} was determined by spontaneous fission measurements.

We include the following table showing the proportion of various isotopes present at the end of the irradiation period:

Isotope	% of Total Alpha Counts at End of Irradiation	% of Total Weight of Heavy Isotopes	Weight (mg)
U^{234}	33.4	0.4	0.9
U^{235}	1.3	48	110
U^{236}	~1.0	0.6	1.4
U^{237}	(1.5×10^{11} beta c/m)	0.0004	0.0009
U^{238}	0.2	51	118
Np^{237}	0.0034	0.0003	0.00073
Pu^{238}	4.6	0.00001	0.000028
Pu^{239}	54.1	0.06	0.133
Pu^{240}	4.9	0.001	0.003
96^{242}	0.18(assuming 6 mo. half-life)		

The results of our preliminary determination of the n,γ cross section of U^{236} appear to lie within the range of 7 to 35 barns. We note we have made similar measurements on a sample of plutonium weighing about 140 mg [CW-2, Met Lab 49NG] that was bombarded at the same time as the U^{235} sample, and have identified the isotopes Pu^{240}, Pu^{241}, 95^{241}, and 96^{242}.

We also provide data to show the proportions of various isotopes that would be present at the end of a five-fold longer irradiation period.

I asked Daniels, by memo, to arrange for the neutron irradiation of 1 mg of Np^{237} in one of the Hanford piles in a position of maximum of neutron flux for a period of 90 days. Such a neutron irradiation is important because substantial quantities of the isotope are produced in any converter pile operating on U^{235} and in breeder piles operating on U^{233}. It is, therefore, important to study all the isotopes formed under very strong neutron irradiation of Np^{237}. The irradiation will give us a method to produce pure Pu^{238} for study of its fission properties.

In another memo to Daniels, I summarize work we have done on the slow neutron fissionability of Pu^{240} using the Pu^{239}-Pu^{240} mixture formed as a result of the bombardment of about 140 mg of Pu^{239} at Hanford for six months [Met Lab 49NG]. I describe slow neutron fission tests made with the aid of the slow neutrons in the thermal column of the Argonne D_2O-uranium pile using samples of this isotopic mixture. I say the conclusion to be drawn at the present state of the experiments is either that Pu^{240} does not undergo fission with slow neutrons or does so with a cross section smaller than some 15 to 20 percent of that of Pu^{239} for the same reaction. I tell Daniels of our plans to check these measurements using another Pu^{239}-Pu^{240} mixture containing about 50 percent more Pu^{240} formed as the result of a 50 percent stronger neutron irradiation.

I also say these studies have led to a value of about 6,000 years for the half-life of Pu^{240}, compared with our earlier value of about 8,000 years.

I wrote to Lavender stating Kennedy, Segrè, Wahl, and I are now ready to sign the agreement about Cases 52 and 61, as drafted by Lavender on June 2, 1945, subject to approval of this action by the University of California. I point out, however, that the May 18, 1945 letter from President Sproul indicates that the Regents are not ready to agree to the present proposals until more information is available. I suggest that, in the interest of saving time, Lavender should send the "Certificate of Disclaimer" to President Sproul, asking for the proper signature; in the meantime we will continue to try to supply the University with any additional information the University feels it needs.

I sent a carbon of this letter to Kennedy at Site Y. I observe that it is really becoming difficult to know just how to carry on in this matter. I say I shall probably write to Sproul after he returns from Europe and ask for suggestions as to how to carry on.

In a teletype to Allison at Site Y, I ask if he yet has mass spectrographic values for the 40-49 ratio or 26-25 ratio in sample CW-2.

I also replied to Russell Williams' letter of July 26. I tell him that our chemical procedure is described in the article by Seaborg, Livingood, and Kennedy in Phys. Rev. 57, 363 (1940). I say that we cannot claim high accuracy for our experiments, but believe that our amount of reduction exceeded his 55 percent. I also mention that I am sending a carbon of my letter to Kennedy and Friedlander in order that they may send him (Williams) any additional information they may recall.

W. A. Felsing of the University of Texas asked me, in a letter dated July 21, for suggestions for an Assistant Professor in chemistry. Today I replied that I have not been in contact with the type of man he desires, but I suggest B. B. Cunningham, I. Perlman, and (his own) R. C. Thompson as possibilities. I also mention that I am certain George Watt has also thought of these men.

Hogness has prepared a summary of the manpower distribution in the Chemistry Division as of August 1. It shows the following for my section:

	Heavy Isotopes	23
Section	Kohman, Control Analysis	6
C-I	Stewart, Services	3
Seaborg	Lawroski, Solvent Extraction	12
	Administration - Seaborg, Manning, Perlman	3
	Total	47

Perlman called me from Hanford. He said that John Wheeler is interested in finding out what value we have obtained for the cross section of the $U^{236}(n,\gamma)$ reaction. I replied that we have observed a cross section of 25 barns. He then said that Sullivan has measured four years for the half-life of Am^{241}.

Perlman mentioned that the special run to produce and separate a large batch of Np^{237} will start on August 10 or 12. He also said that Wheeler has told him that a new neutron bombardment of thorium to produce approximately 5 to 10 grams of U^{233} seems feasible.

Jeanette and I had dinner at the Ghiorsos'. I then wrote to Helen before I went to bed.

The predicted temperature of 100°F for today fell short with a high of 90° at 1:00 p.m.

<u>Thursday, August 2, 1945</u>

At 8:28 a.m. I held a meeting in the New Chemistry conference room of the Solvent Extraction Group. It was attended by Ader, Blaedel, Gaarder, Gilbreath, Goeckermann, Hagemann, Hausman, Hyman, Kelley, Lawroski, Manning, Post, Schaffner, and Schraidt. Hyman reported on batch work in the solvent extraction process and indicated that Ader is investigating fission product decontamination while Goeckermann is studying the problem of how best to introduce hydrazine into hexone. Hyman discussed the experiments carried out to determine what method will be most suitable for oxidation of the product stream from column IB.

Blaedel described the analytical apparatus for control work being constructed and said that the analytical methods to be employed are also being studied.

Schaffner reported that the first three 1-inch columns have been tested with both water and hexone. Initially, the flows were not smooth, but this difficulty has been corrected. I asked about the schedule for actual runs and was told that column operations will be observed first with normal solutions containing no product nor fission product elements. The flow rates will be fixed and the flooding rates will be determined, then plutonium will be put in the feeds.

The next meeting was scheduled for next Thursday.

I replied to Professor Paneth's letter of July 14 on the question of nomenclature for the new heavy radioactive isotopes. I said that I agree with his ideas, but I would prefer the series use the prefix "Neptuno" rather than "Nep"; however, I would like to defer a decision. For the 4n + 1 series I say that my present inclination is to adopt "Neptunium Series." I agree with his idea that the names "Virginium" and "Alabamine" should not be perpetuated, and this, of course, is true for "Illinium" and "Masurium."

I played golf with Lawroski and Thompson at Jackson Park after work.

Jeanette and I had dinner and spent the evening at home. I then wrote to Helen.

This morning's paper carries the story of the shelling and bombing of Wake Island.

Friday, August 3, 1945

Morgan, at James' request, took soft electron absorption curves on the neptunium fractions of samples 25DA (U^{235} + 22 Mev deuterons - Berkeley bombardment 7c) and 28DA (U^{238} + 22 Mev deuterons - Berkeley bombardment 7a). In 25DA he finds electrons with about a 5-day half-life. He does not find these soft electrons in 28DA.

Perlman returned from his trip to Site W at 1:00 p.m.

I wrote to Segrè at Site Y about nomenclature of the members of the 4n + 1 family. I tell him of my correspondence with Paneth. I say I think eventually it will be up to him, Corson, and MacKenzie to propose a name for element 85 and for him and Perrier to propose a name for element 43. I also suggest that perhaps Mlle. Perey should suggest a name for element 87. I explain I am interested in learning his reaction since I have been asked for advice on terminology by many people.

I wrote a "letter of recommendation" for Ed Orlemann to Professor Edward O. Holmes, Jr., at Boston University. Holmes requested information about Orlemann in a telegram today. I say that I have an extremely high regard for Orlemann and that he is an exceptionally good research man and has a very thorough training in and understanding of inorganic and analytical chemistry. I go on to say, "Dr. Orlemann is a forthright person and conducts his work with a determination to carry through to a successful conclusion that which he starts."

I summarized for Allison at Site Y the information on the fractions we sent him as a result of our work on the CW-2 samples (samples GTS-101, GTS-102, GTS-106, and GTS-100). I gave him the tentative results of a number of our measurements made in connection with the processing of this material, including (1) the finding in the U^{235}-U^{238} sample of isotopes U^{237}, Np^{237}, and Pu^{238} in such amounts as to permit us to identify the reactions by which they are formed; (2) the tentative values for the effective cross section of the n,γ reaction on U^{236}; (3) the identification of Pu^{239}, Pu^{240}, Pu^{241}, and 96^{242} in the U^{235}-U^{238} mixture; (4) our tests for the slow neutron fissionability of Pu^{240}, using the CW-2 plutonium sample (Farmer's sample no. 2).

I then refer to Allison's earlier statement that he has evidence for the production of a greater proportion of Pu^{240} in the plutonium manufactured in the Hanford piles than he can calculate from the fundamental constants involved. I suggest the possibility that this could be accounted for by the n,γ reaction on Np^{239} followed by beta decay. I then point out that a cross section of a couple of thousand barns would seem to be adequate.

I sent a letter to John Beal, Secretary, The Quadrangle Club, asking that my membership be discontinued since I consider the quarterly membership basis (as in my case of 13 quarters) to be an unfair arrangement for me.

Jeanette got up early in order to meet her friend Inez Wyman at 5:30 a.m. at the Greyhound Bus Depot.

In the evening Jeanette cooked a "pastie" dinner for Inez, herself, and me. Jeanette and Inez then went out for a walk while I wrote to Helen.

The full text of the Big Three Potsdam conference announcement was carried in this morning's paper. Important items include the fixing of Poland's western borders, creation of the mechanism for writing peace treaties, and reparations from Germany and the prohibition of any military industry for that nation.

<u>Saturday, August 4, 1945</u>

I wrote to Reuben G. Gustavson at the University of Chicago, at his request, to give my ideas as to the constitution of a group to work in the nuclear chemical field in the proposed Institute of Nuclear Studies. I say I believe that eventually there should be about a dozen men in the associate and assistant professor class, but I recommend starting with half of that number, supplemented by about ten post-doctorate men who would serve as a future source of staff members in arriving at the indicated strength. I also propose that at the start there should be about ten graduate students of an advanced type, that is men who have had several years of experience in this field on war research programs. I go on to propose the name of two men he would probably want, irrespective of the other appointments--Dr. I. Perlman with the rank of Associate Professor and S. G. English with the rank of Assistant Professor.

I say I would appreciate his considering the contents of my letter only as my personal ideas about how a chemical staff could be built up and not as a commitment on my part to accept a position with the University of Chicago.

Perlman sent a memo to Daniels stating the production of about 10 grams of U^{233} in any of the Hanford units is entirely feasible and should not upset operations as currently carried out there. He says that the preparations required for the bombardment are the processing of over 80 kg of thorium metal free to the extent of 1 ppm of uranium, the determination of an adequate method for canning the thorium, and the carrying out of the operation.

Captain Chapman of the Area Engineer's Office phoned to give me information on CW-3 samples B and M including the daily log on these bombardments.

The U.S. and Great Britain have agreed at Potsdam on new blows to speed the surrender of the Japanese in the Pacific, according to reports in this morning's paper. But at same time Japan sank one light American vessel and damaged another in air attacks on Okinawa.

Sunday, August 5, 1945

I wrote a letter to Helen early in the morning, then I played 18 holes of golf at Evergreen with Stan Thompson and Steve Lawroski. Jeanette and Inez came out, picked us up at the twelfth hole and followed us around the course. (SL-107, ST-104, GS-102.) Then the three of us went to the train station where I caught the "City of San Francisco" at 6:00 p.m. for Berkeley. My reservation was for bedroom O, car 128.

The top headline in today's Sun reads "B-29s Doom 12 More Cities." Fleets of superfortresses scattered "Evacuate or Die" leaflets over twelve Japanese cities early today. MacArthur is set to head a huge invasion of Japan from Okinawa, the smaller Ryukyus, and the Philippines.

Monday, August 6, 1945

At 11:00 a.m., Eastern time, ratio stations began broadcasting the following statement by the President of the United States:

> Sixteen hours ago an American airplane dropped one bomb on Hiroshima, an important Japanese army base. That bomb had more power than 20,000 tons of T.N.T. It had more than two thousand times the blast power of the British "Grand Slam" which is the largest bomb ever yet used in the history of warfare.
>
> The Japanese began the war from the air at Pearl Harbor. They have been repaid many fold. And the end is not yet. With this bomb we have now added a new and revolutionary increase in destruction to supplement the growing of our armed forces. In their present form these bombs are now in production and even more powerful forms are in development.
>
> It is an atomic bomb. It is a harnessing of the basic power of the universe. The force from which the sun draws its power has been loosed against those who brought war to the Far East.
>
> Before 1939, it was the accepted belief of scientists that it was theoretically possible to release atomic energy. But no one knew any practical method of doing it. By 1942, however, we knew that the Germans were working feverishly to find a new way to add atomic energy to the other engines of war with which they hoped to enslave the world. But they failed. We may be grateful to Providence that the Germans got the V-1's and the V-2's late and in limited quantities and even more grateful that they did not get the atomic bomb at all.
>
> The battle of the laboratories held fateful risks for us as well as the battles of the air, land, and sea, and we have now won the battle of the laboratories as we have won the other battles.
>
> Beginning in 1940, before Pearl Harbor, scientific knowledge useful in war was pooled between the United States and Great Britain, and many priceless helps to our victories have come from that arrangement. Under that general policy the research on the atomic bomb was begun. With American and British scientists working together we entered the race of discovery against the Germans.
>
> The United States had available the large number of scientists of distinction in the many needed areas of knowledge. It has the tremendous industrial and financial resources necessary for the project and they could be devoted to it without undue impairment of other vital war work. In the United States the laboratory work and the production plants, on which

a substantial start had already been made, would be out of reach of enemy bombing, while at that time Britain was exposed to constant air attack and was still threatened with the possibility of invasion. For these reasons Prime Minister Churchill and President Roosevelt agreed that it was wise to carry on the project here. We now have two great plants and many lesser works devoted to the production of atomic power. Employment during peak construction numbered 125,000 and over 65,000 individuals are even now engaged in operating the plants. Many have worked there for two and a half years. Few know what they have been producing. They see great quantities of material going in and they see nothing coming out of these plants, for the physical size of the explosive charge is exceedingly small. We have spent two billion dollars on the greatest scientific gamble in history--and won.

But the greatest marvel is not the size of the enterprise, its secrecy, nor its cost, but the achievement of scientific brains in putting together infinitely complex pieces of knowledge held by many men in different fields of science into a workable plan. And hardly less marvelous has been the capacity of industry to design, and of labor to operate, the machines and methods to do things never done before so that the brain child of many minds came forth in physical shape and performed as it was supposed to do. Both science and industry worked under the direction of the United States Army, which achieved a unique success in managing so diverse a problem in the advancement of knowledge in an amazingly short time. It is doubtful if such another combination could be got together in the world. What has been done is the greatest achievement of organized science in history. It was done under high pressure and without failure.

We are now prepared to obliterate more rapidly and completely every productive enterprise the Japanese have above ground in any city. We shall destroy their docks, their factories, and their communications. Let there be no mistake; we shall completely destroy Japan's power to make war.

It was to spare the Japanese people from utter destruction that the ultimatum of July 26 was issued at Potsdam. Their leaders promptly rejected that ultimatum. If they do not now accept our terms they may expect a rain of ruin from the air, the like of which has never been seen on this earth. Behind this air attack will follow sea and land forces in such numbers and power as they have not yet seen and with the fighting skill of which they are already well aware.

The Secretary of War, who has kept in personal touch with all phases of the project, will immediately make public a statement giving further details.

His statement will give facts concerning the sites at Oak Ridge near Knoxville, Tennessee, and at Richland near Pasco, Washington, and an installation near Santa Fe, New Mexico. Although the workers at the sites have been making materials to be used in producing the greatest destructive force in history they have not themselves been in danger beyond that of many other occupations, for the utmost care has been taken of their safety.

The fact that we can release atomic energy ushers in a new era in man's understanding of nature's forces. Atomic energy may in the future supplement the power that now comes from coal, oil, and falling water, but at present it cannot be produced on a basis to compete with them commercially. Before that comes there must be a long period of intensive research.

It has never been the habit of the scientists of this country or the policy of this Government to withhold from the world scientific knowledge. Normally, therefore, everything about the work with atomic energy would be made public.

But under present circumstances it is not intended to divulge the technical processes of production or all the military applications, pending further examination of possible methods of protecting us and the rest of the world from the danger of sudden destruction.

I shall recommend that the Congress of the United States consider promptly the establishment of an appropriate commission to control the production and use of atomic power within the United States. I shall give further consideration and make further recommendations to the Congress as to how atomic power can become a powerful and forceful influence towards the maintenance of world peace.

During the afternoon a more detailed statement was released by the Secretary of War describing how the War Department had brought the bomb project to fruition and had made a start on planning for the future.

Enroute to Berkeley on the "City of San Francisco." The dropping of the atomic bomb was announced in newspaper headlines in papers that the passengers picked up at stops enroute. There was great excitement, and I had some difficulty keeping my counsel during the many conversations I heard in the club car and elsewhere. When the train stopped in Ogden, Utah, at 6:30 p.m., mountain time, I too, bought a newspaper announcing the event.

Tuesday, August 7, 1945

At 8:50 a.m. I arrived in Berkeley on the "City of San Francisco."

During the day I had meetings with Lawrence and others. We discussed the great newspaper interest over the Hiroshima atomic bomb. The papers are replete with articles on the atomic bomb, how and where it was developed. Churchill made a statement on the importance of the bomb, indicating that the course of the war would have been altered had the Germans produced this weapon.

Jeanette and Inez are scheduled to play golf at Jackson Park with Steve Lawroski late this afternoon.

In the evening I went to a lecture in San Francisco with the Calvins--Professor McBain of Stanford spoke on his recent trip to Russia. I then spent the night at the Calvins.

<u>Wednesday, August 8, 1945</u>

I called Perlman from Berkeley early in the morning. We talked about the news release on the atomic bomb. Perlman also mentioned that I received a letter from Allison on Monday describing the 5.1 mg of Np^{237} he is returning to us and the disposition of the remainder of the 22 mg we sent him.

I then visited with Latimer in the Chemistry Department. I met with Gilles, Thomas, Crandall, McVey, and others to discuss the TTA process they are developing for the separation and decontamination of plutonium from neutron irradiated uranium and fission products. During the afternoon I talked with Lawrence and visited the Radiation Laboratory.

After writing to Helen to tell her that I will arrive in Los Angeles Saturday morning, I again spent the night with the Calvins.

Today's headlines are still devoted to the Hiroshima bomb. Four square miles of the city were destroyed.

<u>Thursday, August 9, 1945</u>

I am still in Berkeley staying with the Calvins.

A plutonium bomb was dropped on Nagasaki shortly before 11:00 a.m. Japanese time, August 9 (yesterday our time). The announcement was the top headline in this morning's paper here.

I had a meeting with Provost Deutsch (President Sproul is out of town) to tell him about my offer of a professorship at the University of Chicago (at $10,000 per year) with the additional inducement of other academic staff as well as graduate students in the Department of Chemistry to work with me. Deutsch was encouraging that the University of California's offer to me could be improved to reduce the discrepancy between it and that from the University of Chicago. Because I hold the rank of Assistant Professor, in which I served only one semester before going on leave to work on the Manhattan Project, it is not surprising that the Administration at Berkeley finds it difficult to go much beyond the direct promotion to Full Professor that they already provided for me last month.

I talked at the regular weekly meeting of Latimer's group.

The Soviets have declared war on Japan and have made their first attack on the eastern border of Manchuria.

Friday, August 10, 1945

I am still in Berkeley. Perlman and Manning called me from Chicago. We discussed the bombardments of depleted uranium with deuterons and with helium ions, scheduled for the 60-inch cyclotron at Berkeley. We also talked about a forthcoming bombardment of thorium with helium ions and perhaps also with deuterons.

Perlman told me that the Institute for Nuclear Studies in the University of Chicago has announced the appointment of Enrico Fermi, Harold C. Urey, Samuel K. Allison, and Cyril Smith.

After the phone conversation, I dictated a letter to Allison to inquire whether it has been possible to recover any material from the New Mexico explosion with appreciable amounts of radioactivity. I said that we would like to analyze a sample for the heavy isotope content.

At 9:00 p.m. I boarded the "Lark" for Los Angeles.

President Truman warned the Japanese that they will suffer decimation and destruction if they do not surrender. Japanese radio claims that they have similar weapons and will retaliate. Smoke and dust is still covering Nagasaki, according to today's paper.

Saturday, August 11, 1945

Helen met me when I arrived in Los Angeles early in the morning. We will vacation at my parents' home (and my childhood home) at 9237 San Antonio Avenue in South Gate. Helen had travelled earlier directly to Los Angeles from Chicago by bus in order to visit friends and stay with her mother on Van Ness Avenue, before I arrived. My sister Jeanette's Dodge sedan is available for our use during our vacation.

Helen, Clayton Sheldon, and I played golf at Rio Hondo Golf Club.

8/11/45 (cont.)

Today's paper is full of hope for peace. It also reports that 30 percent of Nagasaki was destroyed.

Sunday, August 12, 1945

We are on vacation in South Gate. Helen and I played 18 holes of golf with Vance Cooper at the Lakewood Golf and Country Club in Long Beach. After Vance left, Helen and I played an additional nine holes in which she had her best ever score.

"Hirohito Can Stay On, But Only as Puppet" says this morning's headline. Peace in the Pacific grows closer as the Japanese were given a final chance of surrendering and keeping the Emperor, with occupation forces under an American supreme commander.

Monday, August 13, 1945

On vacation in South Gate. Perlman called me at my parents' home from Chicago to tell me that Al Ghiorso is making measurements on about 0.25 microgram of plutonium recovered from about 0.5 pound of dirt from the Alamogordo test. He will attempt to make alpha particle and fission measurements in order to determine the concentrations of Pu^{240} and Pu^{238} in this sample.

The war is still going on. American and British pilots bombed Tokyo again this morning.

Tuesday, August 14, 1945

On vacation in South Gate. Helen and I played 18 holes of golf with Fred and Edrey Albaugh at Los Angeles Western Avenue Golf Course. At about the twelfth hole, we heard horns and whistles blowing signalling that World War II is over--VJ Day. The four of us had dinner at a restaurant at Long Beach and Firestone Boulevards, next to Topsy's nightclub.

Throughout the country crowds are in the streets to celebrate the end of the war. Thousands of people jammed into Lafayette Square in Washington, D.C., chanting, "We want Truman," until he and Mrs. Truman appeared on the White House steps and the President spoke to the crowd.

Wednesday, August 15, 1945

On vacation in South Gate. Helen and I had dinner with Saul and Sylvia Winstein at their home in West Los Angeles.

"Great War Ends!" is today's banner headline. The Emperor of Japan said that the "atom bomb" caused them to give up.

Thursday, August 16, 1945

On vacation in South Gate. Helen and I played 18 holes of golf with my cousin Sidney Johnson at the Montebello Municipal Golf Course. In the evening Helen and I had dinner at Knotts Berry Farm with my parents, Helen's mother, and Bob and Ruth Engstrom.

Friday, August 17, 1945

On vacation in South Gate. Helen and I played 18 holes of golf at the Southern California Golf and Country Club in Monterey Park.

Today's headlines indicate that MacArthur has given the Japanese more time before the surrender conference, but twelve allied transports were attacked and damaged by the Japanese since the surrender.

Saturday, August 18, 1945

On vacation in South Gate. In the morning I had an appointment at the Francis Lay Studio on Tweedy Boulevard to have some new photographs taken. See Figure 5. When I returned, we visited Helen's mother and took some snapshots in front of her home. See Figures 6 and 7.

The Japanese surrender delegation has been selected and will fly to Manila tomorrow. Also in today's paper appeared pictures of the bomb test at Alamogordo.

Sunday, August 19, 1945

On vacation in South Gate. Helen and I played 18 holes of golf with Clayton Sheldon at the Potrero Golf Club on Manchester Avenue. We had a long wait because of heavy play. Helen, Clayton, and I then went directly to Rita and Clayton's home (1005 Hildreth, South Gate) where we joined Bud and Bonnie Coffin for a charcoal-broiled steak dinner in the Sheldons' patio. There was some delay before dinner because Clayton decided to try to install a fan in his barbecue.

The top headline in the paper today reads "Peace Envoys Leave Japan." They will fly to Manila for peace talks.

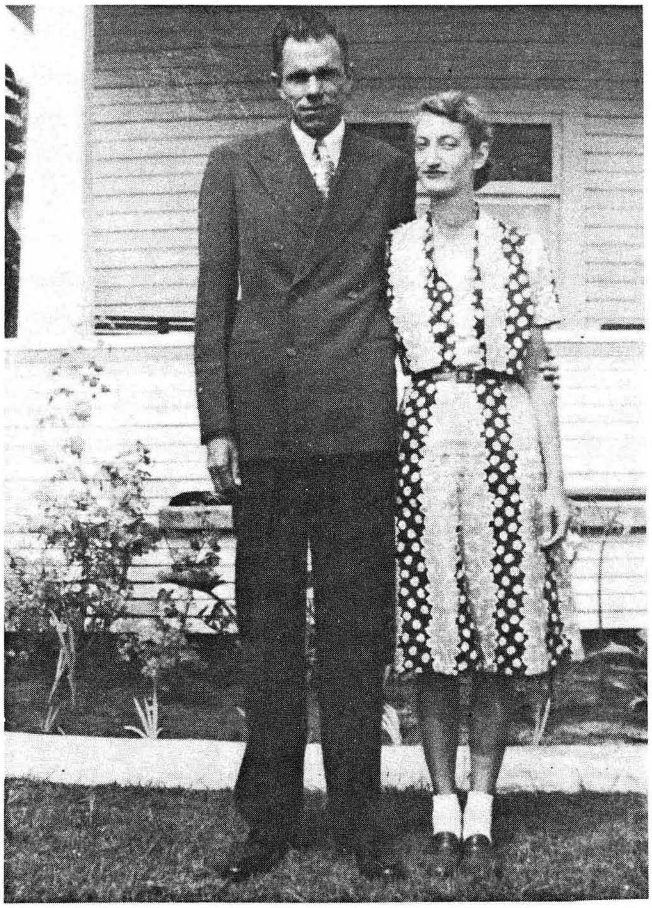
XBB 801-461

Figure 5. Glenn T. Seaborg, August 1945.

Figure 6. Selma (mother) and Glenn Seaborg in front of Helen's mother's home at 1637 S. Van Ness Avenue, Los Angeles, August 1945.

XBB 801-464

Figure 7. Glenn and Helen Seaborg in front of Helen's mother's home, August 1945.



XBB 801-463

Monday, August 20, 1945

On vacation in South Gate.

MacArthur aides are meeting with the Japanese to arrange for a formal surrender. MacArthur himself has not yet met with the Japanese.

Tuesday, August 21, 1945

On vacation in South Gate. Helen and I played 18 holes of golf at one of the Griffith Park golf courses. I shot 79, the first time I broke 80. We then had dinner with Vance and Mary Cooper in their apartment.

MacArthur announced today that he will leave for Japan soon and will deliver surrender articles within ten days.

Wednesday, August 22, 1945

On vacation in South Gate. Helen and I played 18 holes of golf at the Rio Hondo Golf Club.

The formal surrender of the Japanese will take place on August 31 and will be formally signed on a U.S. battleship in Tokyo Bay, according to today's paper.

Thursday, August 23, 1945

On vacation in South Gate. Helen and I played 18 holes of golf at one of the Baldwin Hills Golf Courses on Slauson Avenue.

The surrender papers will be signed on the U.S.S. Missouri with Admiral Nimitz signing for the U.S., says today's papers.

Friday, August 24, 1945

On vacation in South Gate.

Typhoons are delaying preparations for Japan's surrender, and the signing has been postponed until September 2.

Saturday, August 25, 1945

On vacation in South Gate. Helen and I, along with Clayton

Sheldon and some others, attended the Beverly Hills Open Golf Tournament at the California Country Club. Dale Andreasen, who lived one house removed from our 9237 San Antonio Avenue home as a boy, came in third (285), beaten by Ray Mangrum (281) and Joe Kirkwood (281).

Sunday, August 26, 1945

I took a train to Berkeley. Helen will remain in Los Angeles with her mother.

Jeanette is scheduled to leave Chicago, where she has been staying in our apartment, for a trip to New York and Washington, D.C. She will return to Chicago on September 5.

Today's top headline reads "U.S. Warships Enter Jap Bay." The surrender, however, was delayed due to typhoons, as mentioned before.

Monday, August 27, 1945

In Chicago Ralph James is scheduled to begin his vacation. He will return on September 10.

Latimer met me at the station when I arrived in Berkeley this morning and then drove me to the University. There I had an appointment with Joe Kennedy, who is in Berkeley with his wife Adrienne and their baby, to discuss our plutonium patent situation. Later I met Reg Richardson, a physicist who attended UCLA with me, and some of his men in order to discuss our chemistry group's needs and our relationship with the cyclotron. Joe Hamilton and I then had coffee on Telegraph Avenue

The Calvins took me to the Curve in Lafayette for dinner.

This morning's newspaper describes the entry of the U.S. third fleet, led by the U.S.S. Missouri, into the north end of Sagami Bay. There the fleet will await minesweeping operations of Uraga Strait before entering Tokyo Bay.

Tuesday, August 28, 1945

In Berkeley. At 8:30 a.m. I had an appointment with President Sproul. I told him what I need in order to stay at Berkeley instead of accepting the University of Chicago's offer. In addition to a full professorship for me, I want Perlman as an Assistant/Associate Professor; English and Orlemann, as Assistant Professors; Cunningham as an Assistant Professor in the Radiation Laboratory; Ghiorso and other research associates, about twelve graduate fellowships. Sproul said he would try to get all this through the Regents.

In the afternoon I had another meeting with Reg Richardson and his group. Reg and Louise, an old girl friend of mine, invited me to have dinner at their Piedmont home. After dinner Reg and I went to the Oakland Telenews Theater to see a movie of the Alamogordo atomic bomb explosion of July 16.

U.S. ships have entered Tokyo Bay in preparation for Japan's formal surrender.

Wednesday, August 29, 1945

In Berkeley. Perlman, at the Met Lab, and I talked by phone. I have decided to accept the Berkeley offer, so I wrote Helen a note to tell her of my decision. Just before I left the University to catch the "City of San Francisco," I told Latimer what I have decided to do.

Admiral Halsey steamed into Tokyo today; MacArthur is headed for Japan; and General Joseph Wainwright, who has been a prisoner since the fall of Corregidor, will be there for the surrender.

Thursday, August 30, 1945

Enroute to Chicago on the "City of San Francisco." I am sharing a bedroom with Professor Preston Harris of Ohio State.

The newspaper we picked up enroute says that the occupation of Japan has begun. American prisoners are being freed from prison camps, and these camps have proved to have been operating under terrible conditions.

Friday, August 31, 1945

Stan Thompson and Iz Perlman met me at the station when I arrived in Chicago at 1:00 p.m. After seeing Hogness to tell him of my decision to accept the Berkeley offer, I spent the rest of the afternoon going over the work done during my absence.

Monday, August 6

The Met Lab Report for July 1945 was issued by the Laboratory Director's Office. The Summary section of the report noted that the decrease in personnel is complete, and the Met Lab is operating with 221 academic employees (including SED men) and 425 non-academic employees. There are 278 academic employees and 479 non-academic employees, including all branches of the scientific work in Chicago and Argonne. The total personnel employed on Contract W-7401-eng-37 during the month of July, including administration and security, was 1,415.

Manning spoke on tracers in chemistry and biology at the Chemistry Seminar at 7:45 p.m. in Room 251, Ryerson Laboratory.

Tuesday, August 7

At 8:28 a.m. there was a meeting in the New Chemistry conference room of Groups 1 and 3. It was attended by Britain, Cunningham, Ghiorso, Hopkins, Hyde, Jaffey, James, Manning, Morgan, Perlman, Peterson, Robinson, Scott, Stewart, R. Thompson, S. G. Thompson, and Westrum. Manning announced the plant run at Site W for extraction of Np^{237} will start August 11. We will probably get the sample two weeks later--Cunningham, Hindman, Florin, and Magnusson will give it high priority. Manning also mentioned that 20 mg of U^{235} from the first CW bombardment were given to Britain yesterday.

Perlman said that all the Agruss material must be gathered together and shipped to the Bureau of Standards, along with our method of analysis. Agruss has contested our analysis and says he is certain there are 50 mg of Pa^{231}.

Morgan described his efforts to follow the decay of sample 49NG (Hanford-bombarded plutonium from Farmer's No. 2). He summarized the latest approximations that indicate Pu^{241} is decaying with a beta half-life of 100 years and an alpha half-life of 5×10^5 years; the alpha half-life of 95^{241} is 40 years; the neutron capture cross section of Pu^{240} is 800 barns while the neutron capture cross section of 95^{241} is 5,000 barns. If the half-life of 95^{241} is larger, its neutron capture cross section is correspondingly smaller.

Cunningham said that the isolation of the latest 95 sample has confirmed the pink fluoride that can be metathesized to a pink hydroxide. Perlman suggested that it would be worth getting extra pure reagents to make certain no rare earths are present. Cunningham agreed.

Perlman and James then discussed the unexplained discrepancy in the half-life value for 95^{241} of a few years, obtained by Sullivan at Site W, while our figure is 40 years.

James and Ghiorso talked about their efforts to determine whether the apparent 4.3 cm peaks are the same in the various helium ion bombardments (normal uranium, U^{235}, and depleted uranium).

Thompson discussed his plans for decontaminating some 95^{241} using the CW-2 sample, and taking the rare earth fraction. After the removal of lanthanum, cerium, and yttrium by precipitation reactions, he will try removal of element 61 by use of the IR-1 although it is not very satisfactory. He intends to look for element 97 that may be like cerium from analogy of the actinide and lanthanide series. He plans to do this by adsorbing the cerium on IR-1, eluting, and looking for a small bump on the activity versus volume elution curve. Perlman observed that, with all the activity the cerium has, he thinks it is hopeless to look for beta particles from element 97.

Under Secretary of War, Robert P. Patterson, issued the following statement to the men and women of the Manhattan District Project:

> Today the whole world knows the secret which you have helped us keep for many months. I am pleased to be able to add that the warlords of Japan now know its effects better even than we ourselves. The atomic bomb which you have helped to develop with high devotion to patriotic duty is the most devastating military weapon that any country has ever been able to turn against its enemy. No one of you has worked on the entire project or known the whole story. Each of you has done his own job and kept his own secret, and so today I speak for a grateful nation when I say congratulations and thank you all. I hope you will continue to keep the secrets you have kept so well. The need for security and for continued effort is fully as great now as it ever was. We are proud of every one of you.

As a reaction to the dropping of the atomic bomb on Hiroshima there was an evening meeting on "Political and Social Implications," which was attended by Brues, Cole, Cunningham, Daniels, Friedman, Ghiorso, Goldsmith, Hill, Hughes, Kohman, Manning, Maurer, Moon, Nickson, Perlman, Rabinowitch, Schuman, Simpson, Steinberg, Szilard, and Way. Rabinowitch was selected as chairman of the group. It was decided that a declaration would be prepared indicating that (1) forms of control are possible but will break down if an individual nation gets out of hand, (2) collective sanctions will not prevent war, (3) police force will not help, (4) world government is the only solution, (5) world government is possible by (a) conquest or (b) union.

It was agreed that a rough draft would be prepared for the next meeting, provisionally scheduled for Tuesday, August 14.

Wednesday, August 8

In a memo to Captain Chapman, Manning announced that the entire Agruss protactinium sample received a few weeks ago from his office is now available in two fractions except for negligible amounts used to assay the material. Manning said he will hold the fractions until Chapman sends further instructions.

Manning sent Howe a list of the names of 49 men of associate chemist rank and higher in Section C-I, together with a statement of their roles in the Metallurgical Project. Manning points out to Howe that, in addition to these men, there are also junior chemists and research assistants in Section C-I who have made more important contributions than some of the people on the list, and it would be unfortunate for them to be discriminated against in any publicity releases.

Perlman acknowledged receipt of Allison's letter to me of August 4 about the Np^{237} he is sending us. Perlman said, "The amount and conditions of the samples being sent will be satisfactory for our present needs."

Thursday, August 9

Manning transmitted Deferment Forms to Johnson for Morgan, James, Peterson, Hyde, Scott, O'Connor, Florin, Hindman, and R. Thompson.

There was a meeting of the Control Analysis Group of Section C-I, which was attended by Ames, Anderson, Fineman, Kohman, Manning, Perlman, and Sedlet. Fineman described his experiments on the measurement of radon emanation.

Ames and Sedlet reported on a method they have adapted for carrying radium with $BaCl_2$.

Kohman reviewed the problems to be solved concerning radon emanation, counting accuracy, new all-metal chambers, and direct counting methods for radium.

Friday, August 10

Daniels and Branch issued the following notice to all employees of the Metallurgical Laboratory.

> Despite the fact that the surrender terms of the Japanese may be accepted, it is necessary that all of us keep in mind that the Metallurgical Laboratory still has important and significant work to do.
>
> The Manhattan District has requested that all employees remain at work as usual.
>
> In the interest of future peace it is more important than ever that security regulations be maintained regarding the disclosure of scientific information.
>
> The Metallurgical Laboratory has not yet been authorized to release any information or publicity. Announcement of V-J Day must not be construed as authority to release such information. We will be notified officially when such releases are possible.

In view of our solemn responsibilities and the fine spirit evidenced by the employees of the Metallurgical Laboratory there is no doubt of your full and whole-hearted cooperation.

Saturday, August 11

Radiation surveys by the Health Division for the week ending August 11, 1945, showed the following rooms in the New Chemistry Building had above maximum permissible levels for alpha, beta, or gamma contamination: Room 1 (used for protactinium work), Room 2, Room 4, Room 11, Room 13, Room 34, Room 35, and Room 37. Excessively contaminated rooms in the West Stands were Room 218 (used for chemical work for Ghiorso's group), Room 217, Room 216, Room 210, and Room 6.

Monday, August 13

My office received an acknowledgement from Lavender of receipt of my letter of August 1. Lavender suggested that, instead of sending the Certificate of Disclaimer for execution to Dr. Sproul as I proposed, it would be preferable to have the inventors execute the agreement first. He would then hold the agreement in escrow pending the University's final action.

Truman Kohman wrote to L. F. Curtiss, with a copy to me, that he believes he can determine the half-life of radium to better than one percent if he has several milligrams of radium of known high purity. Kohman asks whether Curtiss can inform him of a source of such material. Curtiss had written me in July that the half-life of radium is not known to better than several percent.

Report CS-3160, "Chemistry Division Summary Report for July 1945," was issued. All Section C-I information in this report appeared in my July 20 memo to Hogness.

The British Information Services issued a 22-page news release entitled "Britain and the Atomic Bomb." The statement includes historical background, descriptions of the organization of the work in the UK and the scope of the research programs undertaken, the decision to transfer to Canada one section of the work which became a joint Anglo-Canadian-U.S. enterprise, and the participation of British scientists in the work at Berkeley, Oak Ridge, and Los Alamos.

Tuesday, August 14

Groups 1 and 3 met at 8:28 a.m. in the New Chemistry conference room. The meeting was attended by Asprey, Cunningham, Florin, Ghiorso, Hindman, Hopkins, Hyde, Jaffey, James, Katzin, Manning, Morgan, O'Connor, Perlman, Peterson, Robinson, Scott, Simpson, R. Thompson, S. Thompson, Van Winkle, and Weissbourd. The meeting started ten minutes late because of the absence of Roy Thompson, the minute-taker. Manning announced that Nickson is very disturbed about hot spots in the laboratory and has asked that such spots be cleaned promptly

Perlman announced that Nickson was expected about 11:00 a.m. for a discussion of the opening of sample CW-3 and asked Cunningham, O'Connor, Asprey, and Britain to be present. He thought that the opening of the sample might be started this afternoon.

The possibility of a trip to the Dunes by the personnel of the section in the near future received considerable discussion; Peterson was delegated to investigate the matter. See Figure 8.

Perlman inquired about the progress on CW-1 (Hanford-irradiated U^{235}--the first Farmer's special). Britain replied that the sample was opened yesterday and plates could be made today to determine the plutonium content.

There was a discussion about having Ghiorso use the Argonne pile for a whole shift about Saturday to make measurements on the uranium from CW-1 and repeat the previous measurements on uranium from CW-2.

Perlman outlined the samples that are expected in the near future. He indicated the 23 plus helium ions target is expected to arrive any day (for Hyde), the thorium plus helium ions and ionium plus helium ions targets are expected to follow in about a week (for Studier), and two new bombardments have been scheduled--depleted uranium plus deuterons (to check the older bombardment of which 40 percent was lost in the cyclotron) and a depleted uranium plus helium ions bombardment (this is a repetition of a previous bombardment in which several things went wrong). The Np^{237} from Hanford is still expected around the 25th of the month and will be handled by Cunningham.

There was nothing new to report on Agruss-Saturnium (Pa^{231}) controversy. The material purchased from Agruss is being kept separate pending developments.

James talked at length about the present state of thought on Np^{236} and Pu^{236}. He mentioned that soft electrons of about 5-day half-life have been found by Morgan in the 93 fraction of the U^{235} plus deuterons bombardment and that the growth of 94 alpha-particle activity in the 93 fractions from the U^{235} plus deuterons and U^{238} plus deuterons samples has been followed and gives a half-life for 93^{236} of about 17 hours.

Stan Thompson reported that he had taken 10 percent of the rare earth fraction from 49NG (Hanford-irradiated plutonium from Farmer's No. 2) and decontaminated it with a 10 percent yield of 95^{241}-96^{242} mixture. He gave a sample to Ghiorso for range measurements.

Figure 8. Indiana Dunes, Summer 1945. (Left to right) Kay Florin, Pat Walsh, Olga Giacchetti, Marty Studier, Chris Studier; Sig Peterson in back.

Cunningham reported that the 95 isolated from the last milking was not appreciably higher in specific activity than that obtained from the first. The rare earth content was about 30 percent. His value for the half-life is still eight to ten times that obtained by James. Asprey's opinion was that lanthanum or other rare earth impurities must be introduced with reagents because each milking of the plutonium cow should give a decontamination with respect to lanthanum of at least 100.

There was a discussion of the possibility of obtaining 95^{241}-rich supernatants from Site Y. Perlman indicated that, perhaps after the wartime pressure is relaxed, arrangements might be made for the milking of large amounts of plutonium at Site Y.

Reports were given by Van Winkle on the progress of isolating ionium (400 mg are now concentrated on 260 g of bismuth phosphate), by Peterson on separation of actinium tracers from gram amounts of lanthanum in IR-1 columns (results are encouraging), by R. Thompson on the processing of the saturnium, element 91, (it should be ready for final purification by solvent extraction by the end of the week).

Simpson displayed and discussed his chart of isotope stabilities. He plots the log of the half-life against atomic number for each radioactive series.

Perlman thought that most of the material not covered could be postponed until next week; however, he asked how the electrolytic method of plate preparation was progressing. James replied that he felt that small amounts of plutonium could now be consistently plated in a satisfactory manner. Scott suggested that the hydroxide method should be investigated again because the quality of those plates was better than ones obtained by the carbonate method.

The meeting was adjourned at 10:45 a.m.

Wednesday, August 15

John P. Howe, Associate Director of the Met Lab, resigned to join the General Electric Company.

The University of Chicago received the following telegram from General Groves today:

> Official declaration of cessation of hostilities with Japan does not in any way alter security limitations on release of information on the atomic bomb project. Security restrictions imposed in my telegram of six August continue to apply. The President in his broadcast of nine August emphasized the necessity in the interest of national safety for controlling release of information on this revolutionary development. Loose talk and idle speculation by persons now or formerly

connected with the Project jeopardizes the security of the nation and must be controlled. I am again asking you personally to continue your complete cooperation through my office by your entire organization and by each of your subcontractors in the maintenance of national security.

Friday, August 17

The Solvent Extraction Group met at 8:28 a.m. in the New Chemistry conference room. The meeting was attended by Ader, Blaedel, Gaarder, Goeckermann, Gilbreath, Hagemann, Hyman, Kelley, Lawroski, Manning, Perlman, Post, and Schraidt. Manning conducted the meeting.

Blaedel reported on the first runs made in the first-cycle columns with uranium present in the feed. Five runs have been made, two of about 30 hours and three of 8 hours. The primary objective was to improve the mechanical operations. No insurmountable problems were encountered.

Hyman reported on batch experiments that he, Ader, and Goeckermann carried out. They used the latest flowsheets in their decontamination studies and included experiments where unfavorable column conditions were simulated; no appreciable differences in decontamination were noted between the two procedures. Hyman then talked about preliminary experiments using aluminum nitrate instead of ammonium nitrate as desalting agent. It appears to perform satisfactorily.

Goeckermann discussed the effect of temperature upon plutonium and uranium distribution ratios and gave some results on hydrazine recovery from waste hexone.

Lawroski said that plans for the coming week include a run without product using the pretreated hexone, when available, and finishing the construction of the second-cycle columns.

My office received a letter dated August 14 from Kennedy at Site Y acknowledging receipt of my August 1 letters to him and to Lavender. He indicated he believes my action has been exactly right; however, if no results are achieved soon, he, Segrè, and Wahl propose asking Lavender to go ahead on the same agreement without approval or "Certificate of Disclaimer" from the University of California. He went on to say they believe the recent release of Smyth's report to the public makes it very necessary that applications be filed at once. He asked if I had received and considered the proposed draft of an agreement between the inventors, and enclosed an additional copy. In a footnote he said he has just received a copy of the letter from Lavender and Lavender's suggestion looks to him similar to the course they are proposing.

A letter arrived for me from Allison dated August 15 transmitting the outline of detailed recommendations on research and development programs in nucleonics to be prepared for use of the Scientific Panel of Compton, Fermi, Lawrence, and Oppenheimer who are to advise the Interim Committee. Allison said the Panel asked that I prepare the material on research in chemistry. He noted that the problems need not all be in basic research of radiochemistry, and methods of improving separation and extraction processes should be mentioned. Physics is to be handled by Bacher and R. R. Wilson, metallurgy by C. S. Smith, biology by Hamilton, and health by Stone.

The University of Chicago issued a 12-page news release describing the University's role in the work which culminated in the release of atomic power. (Reference numbers in right hand margin of the release refer to chapter and paragraph of the official War Department report, The Smyth Report.) The Met Lab personnel identified by name were Compton, Fermi, Allison, Franck, Warren Johnson, Hogness, Seaborg, and Stone.

Saturday, August 18

Ghiorso remeasured the slow neutron fission cross section of Th^{229} at Argonne. He found it to be unchanged by the last repurification and seems to be about 0.1 that of U^{233}.

Radiation surveys by the Health Division for the week ending August 18, show the following rooms had above maximum permissible level of alpha, beta, or gamma contamination: Room 1 (used for protactinium work), Room 2, Room 4, Room 9, Room 11, Room 13, Room 33, Room 35, and Room 37,

Monday, August 20

Al Ghiorso began his vacation.

Joe Hamilton is visiting the Met Lab from Berkeley. Manning and some of the other men talked with him and learned that he is making a bombardment for the Health Group at Clinton and will not be able to bombard our targets for about three weeks.

Hindman sent Perlman the flowsheet for isolation of the Np^{237} to be received from Hanford in the form of 30 liters of solution containing 37 grams of lanthanum per liter and about 15 grams of plutonium. The procedure will involve hexone extractions and hydroxide precipitations followed by bromate oxidation cycles until the Np^{237} is completely decontaminated from plutonium.

Daniels requested for us the following Hanford irradiations from Compton: (1) neutron irradiation of 1 mg of Np^{237} for 90 days, (2) irradiation of 10 grams of thorium for 30 days in order to obtain material for measuring the neutron-capture cross section of Pa^{233}, (3) production of gram amounts of U^{233} by irradiation of 80 kg of pure thorium metal to be obtained from Spedding at Ames. The latter irradiation would be for about three months.

Szilard spoke on future applications of nucleonics at the Chemistry Seminar at 7:45 p.m. in Room 251, Ryerson Laboratory.

Tuesday, August 21

There was a meeting of Groups 1 and 3 at 8:28 a.m. in the New Chemistry conference room. It was attended by Asprey, Britain, Hindman, Hopkins, Hyde, Jaffey, James, Katzin, Manning, O'Connor, Perlman, Peterson, Simpson, Studier, R. Thompson, S. Thompson, and Van Winkle. Manning mentioned his conversation with Hamilton yesterday and pointed out this will give us time to get CW-3 out of the way before the targets start to come. In the meantime, Studier is to help work on the pulse analyzer.

Manning asked about the plans for the ionium plus neutrons. Jaffey explained the idea was to determine the neutron capture cross sections of ionium and thorium relative to U^{238}, but they are waiting for the Pa^{233} to decay. They plan to take a pulse analyzer curve to determine the ratios of alpha activities.

Perlman announced that Hanford has concluded that poisoning of the pile will come from heavy isotopes and for this reason we have been asked for values of the capture cross sections of U^{236}, Pu^{239}, and Pu^{240}. There was a discussion of the present uncertainties in the capture cross sections of U^{236} and Pu^{240} and the means by which better values might be obtained.

O'Connor said the first ether extraction is completed for the CW-3 uranium sample (Hanford-irradiated U^{235}--Farmer's Special No. 3).

Manning reported that last Saturday at Argonne the slow neutron fission cross section of Th^{229} was measured and was unchanged by the last repurification--it appears to be about 1/10 that of U^{233}.

Manning then asked whether James had done anything with the 49αB sample (100 mg plutonium plus 40 Mev helium ions) from which he was to isolate the decay products of 96^{241} and 96^{242}. James replied that the pulse analyzer curves are being completed now. He will then examine the x-rays and make chemical separations into uranium, neptunium, plutonium, and 95-96 fractions.

Hindman described his plans to study the hydrolytic behavior of Np(V).

Van Winkle reported preliminary findings of a protactinium complex $PaF_7^{=}$. He and Roy Thompson have also isolated 24 to 28 mg of protactinium.

Studier presented a range curve on the uranium fraction from the thorium plus helium ions bombardment. He has found a short-lived activity that may be U^{231} produced by an α,5n reaction.

The meeting concluded with a discussion about the release of the Smyth Report. There has been some concern that this revealed too much information. The unanimous opinion was that the recall of the Report would be ridiculous.

James began the separation of sample 49αB-15a3 (element 95 fraction from plutonium plus 40 Mev helium ions Berkeley bombardment of March 1945) into uranium, neptunium, plutonium, and 95-96 fractions. He is looking for daughter products in an attempt to pin down mass assignments.

Manning and Perlman sent a jointly-authored memo to Hogness, summarizing the work of Section C-I for the period July 15 to August 15, 1945. Results described were as follows: (1) Further work on the plutonium highly irradiated at Hanford has led to a revision in the half-life of Pu^{240} from 8,000 years to 6,000 years. The rate at which 95^{241} alpha activity appears in this material indicates a half-life of 40 to 50 years for 95^{241}. (2) Further work on the helium ion bombardment of natural uranium, enriched uranium, and depleted uranium shows the x-rays in the plutonium fraction are attributed to a reaction on U^{235} and not on U^{238}. The half-life of the activity resulting in these x-rays is of the order of several months and has been tentatively assigned to Pu^{237}. (3) Analysis of a depleted uranium plus 22 Mev deuterons bombardment showed the production of U^{239}, Np^{239}, Np^{238}, Np^{236}, and U^{237}. A half-life of 0.7 day for Np^{236} has been deduced. (4) A sample of U^{235} oxide was also bombarded with 22 Mev deuterons in the Berkeley cyclotron. The half-life for the Np^{236} formed in this bombardment is in agreement with that determined from the U^{238} sample. There is also evidence that another neptunium isotope, perhaps Np^{233} or Np^{234}, was formed in this bombardment--there was found in the neptunium fraction a hard gamma-radiation and soft electrons, both of which decay with a 5 to 6 day half-life. Study of the plutonium fraction from this bombardment gives evidence for a half-life of the order of three years for Pu^{236}. (5) The relative yields of Pa^{232} and Pa^{233} were measured from a thorium metal target bombarded with 22 Mev deuterons. (6) Three additional tests of carefully repurified Th^{229} indicate the cross section for slow neutron fission is approximately 50 barns. (7) Further attempts to isolate pure element 95 have produced samples whose activity corresponds to a half-life of about 400 years for 95^{241}. Since this value is about ten times the half-life indicated by direct measurement of the decay of 95^{241} and by the growth of 95^{241} associated with a given beta activity of Pu^{241}, it is probable that the material isolated thus far still contains appreciable impurities. (8) Additional tracer experiments on methods for separating element 95 from rare earth fission products indicate that element 61 is the most difficult fission product to separate from element 95. (9) Vapor pressure measurements of the oxides of plutonium by the effusion method have given upper limits to the vapor pressure of PuO_2 of 2.8×10^{-6} mm of mercury at 1,843°K and 8.3×10^{-6} mm of mercury at 1,923°K. (10) Separation of lanthanum and actinium by means of adsorption on IR-1 resin has proved successful if relatively low amounts of lanthanum carrier are present. (11) In the Redox Solvent Extraction Process, five first-cycle runs have been made in the 1-inch continuous extraction equipment employing an inactive feed which was 2 M in UNH, 0.2 M in $Na_2Cr_2O_7$ and 1 M in HNO_3. Although some mechanical difficulties were encountered, the operation of the entire first cycle equipment appears to be generally satisfactory. (12) In the uranium ore control analysis work, the apparatus for radium analysis in which the radon produced is swept out of the solution and determined by an alpha-particle counting technique has given results which are accurate to about 5 percent. A search is being made for more rapid methods of analysis.

Wednesday, August 22

The meeting of the Control Analysis Group at 8:28 a.m. was attended by Ames, Anderson, Fineman, Kohman, Manning, Perlman, Sedlet, and Weissbourd. Subjects discussed were radon emanation (Fineman), the design of a simple circuit for ease of construction (Weissbourd), a new design for an all-metal chamber (Kohman), and the direct counting of radium with $BaCl_2$ as carrier (Ames).

The scientific program on "Power Possibilities" was held during the afternoon. The speakers were Metcalf (Patent Possibilities), Daniels (Preliminary Design for a High-Temperature Oxide Pile), Willard (Progress Report on High-Temperature Piles), Goldberger (Calculations of Critical Size of BeO High-Temperature Piles), Ohlinger (Engineering Features of Nuclear Power), and Gale Young (Turbine Power Plants).

The Policy Meeting of the Project Council was held at 9:20 a.m. in Room 209, Eckhart Hall. It was attended by Bartky, Chapman, Chipman, Compton, Daniels, Dempster, Doan, Hamilton, Harrell, Hilberry, Hogness, Howe, Jacobson, Jeffries, Johnson, Leverett, McKinley, Murphy, Nordheim, Spedding, Stone, Szilard, Thomas, Tracy, Warner, Watson, Whitaker, Wirth, and Zinn. Compton opened the meeting by saying he knew that all those present are glad to see that eight days after the first bomb was dropped, the war was at an end, indicating that, in spite of the great loss of life, it was a saving in the end.

There was a discussion of the statement from the President to General Groves to hold everything in publishing information. As a result those on the Project have been asked not to give any information at all. Warren Johnson asked if it is necessary to get permission from Washington to disclose anything that is in the Smyth Report and was told by Compton that technically it is not necessary, but we are likely to get into trouble if we do not. Compton then said that Truman has ordered no more information be disclosed. Zinn remarked, "It's more important than who gets the credit and who does not. If people are going to get the correct information they have to get it from the right people. Washington is really cutting off the right source of information which would educate the public."

Thomas, Watson, and Jeffries argued for patience, Thomas suggesting that if the situation is not clarified in two weeks they should take their troubles to the Panel through Compton. Jeffries felt the release of the Smyth Report was more than many of us had any right to expect from the standpoint of the amount of information that was given out. He went on to observe that the Smyth Report deals very little with future development. This led Compton to state there is a strong feeling in the group that the American public should have an opportunity to know as soon as possible from those who are informed on the scientific aspects about their own feelings concerning the future implications of this Project and to make this group free in expressing themselves as American citizens on the implications of the work in which we have been involved.

During the discussion of the release of scientific information Hogness said he felt impelled to speak for the chemists--there was not proper credit given to them in the Smyth Report.

Szilard said the group is not really clamoring for the release of scientific information and added that he thought they all feel the Smyth Report went far too far.

There was a discussion of plans for the next meeting. Compton suggested there be a general information meeting Monday and Tuesday (September 17 and 18 at Clinton, each division to have a separate half day, Colonel Nichols' meeting on Wednesday, with the Scientific Panel meeting on Thursday, Friday, and Saturday.

Thursday, August 23

A letter, dated August 21, arrived from Ed Orlemann. He said, "Well, they worked. Groves can roll over and quit having Congressional Committee nightmares; stupid people like me can make puns like Hir-o-shi-ma now you don't; and the 'scientific' journalists can have a helluva time with the great new era..." He is concerned about postwar job opportunities and asks if I know anything about Berkeley's future plans. He would prefer to return there but wants to make certain that it will be a permanent position. The Boston University position, which I suggested he investigate, was not offered to him because of lack of enough budget.

My office also received an August 16 letter from Paneth on the subject of nomenclature for the new radioactive heavy isotopes. He agreed with my inclination which I mentioned in my August 2 letter that we adopt without further delay the term "neptunium series." He suggested I ask Segrè and his collaborators to propose names for elements 43 and 85. As to element 91, he favors the name "protactinium" and is opposed to the names "saturnium" or "brevium."

During the morning Perlman, Manning, and Kohman met with A. Q. Butler and G. L. Martin of the Mallinckrodt Chemical Company in St. Louis, Captain Bassett of New York, and Lieutenant Velten of St. Louis for discussions of Kohman's radium assay program. The progress of our program was outlined and the Mallinckrodt representatives were satisfied with the progress being made. They were especially interested in the direct counting radium assay.

Another meeting was held in the afternoon in Dr. Jesse's office, attended by Butler, Martin, Captain Bassett, Captain Chapman, Lieutenant Velten, Jesse, Bradley, Dana, Kohman, and Weissbourd. We were told our emanation analysis method will not be installed in St. Louis. However, the direct radium counting method will be installed, and the Instrument Section will supply two standard alpha counters for this purpose.

Friday, August 24

Manning and Perlman sent a jointly-authored memorandum to Hogness giving the highlights of work of Section C-I for the past month. It covers the same information given in a Perlman-Manning memo to Hogness of August 21.

Perlman sent Wheeler information on some of the cross sections that are of interest to him. He said we expect to obtain better data on all these in the future. (1) The capture cross section of U^{236} was calculated from three sets of data, the values obtained being 5 barns, 35 barns, and 45 barns. (2) A guess as to the capture cross section of Pu^{240} is 800 barns. (3) We are in agreement with Site Y that the amount of Pu^{240} formed in the Hanford pile is greater than that which would be calculated from the thermal neutron capture cross section of Pu^{239}. The measured value for Pu^{240} produced in uranium slugs is 0.76 percent of the plutonium.

Saturday, August 25

At 8:28 a.m. there was a meeting of the Control Analysis Group of Section C-I, attended by Ames, Anderson, Fineman, Kohman, Manning, Perlman, Sedlet, and Weissbourd. Fineman reported on the results of experiments done on the new emanation line; in general, there are no serious sources of error other than air leaks and coincidence losses. Kohman told the group that the new metal chamber recently constructed is satisfactory but is being redesigned with a view towards simplicity. Weissbourd is still working on a new circuit design. Ames reported on some self-absorption experiments on the direct counting of radium with $BaCl_2$ as carrier; little difference was noted between the two techniques tested.

In the search begun on August 21 for daughter products in sample 49αB-15a3 (element 95-96 fraction from plutonium plus 40 Mev helium ions bombardment), James has found evidence for element 96 in the 93 fraction that he has separated from the sample. Before leaving on vacation today he ran another cycle on this fraction to separate it further into a 93-94 fraction and a 95-96 fraction. He took alpha and GM counts on both fractions. James' vacation will last until September 10.

Farrington Daniels announced that 150 copies of the Smyth Report have been shipped to the Metallurgical Project. Daniels states, "The statements in the Smyth Report continue to be the limit of disclosure which we are permitted to make."

An invitation arrived in the office for me from Stewart S. Kurtz, Jr. Kurtz is chairman for the 1946 Conference on Petroleum Chemistry at Gibson Island. He would like me to lead the discussion on the use of radioactive tracers in chemical research at their 1946 meeting which will probably be held the second week of June.

Tuesday, August 28

Groups 1 and 3 met in the New Chemistry conference room at 8:28 a.m. Asprey, Britain, Florin, Hindman, Hopkins, Hyde, Jaffey, Katzin, Kohman, Manning, O'Connor, Perlman, Peterson, Robinson, Simpson, Stewart, Studier, R. Thompson, Van Winkle, and Weissbourd were present. Perlman opened the meeting by announcing there is a meeting tonight to discuss Rabinowitch's report on political implications and postwar plans. He also noted the Smyth Report will soon be available in the New Chemistry Library.

There have been delays in the bombardments from Berkeley, and the U^{233} will be the first target to arrive, three weeks from August 25.

Perlman described plans for work on the plutonium CW-3 sample. Florin and Asprey will help O'Connor in sawing the sample open; Studier, Hyde, Stewart, and Britain will help on the ether extractions.

Roy Thompson and Van Winkle said they now have 26 mg of protactinium about ready for solvent extraction.

Britain reported on the uranium CW-3 sample. He obtained two-thirds of the uranium from O'Connor following his ether extraction, and after further processing gave the 93-94 fraction to Hopkins. He will purify the uranium fraction further.

Hopkins has now run one bromate cycle on the 93-94 fraction and will finish the rest of the work this week.

Manning announced plans to bombard about 10^6 c/m of 95^{241} with neutrons at Argonne in order to check the half-life of 95^{242} and 96^{242}; Perlman then said that the Np^{237} sample from Hanford will arrive today.

Jaffey reported that Hyde and Studier have worked up the helium ion and deuteron bombardments of Th^{232} and have found evidence for the following reactions: α,2n; α,6n; d,n; d,2n; d,3n; and d,4n. Hyde reported the protactinium fraction grew alpha particle emitters.

Branch announced, in an official notice, that Labor Day, September 3, will be an authorized legal holiday.

The Np^{237} from the second special run at Hanford was received.

A meeting was held during the evening to discuss Rabinowitch's report on political implications and postwar plans. Rabinowitch listed eight possible comments on the atomic bomb crisis for discussion: (1) Solution is needed, and it ought to be the correct one. (2) Do not reveal the secret. (3) Let's corner the raw materials. (4) A defense is sure to be found. (6) Let's scatter our cities or dig underground. (7) If you smash our cities, we smash yours. (8) Let's prohibit atomic warfare.

Wednesday, August 29

My office received a summary dated August 27 from James of all data and conclusions reached in the work he, Florin, and Hopkins have done on targets 25DA (U^{235} plus 20 Mev deuterons) and 28DA (U^{238} plus 22 MeV deuterons). Ghiorso helped with the pulse analyzer, and Morgan used the soft electron counter. The conclusions are that the following reactions take place when U^{235} and U^{238} are bombarded with 22 Mev deuterons:

(1) $U^{235}(d,n)Np^{236} \xrightarrow[18\ hr]{\beta^-} Pu^{236} \frac{\alpha(4.3\ cm)}{T_{\frac{1}{2}}\sim 3\ yr(?)}$

(1') $U^{238}(d,n)Np^{239} \xrightarrow[2.3\ d]{\beta^-} Pu^{239}$

(2)* $U^{235}(d,p)U^{236} \xrightarrow{\alpha}$

(2') $U^{238}(d,p)U^{239} \xrightarrow[23\ m]{\beta^-} Np^{239} \longrightarrow Pu^{239}$

(3) $U^{235}(d,2n)Np^{235} \frac{\text{K capt(?)}}{\text{1 yr (?)}}$

(3') $U^{238}(d,2n)Np^{238} \xrightarrow[2\ d]{\beta^-} Pu^{238} \xrightarrow[60\ yr]{\alpha(4.1\ cm)}$

(4)** $U^{235}(d,3n)Np^{234} \frac{\text{K capt (?)}}{\text{5 d (may be } Np^{233})}$

(4')* $U^{238}(d,3n)Np^{237} \xrightarrow[2.2 \times 10^6\ yr]{\alpha}$

(5)* $U^{235}(d,4n)Np^{233} \frac{\text{K capt (?)}}{\text{short (?) (may be 5 d)}}$

(5') $U^{238}(d,4n)Np^{236} \xrightarrow[18\ hr]{\beta^-} Pu^{236}$

---

* Reaction not observed, but, by analogy, it must take place.
** It should be emphasized that mode of decay and half-lives of Np^{235}, Np^{234}, and Np^{233} are very tentative and only represent the best estimate possible at this time.

James suggested that when the metal U^{238} targets are available, an experiment where successive layers are shaved off should give conclusive proof or disproof of the d,4n mechanism.

Kohman addressed a memo to me, dated August 27, summarizing the results of the August 23 meetings that he, Weissbourd, Manning, and Perlman have held with Mallinckrodt and Army Representatives about the radium assay program.

Thursday, August 30

The Solvent Extraction Group met at 8:28 a.m. Blaedel, Gaarder, Gilbreath, Goeckermann, Hagemann, Lawroski, Manning, Perlman, Post, and Schaffner were present. Blaedel reported on the results of three Redox Process first cycle runs with uranium, but without plutonium. All runs were of at least 24 hours duration. Reducing power in all cases was satisfactory, but some difficulty was encountered with "slugging" and flooding of the columns. There was a discussion of the possibility of eliminating these difficulties through repacking the columns and enlarging the lines between the columns by replacing the tubing with pipe. Lawroski pointed out it would slow up the schedule, but Perlman said he was in favor of eliminating the present difficulties before proceeding to product runs. It was then agreed to change the piping, try an inactive run, and repack if necessary. This procedure will require about a week.

Gilbreath had no lab results to report but said Goeckermann is working on the use of aluminum nitrate in column 1B and Gaarder is determining viscosity, surface tension, and interfacial tensions on 1A column solutions. He is also making measurements of the viscosity of the uranium feed solution.

Kohman, in a memo addressed to me and dated August 27, discussed the possibility of observing radium E alpha particles. He proposes separating a sample of radium E, purifying it from polonium, and observing the radium E alpha particles before the polonium grows in too strongly.

Westrum sent Brewer the results of his measurements of the heats of solution and heats of formation of plutonium trichloride, tribromide, and oxychloride.

* * *

I received a teletype dated August 30 from Don Cooksey saying that Lawrence will be in New York Friday and Saturday (today and tomorrow) and that he (Cooksey) has wired Lawrence to call me.

Roy Thompson received the final draft of Chapter VII of Volume 16A of the Metallurgical Project Record, entitled, "Methods for Extracting Plutonium from Uranium" from Harrison Brown. Brown asks for corrections and comments by September 10.

Manning asked Wayne Johnson to give Pearline Boykin, a technician in the West Stands, an increase of $1.00/40-hour week for her conscientious work.

I also found on my desk a copy of some unknown Met Lab artist's concept of the atomic bomb. See Figure 9.

Steve Lawroski and I had dinner at the Palm Grove Inn (1780 E. 56th). Later in the evening I dropped in on the Ghiorsos to tell them of my plans to accept my offer from Berkeley. I wrote Helen before going to bed.

The Green Bay Packers defeated the College All-Stars at Soldiers Field in that annual game; the score was 19-7.

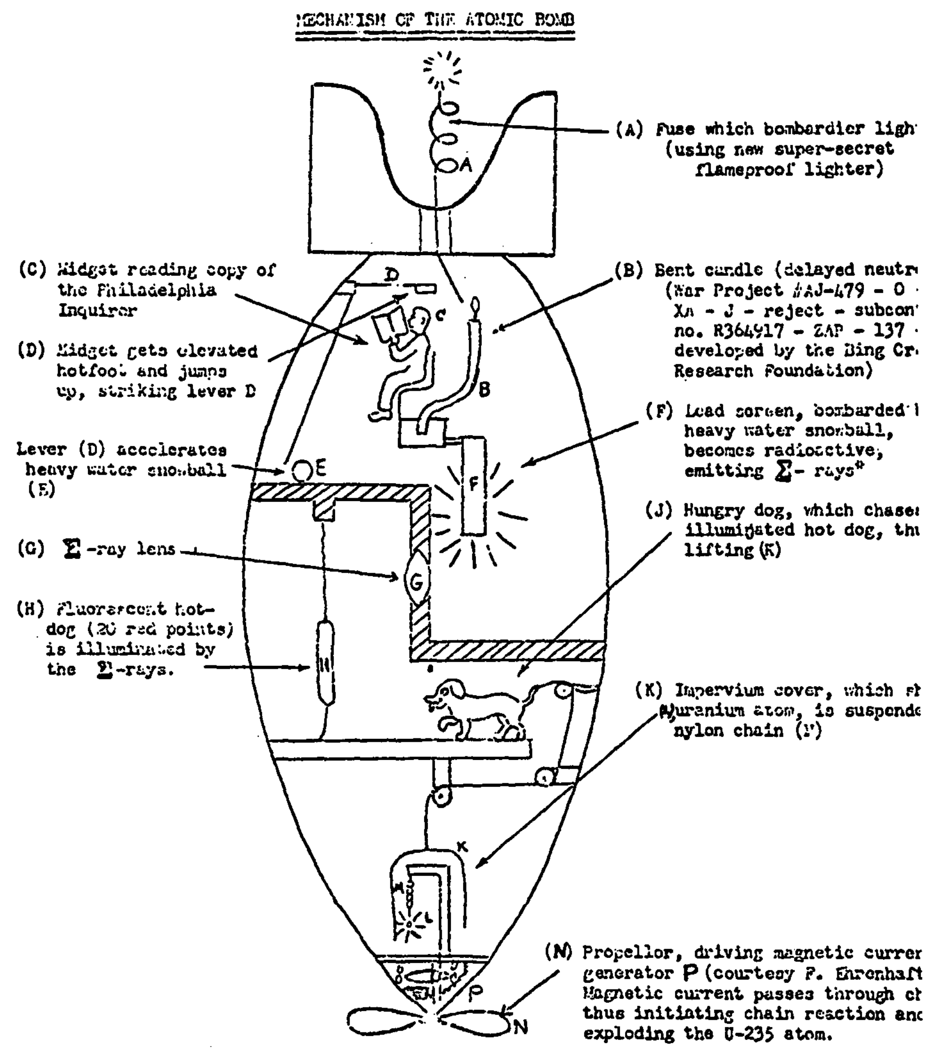


Figure 9. Unknown Met Lab artist's concept of atomic bomb.

SEPTEMBER 1945

Saturday, September 1, 1945

I read a note from Darrell Osborne dated August 30, saying he and his family are starting for Chicago by automobile on Saturday, September 1, and expect to arrive in Chicago on September 3. He will see me the next day.

I telephoned Ernest Lawrence at the Maystone Arms Hotel in New York. I told him of my talk with President Sproul in which Sproul agreed that my group would consist of a few men of academic rank, about ten research associates, and about twelve graduate students in the chemistry department with promise of higher salaries and subsequent expansion. I told Lawrence of my decision to come to Berkeley on the basis of such satisfactory arrangements. Lawrence thought I could begin to move people to Berkeley now. He also said the okay on the hot laboratory depends on a policy decision--whether such a building can be built on private (University) property. This decision will probably be made in a meeting in Chicago.

I then called Latimer in Berkeley. I told him about my conversation with Lawrence about the proposed hot laboratory. Lawrence will talk with him about it. Latimer was happy to hear this since he knows that Sproul is interested in Lawrence's opinions. I then mentioned that we shall send the blueprints for the hot laboratory to him today.

Latimer informed me that English's appointment is on a nine-month basis consistent with being on a government contract during the summer.

I immediately mailed Latimer the hot lab drawings along with a description of some of the features.

I then called Orlemann and English in Tennessee to tell them about the probability of positions in Berkeley.

Hogness prepared a summary of the manpower distribution in the Chemistry Division as of September 1. This showed the following for Section C-I.

	Heavy Isotopes		23
Section C-I	Kohman, Control Analysis		6
Seaborg	Stewart, Services		3
	Lawroski, Solvent Extraction		12
	Administration - Seaborg, Manning Perlman		3
		Total	47

When I returned to the apartment, I found a card from Jeanette. I ate dinner alone at the Palm Grove Inn. I then wrote Helen a letter before going to bed.

Allied dignitaries are arriving in Tokyo for the signing of the peace treaty. Russian, French, and Dutch delegations had a wild ride into Tokyo from the airport--one bus got lost; one got stuck in mud; one bus nearly overturned.

Sunday, September 2, 1945

The New York Times published a long "letter to the editor" from Warren Weaver, Director for the Natural Sciences of the Rockefeller Foundation. Weaver feels that the recent articles and editorials in the Times which express such ideas as "they solved it so rapidly because they were organized and competently directed; why should not the same principle be followed in peace?" could have disastrous results. Weaver argues that in order to advance, science must be free. He then says,

> We should, as a national policy, support science adequately. We should sponsor every movement and development that helps to create a favorable climate within which cooperative scientific enterprises may flourish... But we should not, in my judgment, try to set up some group of scientific supermen who, seeing, hearing, and knowing not only all of present science but also divining its mysterious future course, try to chart that course and tell all the rest of us what to do.

I had breakfast with the Ghiorsos. Then Al Ghiorso, Steve Lawroski, Stan Thompson, and I played 18 holes of golf at Timber Trails. I played with my old golf clubs. Steve and Stan won a "low ball plus low total" match, 4 up. (ST-105, SL-99, GS-108, AG-111.)

Steve Lawroski and I ate dinner at Palm Grove Inn. I wrote Helen a letter in the evening.

The peace treaty was signed on the U.S.S. Missouri. Thirty minutes after the signing, a 42-ship convoy steamed into Tokyo Bay and began to land U.S. troops, putting Japan proper under foreign military rule for the first time in Japan's history.

Monday, September 3, 1945

Today is Labor Day and an official Met Lab holiday.

The Osbornes arrived in Chicago and are staying at the Miramar Hotel.

I played 18 holes of golf at Indian Head Golf Club (Joliet and Wolf Roads) with Herman Robinson, Iz Perlman, and Winston Manning. (GS and WM won "low ball plus low total" match, 13 and 12. HR-116, IP-121, GS-99, WM-105.) I used Lawroski's clubs since my regular clubs have not arrived from California.

I then wrote a note to Helen before going to bed.

Over 3,500 U.S. troops are on Japanese soil and will start their march toward Tokyo as soon as the order is given.

Tuesday, September 4, 1945

Darrell W. Osborne started to work at the Met Lab. He has been working with Allegany Ballistics in Cumberland, Maryland.

Groups 1 and 3 met at 8:28 a.m. in the New Chemistry conference room. Asprey, Britain, Florin, Ghiorso, Hindman, Hopkins, Hyde, Katzin, Kohman, Magnusson, Manning, O'Connor, Perlman, Peterson, Robinson, Scott, Simpson, Stewart, Studier, S. G. Thompson, Van Winkle, and Westrum were present. I opened the meeting by arranging an experiment to milk Pu^{238} from the 96^{242} sample from 49NG (Hanford-irradiated plutonium from Farmer's No. 2). Morgan will carry out the work, using material to be supplied by Thompson. The purpose of the experiment is to verify the mass assignment of the 96 isotope thought to be 96^{242}. In answer to a question from Simpson, I said the half-life of 96^{242} is believed to be four to six months but a better value is now being obtained.

I asked about the status of sample 25NB (Hanford-irradiated U^{235}, Farmer's No. 3). Britain replied that not much has been done on it because of vacations, inspections, etc. I then asked about the neptunium fraction from this sample and learned that a plate has been prepared in order to look for Np^{237}. There should be approximately 1000 c/m.

Britain has plans to determine the isotope constitution of 25NO (Hanford-irradiated U^{235} from CW-1) as soon as possible. In response to my concern that Britain has too much to do, he answered that it was okay except for the outside work like preparing standards for the health group, etc. I told him to always check to see if such requests are cleared; he might still have to make the standards but would probably gain some time that way.

O'Connor reported that sample 49NH (Hanford-irradiated plutonium, sample CW-3) has been opened and the product dissolved. I told him to proceed as planned.

Manning discussed the status of the Pu^{239} concentration in sample 49NO. He said the ratio of Pu^{239} to uranium is about right for a factor of 5.4 between 49NO and 49NA. This is in line with the effective capture cross section for U^{238} of 7 or 8 barns rather than the 2 1/2 barns thermal neutron cross section.

In discussing the 93^{237} from the Hanford run, Hindman told us they extracted 96 percent out in one operation. This is considered relatively good. The sample is still too hot for accurate measurements.

I reviewed the thorium plus deuterons and thorium plus helium ion targets with Hyde and Studier. Hyde has set up a Pa^{230} sample (from the thorium plus deuteron target) in order that the U^{230}, to which it decays, can be extracted. Studier thinks the half-life of U^{230} is approximately 20 days. He also reported on the thorium plus helium ions target, saying it appears we got the α,5np reaction to yield Pa^{230} and perhaps the α,6n reaction to yield U^{230}. In view of this we will have Hamilton next bombard ionium with deuterons to produce protactinium isotopes. I also mentioned the bombardment of U^{233} with helium ions to produce neptunium isotopes, bombardment of Pu^{239} with deuterons to produce isotopes of element 95, and the bombardment of element 95^{241} with helium ions to produce isotopes of element 97. I said we want to be able to understand the decay systems of isotopes of odd elements so we will be better able to predict the behavior of the isotopes of 97.

After referring to the Jaffey-Asprey bombardment of a sample at Argonne which had 1.8×10^6 c/m of 95^{241}, I discussed with Asprey and Stewart the handling of 100 grams of plutonium for purification and 95^{241} production. We want to be able to get as much 95^{241} as possible because we have several good purposes for which it can be used--right now it might be considered critical material.

S. G. Thompson described his decontamination method for 95 and 96 that depends upon the fact that fluosilicate complexes the heavy isotopes more than it does the rare earths. He indicated the whole process will have to be worked out more completely. I suggested it would be a good idea to try a samarium-lanthanum separation with 95 present on the chance it has a +2 state similar to that of samarium.

Florin reviewed the work he, James, and Hopkins have been doing on samples 28DA and 25DA (U^{238} and U^{235} plus deuterons bombardments.) It appears they are getting the same 4.3 cm alpha-particle emitter from both bombardments.

The meeting ended with Simpson saying he will talk next time, Westrum noting he had nothing to report today, Van Winkle reporting he is continuing to isolate as much protactinium as possible before doing any experiments with stock supply, and Stewart telling the group that the triglycol dichloride and nitromethane methods of decontamination of plutonium are not working well. I suggested that he discontinue the investigations.

I read Segrè's response, dated August 29, to my letter of August 3 about the nomenclature of the new elements. He has nothing concrete to suggest in the way of names, but will think about it. He congratulated me for receiving the full professorship at Berkeley.

At the apartment I found that my golf clubs have arrived from Los Angeles. Steve Lawroski and I then had dinner at the Palm Grove Inn after which I went to a project meeting to discuss social and political implications of the atomic bomb. I wrote Helen a note when I returned home.

American occupation troops will enter Tokyo on Friday (the 7th). Today's paper indicates U.S. troops are just outside Tokyo.

There is also an item in this morning's paper indicating that there are "two kinds of atomic explosions." One is with fast neutrons and the other with "slow neutrons for atomic power."

Wednesday, September 5, 1945

I replied to Kennedy's letter of August 14, saying that I will sign the agreement between inventors if he wishes.

Perlman sent Captain Chapman information on the preparation for a special irradiation of thorium at Hanford. Spedding will prepare the thorium metal in the form of a billet; and Foote will supervise the extrusion into bar stock which can then be machined and canned. The canning will be done at Hanford.

I played nine holes of golf at Jackson Park with Al Ghiorso, Steve Lawroski, and Jim Watters after work. I again played with my old golf clubs since I have not unpacked the regular ones.

Steve and I again had dinner at Palm Grove Inn. I again wrote to Helen a letter before retiring.

Under a September 4th dateline today's newspaper says that Secretary of State Byrnes disclosed that President Roosevelt agreed at Yalta to let the Soviet Union take over the Kurile Islands and regain the southern half of Sakhalin Island.

Thursday, September 6, 1945

The Solvent Extraction Group met at 8:28 a.m. in the conference room. Blaedel, Gaarder, Gilbreath, Goeckermann, Hyman, Lawroski, Manning, Perlman, and Schaffner were present. I announced that people with six-month contracts would have them extended to a year, although shifts may be made later in the personnel of this group.

Blaedel reported on results of uranium runs (no plutonium) 8U through 11U. Mechanical improvements and the repacking of column 1A has significantly improved the uniformity of flow rates. Losses are considered quite low in spite of the fluctuations encountered. Lawroski indicated the next step will be to repack completely column 1A and go ahead with the plutonium runs.

Gaarder reported on work on physical measurements. These experiments were performed in order to furnish data necessary for column calculations and to throw some light on column behavior, e.g., slugging and flooding. He has made measurements of surface tension, interfacial tension, and viscosity on hexone and aqueous solutions simulating composition found in column 1A. His results were not conclusive and did not correlate well with settling time which is considered to be the most significant measurement. In answer to my question, Lawroski indicated they would like values of interfacial tension twice as high as are observed.

Goeckermann talked about the effect of aluminum nitrate on the uranium distribution ratio in column 1B. Aluminum nitrate seems to be more efficient than ammonium nitrate as a salting agent. Some batch decontamination runs showed that aluminum nitrate does not salt out fission products excessively.

I announced there will be another meeting next week, and the meeting adjourned.

I wrote to Latimer giving him some of my thoughts about the filling out of the Chemistry Department in Berkeley along lines he has outlined. I suggest he might consider M. R. Fenske of Penn State to head the chemical engineering work, J. L. Hoard of Cornell University as the crystal structure and x-ray man, and Winston Manning of this laboratory for research in photosynthesis. I also mention that in the group which might transfer to Berkeley in the nuclear program are Cunningham and Roy Thompson who might gradually transfer to biochemical work.

I rode to the railroad station with Fermi and Allison in order to talk with them about our joint experimental program--involving Los Alamos people and our people--on nuclear properties of heavy isotopes.

Stan and Alice Thompson invited Tom Jones and me to dinner. I then wrote Helen before going to bed.

Prime Minister Mackenzie King wants Canada to have its own flag and a national anthem separate from Great Britain, according to the paper today.

Friday, September 7, 1945

I held a meeting of the Control Analysis Group at 8:28 a.m. The meeting was attended by Anderson, Fineman, Kohman, Manning, Perlman, Sedlet, and Weissbourd. Kohman reported there has been no change in our relations with the St. Louis people. They are not going to use our emanation method for radium. They have, however, asked for a rapid, approximate radium analysis on the UNH-containing filtrate from the lead sulfate-gangue precipitations. A sample of the filtrate has been received and the radium content determined by the emanation method and by the proposed control method, precipitation of $BaCl_2$ carrier.

Fineman discussed the work on the radon emanation line. The variables that are being studied are the time of boiling off of the radon, the flow rate of gas through the line, and the effect of acid concentration and mixtures of acids. I stated that it would be wise to continue work on the emanation method for a while at least, pointing out it is valuable in studying emanation isotopes in new series and from branching in known series. I said that we are planning a half-life determination of radium.

Today Jeanette traveled from New York to Washington where she will visit the Howsers and Esther Hedstrom until Sunday. Mrs. Hilma Howser and Esther, sisters, are cousins of our mother.

This morning's paper carried information about and the text of President Truman's 18,000-word message to the reconvened Congress, outlining his policies for reconversion to peace.

Saturday, September 8, 1945

I sent a request to Daniels for about 100 grams of plutonium of 250 gt level or higher for use primarily as a source of the isotope 95^{241}.

Report CS-3131, "Chemistry Division Summary Report for August, 1945" was prepared by the Chemistry Division Director's Office and issued today. All Section C-I information in this report appears in the Perlman-Manning August 21 memo to Hogness summarizing the work for the period July 15-August 15.

Jeanette is visiting our relatives in Washington today.

U.S. troops entered Tokyo today headed by General Douglas MacArthur. There was a simple ceremony at 11:00 a.m., September 8 (9:00 p.m., September 7, Chicago time), according to this morning's paper.

Sunday, September 9, 1945

I played 27 holes of golf with Stan Thompson and Tom Morgan at Evergreen. (TM-113, ST-94, GS-97 for 18; TM-57, ST-50, GS-incomplete card for nine holes.)

Helen is scheduled to leave Los Angeles by bus today to start her trip home.

According to the paper, MacArthur issued two statements in Japan today--one pledging that Japanese militarism will be stamped out and the other assuring his troops that they will be sent home as quickly as the peace can be secured.

Monday, September 10, 1945

I received a letter dated September 7 from Gordon Leader in Richland, Washington. Leader said that he is sorry that he was unable to come to Chicago in June when we had an opening. Now that the war is over he would like to inquire whether we might have an opening this fall. Leader says that, at Hanford, he has some opportunities to work on fission products, but that work is interrupted by plant work. He is not able to do his best work under such circumstances. Social life in Richland is dull, and he would like to return to Chicago if possible.

Jeanette arrived in Chicago from her visit to Washington.

There has been a great deal in the papers the past few days about the proposed inquiry into Pearl Harbor. According to this morning's newspaper, Senator Homer Ferguson (Republican from Michigan) makes a plea to keep politics out of the inquiry.

Tuesday, September 11, 1945

The meeting of groups 1 and 3 in the New Chemistry conference room at 8:29 a.m. was attended by Asprey, Britain, Cunningham, Florin, Fried (see Figure 10), Ghiorso, Hindman, Hyde, James, Kohman, Magnusson, Morgan, O'Connor, Osborne, Perlman, Peterson, Robinson, Scott, Simpson, Stewart, Studier, Van Winkle, Weissbourd, and Westrum. I announced that Allison has given us the results of mass spectrographic analysis on the plutonium and U^{235} CW-2 samples. The percent of Pu^{240} in the plutonium sample is 4.4 ± 0.05. This is slightly higher than our values. The ratio of U^{235} to U^{236} is 60 to 1 (our value: 1 to 2 percent of U^{236}).

I asked about the status of CW-0 and CW-3 uranium samples, and Britain replied that he has made up plates but has not completed the decontamination. Hopkins is working on the 93, 94 fraction of CW-3, but he must wait a week to allow the Np^{239} to decay before determining the amount of Np^{237}.

I next asked about the status of CW-3 plutonium and learned that O'Connor has run the first ether extraction with an 87 percent yield which represents 80 mg. I asked O'Connor to purify all the material to look for beta particles and then we shall see what should be done. We will want this material for bombardments.

CBB 797-9283

Figure 10. Sherman Fried in front of Jones Hall, August 30, 1945.

Morgan outlined the procedure he and Stan Thompson plan to use to attempt to find element 97. They intend to isolate the cerium fraction from the salt layer of 49NH (Hanford-irradiated plutonium, CW-3) and then separate cerium by fluosilicate precipitations. This should lead to detection of any alpha particles, and Morgan thinks the chemistry will identify any 97 isotope.

Asprey described his work on the bombardment of 95^{241} at Argonne. After bombardment the sample was dissolved and two fluosilicate precipitations carried out to reduce the beta level. Decay curves showed an 18-hour beta decay and a hard component with no decay; these might be, respectively, 95^{242} and 95^{243}. The possibility of whether the activities are from these isotopes or from rare earth impurities was discussed at length.

Stewart said that he is preparing for the milking of the 13.5 grams of plutonium from the Np^{237} run. We discussed the processing of the 100 grams of high gt plutonium that I have requested. Stewart believes he may be able to handle it in glass equipment. I suggested he make all necessary preparations for stainless steel equipment even if he does not use it.

James reported another point on the 95^{241} decay curve still indicates a 40-year half-life. He also reported on additional work on sample 49αB (plutonium plus 40 Mev helium ions Berkeley bombardment). The 95, 96 fraction was milked for any plutonium daughters and gave about 2,000 alpha-particle c/m of range 4.3 cm. The x-rays from the 95, 96 fraction decayed with a half-life of about one month.

I asked about the status of the 4n + 2 series. Hyde reported that the U^{230} alpha decay obtained from the alpha or deuteron bombardments of Th^{232} is followed by five successive alpha-particle decays, the fifth presumably being the RaC'; this is known. He gave the half-lives of the decay products, and I pointed out they decrease right down the line.

Hindman talked about the isolation of Np^{237}; his assay indicates at least 180 mg of Np^{237}. He plans to start bromate cycles.

Van Winkle said he is still carrying out ion adsorption separations on some of the fractions of the protactinium work.

I announced Peterson will be given the job of determining the cross section for the reaction $Ra^{226}(n,\gamma)Ra^{227}$; this in turn will give Ac^{227} by beta decay. If the cross section is as large as 10 barns, it will be feasible to make 1 mg of Ac^{227} at Hanford.

I asked that anyone interested come to my office to read a chemistry report we have prepared for Lawrence, Oppenheimer, Fermi, and Compton. This reviews future chemical research and covers, we think, important aspects connected with the atomic bomb. I said we have suggested a program for about 500 men. I postponed the reporting from the rest of the people until next Tuesday.

James began alpha-particle decay curves on the 93-94 and 95-96 fractions from a 93 fraction isolated on August 21 from the sample 49αB-15a3 (95-96 fraction from the plutonium plus 40 Mev helium ion bombardment of March 1945).

I transmitted to Allison a detailed proposal of the chemical research for the use of the Scientific Panel, which is preparing a set of recommendations on a nucleonics program for the Interim Committee. In his August 15 letter, Allison asked me to prepare the material on research in chemistry. My program is described under the following headings and includes a listing of priority. It will utilize the services of 430 men: (1) Development of Natural Sources of Uranium and Thorium, highest priority, 25 men; (2) Fission Product Studies, highest priority, 30 men; (3) Preparation and Study of Heavy Isotopes, highest priority, 15 men; (4) Basic Chemistry and Physical-Chemical Properties of the Heavy Elements, highest priority, 30 men; (5) Study of Natural Radioactive Isotopes, medium priority, 10 men; (6) Radiation Chemistry, high priority, 25 men; (7) Basic Radiochemical Studies, medium priority, 10 men; (8) Solvent Extraction, highest priority, 40 men; (9) Research in Adsorption Processes, medium priority, 15 men; (10) Recovery of Valuable Substances from Pile Waste, high priority, 5 men; (11) Chemical Methods for Isotope Separation, highest priority, 50 men; (12) High Temperature Chemistry, highest priority, 20 men; (13) Improvement in the Present Methods of Chemical Processing for Weapon Production, medium priority, 15 men; (14) Chemistry of Less Familiar Elements, medium priority, 25 men; (15) Methods of Chemical Analysis, high priority, 40 men; (16) Remote Control Chemical Equipment, high priority, 10 men; (17) Development of Electronic Instruments for Radiochemical Research, high priority, 25 men; (18) Improvement in Microchemical Techniques, medium priority, 10 men; (19) Chemical Studies in Connection with the New Machines for the Production of High Energy Charged Particles, medium priority, 30 men.

I also transmitted a proposed program of chemical research in connection with the production of U^{233}, under the following headings, which will utilize the services of 115 men: (1) Small-scale Production in Present Type Natural Uranium Piles, highest priority, 15 men; (2) Large-scale Production in Converter Piles Using Enriched U^{235} or Plutonium as Fuels, highest priority, 100 men; (3) U^{233} Breeder Piles, medium priority. This problem will follow No. 2 above and the men can be shifted as necessary.

I tell Allison that the report on chemical research needed in connection with production of plutonium will be sent in conjunction with the report which Zinn is preparing.

I phoned Joe Hamilton and told him about the decay scheme starting with mass 230 that we have observed. He said that the bombardment 9b (Io^{230} plus deuterons - 75 microampere-hours) was finished at 9:00 a.m. today and will leave Berkeley on Friday and arrive in Chicago on Sunday or Monday. He said that bombardment 11b (depleted U^{238} plus deuterons) was bombarded the same time as bombardment 9b and received about 200 microampere-hours. Hamilton said he is leaving Berkeley on Thursday and will arrive in Chicago by Sunday. Hamilton then said that bombardment 8a (U^{233} plus helium ions) will be completed in about ten days. Bombardment 10a (Th^{232} plus helium ions--pure thorium metal from Spedding--25-50 microampere-hours) will also leave Berkeley on Friday. Bombardment 9a (Io^{230} plus helium ions) and bombardment 11a (depleted U^{238} plus helium ions) will be simultaneously done at a later date when the ionium arrives in Berkeley.

We discussed the visit of Louis Jacobson to Chicago, and I asked that he come to the Metallurgical Laboratory on his way to the Clinton Laboratories. Hamilton said that Jacobson will make this visit in about ten days.

Hamilton and I then talked about the fact that the yield of U^{232} from thorium plus deuterons bombardment is about twice as great at 22 Mev as at 20 Mev.

Jeanette played golf with Stan and Alice Thompson at Jackson Park.

"U.S. Army to Arrest Tojo!" reads this morning's headline. MacArthur has ordered the "roundup of Japanese war criminals."

Wednesday, September 12, 1945

In a letter dated September 10, P. W. Selwood of the Chemistry Department of Northwestern University, invited me to speak at a meeting on November 16, of the Chicago Section of the American Chemical Society. Selwood has been asked to arrange a symposium on recent developments in nuclear studies. Groves has no objections although he points out that the speakers would be restricted in their remarks. Selwood also mentioned that he is inviting Cyril Smith and Enrico Fermi to participate.

Morgan completed purification of the sample of 96^{242} received from Thompson. He performed five oxidation cycles, to remove plutonium, using dichromate oxidations and LaF_3 precipitations. He set the resulting solution aside to allow the Pu^{238} to grow in for two to four weeks.

I wrote to Selwood, accepting his invitation to speak at the ACS Symposium on November 16.

I teletyped to Allison at Site Y to state we received about 230 mg of LCW-2 (Hanford-irradiated U^{235}) and found about 215 mg after chemical treatment. I say no one here can understand how contamination with natural uranium could have occurred here. I also tell him decontamination of sample CW-3 (Hanford-irradiated U^{235}) is almost complete. We plan to measure the isotopic composition of its uranium soon.

I sent Zinn, for incorporation in his section of the report to the Scientific Panel, my description of the chemical research needed in connection with the production of plutonium. I identify the chemical problems involved in (a) Improvement of Hanford Operations, (b-1) Plutonium Produced as a By-Product in Enriched Uranium Piles, (b-2) D_2O Moderated Piles Using Slurries or Solutions of Natural Uranium, (b-3) D_2O Moderated Natural Uranium Lattice Piles, (c) Beryllium Moderated Piles, and (d) Breeder Piles.

I met with A. T. Cape of Coast Reductions, Inc., 2 W. 45th Street, New York, New York, in New Chem. Cape told me that one of the men in his company found the thorium-uranium mineral in which we are interested in 1937 in the black sand of the beach at Monterey, California. The mineral consists of 60 percent ThO_2, 20 percent U_3O_8, and 20 percent SiO_2; the deposit extends from Monterey north to San Francisco. Cape gave me a sample that has a density of 7.4. He said that his company owns 10,000 feet of beach right at Monterey Bay from which they have taken out 50,000 tons of this black sand for roofing paper. Cape said that he works at Ohio State University. His home address is 40 W. Dominion Blvd., Columbus, Ohio; Jefferson 2166.

Tojo has shot and gravely wounded himself; his chances for survival are fifty-fifty, according to this morning's paper.

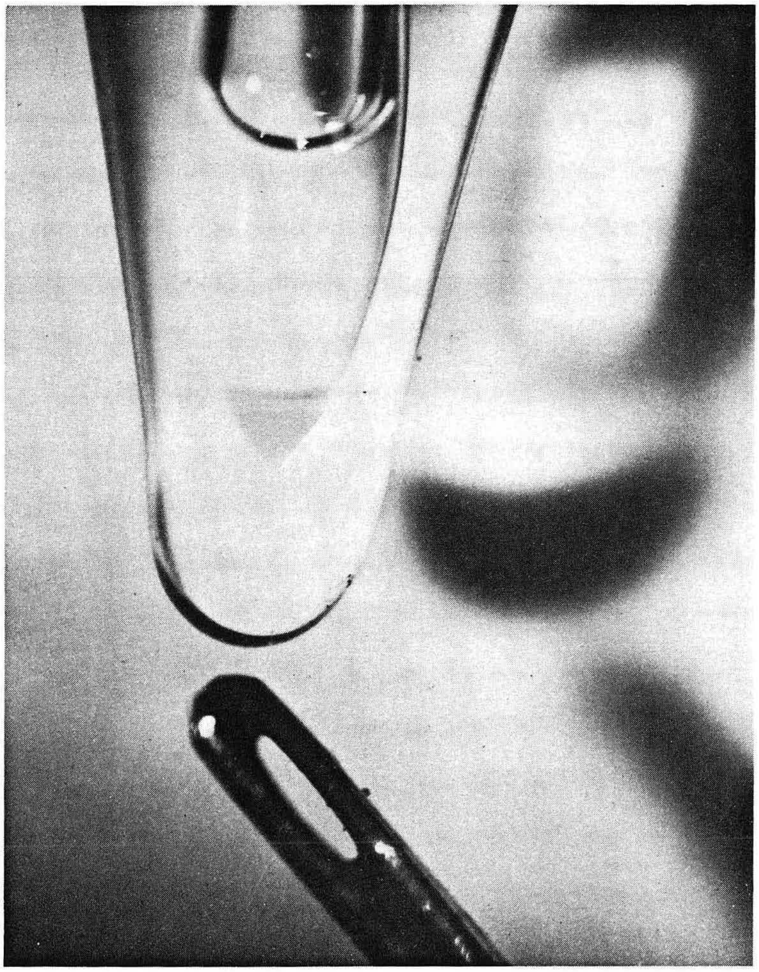
Thursday, September 13, 1945

The Solvent Extraction Group of Section C-I met at 8:28 a.m. in the New Chemistry conference room; the meeting was attended by Blaedel, Gilbreath, Goeckermann, Hyman, Kelley, Lawroski, Manning, Perlman, and Schaffner. Blaedel reported three more runs completed since last week (11U to 13U) and that the first plutonium run (14U1P) is under way. About 3 mg of plutonium per ml are being used. The few measurements so far show a plutonium loss of about 0.7 percent. There was a discussion of conditions which could be changed to reduce this loss.

I asked if they will be able to get out a report in October, pointing out it would be worthwhile to have a rather complete report by then that offers something in the way of column design for the final equipment. After a discussion of capacities for which to plan and the difficulties in making accurate cost estimates, I said I thought a real effort should be made to produce a good report by the middle of October.

Cunningham finished isolation of the first sample of pure element 95--95^{241} hydroxide. See Figure 11.

Manning sent an official request to Captain Chapman for Berkeley bombardments of ionium plus deuterons (75 μah), Q metal (U^{238}) plus deuterons (200 μah), and thorium metal plus helium ions (50 μah).



CHEM 2516

Figure 11. First sample of 95^{241} hydroxide, isolated September 13, 1945. The sample is seen in bottom of solution in capillary tube and is shown photographed with an ordinary sewing needle for comparison.

I read a memo from Robert Maurer to the members of the group engaged in discussion of the social and political implications of atomic power. Maurer, in discussing international control of atomic power and the weapons derived from it, attempts to demonstrate that international control is unsatisfactory as a permanent solution. Since it is assumed that a system of international control will include three features: (1) free publication and exchange of information, (2) a system of accounting and allocation of raw and finished materials, and (3) agents of each nation will have the right of inquiry and search in all areas of other nations, he believes that such a system will mean that each nation will have to yield a fraction of its sovereignty. By yielding only a fraction of its sovereignty, a nation implies it is unwilling to sacrifice more. International control presupposes atomic power in the hands of more than one nation, and with atomic power possessed by all, each nation will fear every other nation. Maurer concludes by stating, "If a temporary system of international control is adopted, it is justified only by a vigorous effort upon the part of each major nation toward the formation of a world union."

In another request to Captain Chapman, Perlman asked for some thorium metal to be obtained from Spedding. The items desired are a thorium foil and some samples of thorium metal to be used by Hanford to test the ease of machining and its canning properties.

Helen arrived in Chicago by bus from Los Angeles. I met her at the bus station. Jeanette left Chicago to return to Los Angeles.

Today's paper indicates that Tojo will survive his self-inflicted gunshot wound below the heart.

Friday, September 14, 1945

Morgan gave me a copy of the paper, dated September 14, that he will present at the Information Meeting at Clinton next Monday. The title of Morgan's paper is "Chemistry of Elements 95 and 96." He covers the history of the elements, the tracer chemistry which shows that elements 95 and 96 behave like rare earths, the separations processes, and the nuclear data. He concludes with a summary of the nuclear data presently known.

		α range	β range	$\sigma(n,\gamma)$	$t_{1/2}$
Pu^{241}	β^-		0.6 mg/cm^2		100 y
95^{241}	α	4.05 cm		3000-5000 b	50 y
95^{242}	β^-		200 mg/cm^2		17-18 hr
96^{242}	α	4.75 cm			3-5 mo

I teletyped Allison at Site Y to ask several questions: what ratio of U^{238} to U^{235} and what ratio of U^{234} to U^{235} did he find in LCW-2 (Hanford-irradiated U^{235}); what was the isotopic ratios in the uranium of CW-1 (Hanford-irradiated U^{235}) and in the original unirradiated uranium foil; how much uranium from CW-3 does he need for mass spectrographic determination; and what was the minimum value of 241 to 240 ratio he found in KCW-2 (Hanford-irradiated plutonium).

Hogness sent Daniels the outline of a report to the Advisory Committee. For my section, he reports on the Redox Solvent Extraction Process runs made with pure uranium nitrate and the one run made with a mixture of uranium and plutonium nitrates. He indicates the results are very satisfactory; the plutonium was separated from the uranium with a loss of plutonium of only 0.7 percent and a purity of greater than 99.9 percent. More than 99 percent of the uranium was recovered.

The decay chain of Pa^{230}, formed by bombarding Th^{232} with 22 Mev deuterons, is presented; this chain constitutes a new 4n + 2 series. Other heavy isotope work described includes the formation of Np^{236} by the deuteron bombardment of U^{235} and U^{238}, the demonstration of fissionability of Th^{229}, the possible fissionability of Pu^{240}, the formation of element 96^{242} by Argonne pile irradiation of element 95^{241}, and the use of fluosilicate complexes to separate elements 95 and 96 from lanthanum.

A. T. Cape called me from Chicago. He told me that next Tuesday and Wednesday he will be at the Fort Shelby Hotel in Detroit and after that he will be home.

After work, I played nine holes of golf at Evergreen with Manning and Stan Thompson (WM-59, ST-47, GS-53).

An article in the paper today with the dateline of September 12 tells of the first open assembly held in Korea in 25 years. Lt. General John R. Hodge is commanding U.S. occupation troops there and has removed the Japanese governor general from office.

Saturday, September 15, 1945

Frank Tomkins made a spectrographic analysis of the element 95 sample isolated by Cunningham on September 13--the first measurement of element 95 emission lines.

A letter dated September 13 arrived from Leonard Dreher, who has been vacationing in southern California and is about to return to Richland. Dreher includes a clipping about a talk given by a physicist from the University of Southern California, who flatly predicts it will be possible to obtain atomic power not only from uranium and plutonium, but from any other element, and even "a handful of dirt." The physicist also states that only a small power source should be required to produce substances of unbelievable energy content. Dreher then describes various possibilities he is considering, although he has made no decision, and asks whether I have decided whether I would go to Berkeley or stay in Chicago. On an additional note, he mentions that they were in the area when the Baumbachs became proud parents of a boy.

I also received a letter, dated September 13, from Jerry Howland. Howland tells me about the unsatisfactory personnel policies and sloppy lab procedures there at Dayton. He states that none of the Chicago men expect to receive good recommendations from the company. Howland also encloses two newspaper clippings from Dayton papers that, although not untrue, are misleading about the role of Monsanto's leadership in the atomic bomb research program.

I phoned E. O. Lawrence at Berkeley and told about the thorium-uranium ore deposit at Monterey Bay. We talked about my impending move to the Radiation Laboratory at Berkeley. He mentioned he will stop in Chicago on a United Airlines flight due to arrive at 11:13 a.m. Tuesday morning, and I said that I would meet him at the airport. We agreed this would give us time to talk about the move of my colleagues and me to Berkeley. He will then take a train to Washington or New York about 3:30 p.m.

I called Joe Hamilton. He has sent three packages to Chicago--bombardment 9b (Io^{230} plus deuterons), bombardment 11b (depleted U^{238} plus deuterons), and a package for Farrington Daniels which will go to Clinton Laboratories. He said that he has not done bombardment 10a (Th^{232} plus helium ions) yet. He said that he will do bombardment 8a (U^{233} plus helium ions) next for a total exposure of 50 microampere-hours and send it by air in about ten days.

We discussed, in some detail, the way the samples are mounted on a grooved platinum-iridium plate and the heat dissipation problem connected with this.

I told him that we will send him a sample of 100,000 alpha-particle c/m of 95^{241}. He said that he will make the usual formal request through the Area Engineer's office.

I received and read a carbon of an acknowledgement Westrum sent Brewer of the receipt of the thorium preparations. He discussed some heats of formation.

Radiation surveys by the Health Division for the week ending September 15 show the following rooms in New Chemistry had above maximum permissible levels of alpha, beta, or gamma contamination: Room 4, Room 10, Room 11, Room 20, Room 30, Room 37, Room 41, Room B-2. Similarly unsatisfactory conditions were found in the West Stands in Room 218 (used for chemical work for Ghiorso's group), Room 217, and Room 212.

Blaedel and Morgan are leaving tonight by train (PRR) to attend the September 17 Information Meeting at Clinton Laboratories.

Under the dateline of September 14, this morning's newspaper reports that Russia reputedly "on good authority" is asking the Big Five council of foreign ministers for some of Italy's Mediterranean possessions --possibly the Dodecanese Islands.

Sunday, September 16, 1945

I played 18 holes of golf at Timber Trails Golf Course with Foster York, Stan Thompson, and Steve Lawroski. Foster and Stan won "low ball plus low total" match, 2 up. (FY-97, ST-99, GS-110, SL-89.)

John McCormack, famous Irish tenor, died at his home in County Dublin tonight at age 61.

Monday, September 17, 1945

Two Berkeley bombardments were received in Chicago: bombardment 9b, ionium plus deuterons (75 microampere-hours ending 9:00 a.m. September 11) and bombardment 11b, depleted U^{238} plus deuterons (200 microampere-hours ending 9:00 a.m. September 11).

I received a request dated September 13 from Kennedy at Site Y, for me to return three signed copies of the agreement among the four inventors.

I immediately signed the agreement that provides that all rights in the inventions described in Cases S-52 and S-61 are to be held in common and all financial benefits are to be shared equally among Segrè, Wahl, Kennedy, and me.

Blaedel, Manning, and Morgan presented papers at the Information Meeting at Clinton Laboratories today. Blaedel talked about the Redox Solvent Extraction Process (as of September 15). Manning summarized recent work on heavy isotopes in Section C-I, while Morgan reported on the chemistry of elements 95 and 96.

Under an article with the dateline September 16, today's paper says the Japanese battleship Nagato is being towed out to sea for an "atomic bomb experiment" to test the effect of a single bomb on one ship, on a task force, and what effect it will have on water.

Tuesday, September 18, 1945

At 8:28 a.m. I held a meeting in the New Chemistry conference room of Groups 1 and 3. It was attended by Asprey, Britain, Cunningham, Florin, Ghiorso, Hindman, Hopkins, Jaffey, James, Katzin, Kohman, Magnusson, O'Connor, Osborne, Perlman, Peterson, Robinson, Scott, Simpson, Stewart, Studier, R. Thompson, S. G. Thompson, and Westrum. We first talked about the CW-3 plutonium sample. O'Connor said he has now completely decontaminated 65 mg. He will use the total sample for gamma-ray absorption curves and then will give 20 mg to Florin who will make a sample for Westrum for specific activity and spontaneous fission measurements. I discussed other experiments we planned for this material, including the bombardment of five mg at Argonne to look for Pu^{243} or an alpha-emitting daughter. We shall put some of it back in the Hanford pile to build up the higher order reactions and then bombard with helium ions to try to produce 96^{245} which may be a beta emitter. I also mentioned that we may put some 95^{241} in the Hanford pile to form high order 96 isotopes and perhaps element 97.

I asked about the uranium sample of 25NB (Hanford-irradiated U^{235}, Farmer's No. 3). Britain has determined the specific activity and now has 150 mg of uranium that is purified. I observed that the cross section we have been using for the capture of neutrons by U^{235} (90 barns) seems inconsistent with the U^{236} found by mass spectrographic analysis and the depletion of U^{235} determined by fission assay.

Hopkins reported he has isolated a total of 5,000 Np^{237} alpha-particle c/m from 25NB. This gives a cross section for U^{236} of about 12 barns using the percent of U^{236} found by Ghiorso and a neutron capture cross section of 92 barns for U^{235}.

Ghiorso reported finding the following ratios in sample 25NB:

$$\frac{Pu^{238}\text{ alphas}}{Pu\text{ alphas}} = 12\text{ percent (compared with 7.7 percent in 25NA)}$$

$$\frac{Pu^{239} + Pu^{240}\text{ alphas}}{\text{total alphas}} = 66\text{ percent}$$

$$\frac{Pu\text{ alphas}}{\text{total alphas}} = 76\text{ percent}$$

I mentioned we are beginning to think about making plutonium metal targets for bombardment in the Berkeley cyclotron, and I said it is time now to start a request for the metal from Site Y while we plan the methods of mounting.

Cunningham described the latest 95 experiments and said that earlier in the isolation, a sample was submitted for spectrographic analysis. It contained considerable amounts of lead and iron. Later another sample was submitted that contained about 1/10 microgram of lanthanum and 0.01 microgram of iron. The total sample contains 4 x 10^7 95^{241} alpha-particle c/m, so we have about 1 microgram of 95, assuming a half-life of 40 years. Cunningham now intends to submit another sample of the material identical with that weighed for the specific activity determination.

I asked if there was any data yet on the Io^{230} plus deuterons bombardment (Berkeley bombardment 9b). Studier replied he has isolated the protactinium fraction and a uranium fraction and intends to separate the uranium and thorium from the protactinium fraction after the Pa^{232} has all decayed.

Hindman talked about the isolation of Np^{237}. He said that he has 140 mg in solution, and mentioned some of the Np^{237} is to be shipped to Hanford to replace the previous smaller sample we sent.

Westrum presented a graph showing the heats of solution of the various chlorides as a function of HCl concentration.

Simpson then reported on the precipitation of some oxide crystals by volatilization from a tantalum crucible at 2,000°C. Zachariasen has been unable to get a pattern for the unusual reason that the crystals were too large.

Peterson is working almost full time on the experiment for determination of the capture cross section of Ra^{226} by bombardment at Argonne.

I suggested some possible methods for detecting 97 in 49NH (Hanford-irradiated plutonium, sample CW-3). First, Stan Thompson is to start with a dissolver solution and work on the idea of a III-IV couple and isolate the cerium fraction. He will consider the IV to be like that of thorium. Second, I mention that O'Connor has isolated a fraction from the supernatant of the PuF_4 precipitation. He has counts that seem to have a range of 4.5 cm and may be element 97. The meeting was then adjourned.

I received a telegram dated September 17 from Lawrence saying he will arrive in Chicago at 11:26 a.m. today via United Airlines flight 32. I met Lawrence at the airport. We talked until he left by train at 3:30 p.m.

Morgan returned to Chicago from his trip to Clinton Laboratories at 2:15 p.m.

President Truman made several important appointments today. He has appointed Senator Burton, Republican of Ohio, to the Supreme Court, succeeding Justice Owen J. Roberts; Undersecretary of War Patterson to succeed Secretary Stimson. Justice Roberts has been appointed chairman of a board to award medals of merit to civilians.

Wednesday, September 19, 1945

At 8:00 a.m. Blaedel returned to Chicago from the Information Meeting at Clinton Laboratories.

James received sample 28DB (U^{238} metal plus deuterons) Berkeley bombardment 11b of 200 microampere-hours. He milled off three layers 0.002 inch thick and started chemical processing to isolate the plutonium fraction from each layer.

I received a reply dated September 17 from Allison to my queries of September 14. He offers the following uranium isotope ratios for L-CW-2: 28/25 = 1.014 ± 1 percent; 25/24 = 154 ± 15 percent; 25/26 = 60. In the original material before it was irradiated at Hanford, 25/24 = 168 ± 15 percent and 28/25 = 0.560 ± 1 percent. The following values apply to CW-1: 25/24 = not run; 25/26 = 464 ± 10 percent; 28/25 = 0.554 ± 1 percent. He informs me that Swinehart wants to make a run on the irradiated plutonium in CW-3 and would like to have two or three metal pellets of 8-10 mg each as in the past.

Perlman and Manning sent a jointly-authored memo to Hogness, summarizing the work of Section C-I for the period August 15 to September 15, 1945. Results described are as follows: (1) Further analysis of the decay of the alpha-particle activity of the plutonium fraction from the enriched U^{235} plus helium ion bombardment shows the remaining alpha-particle activity is now decaying with a half-life of three to four years and is probably Pu^{236}. Measurements of this plutonium fraction in a Geiger-Müller counter give data that suggest two plutonium isotopes (perhaps Pu^{237} and Pu^{235}) may be decaying by an orbital-electron capture mechanism. At least one of the isotopes also emits alpha particles. (2) The plutonium activity that has grown in the 95-96 fraction isolated several months ago from a Pu^{239} plus helium ion bombardment shows an alpha-particle activity characteristic of Pu^{236}. The conclusion is that the 29-day alpha-particle activity originally observed in the 95-96 fraction was due to 96^{240}, rather than 96^{241} as we had previously speculated. (3) The half-life of U^{236} was set at 2×10^7 years, as a result of measuring the alpha particles from a sample of U^{235} containing about 2.7 percent U^{236} formed by a prolonged neutron bombardment at Hanford. The results are in agreement with an earlier measurement on 1.7 percent U^{236} material. (4) The alpha-particle decay of the 4.05 cm range alpha emitter attributed to 95^{241} has now been followed for five months and indicates a half-life of about 40 years. The element 95 used was derived from 25 gt Hanford plutonium. Similar samples from much higher gt material have now been prepared for half-life observations. (5) A sample of relatively pure 95^{241} has been bombarded with neutrons in the P-9 pile at Argonne and the beta-particle decay of 95^{242} has been observed directly. The half-life is 17 to 18 hours. (6) Further work on the deuteron-bombarded thorium metal has yielded the new isotope Pa^{230}. It decays by beta emission with a half-life of approximately two weeks to yield a new isotope of uranium, U^{230}, whose decay chain has been partially identified chemically and appears to be the following:

$$U^{230} \xrightarrow[\text{3 weeks}]{\alpha} Th^{226} \xrightarrow[\text{30 min}]{\alpha} Ra^{222} \xrightarrow[\sim\frac{1}{2}\text{ min}]{\alpha} Em^{218} \xrightarrow[<1\text{ sec}]{\alpha} RaC'(Po^{214}) \xrightarrow[10^{-4}\text{ sec}]{\alpha} RaD$$

(7) A new method of decontaminating elements 95 and 96 has been developed. It is based on a finding that fluosilicate ion forms more firm complex ions with elements 95 and 96 than it does with the rare earths. (8) Redox Solvent Extraction Process runs have demonstrated that it is possible to maintain both uranium and plutonium losses in the first column below 0.1 percent. Plutonium losses in the second column have ranged from 0.5 to 1.5 percent; it is expected that chemical modifications will result in even lower losses. The second cycle columns have been completed and are ready for operation. (9) In the uranium ore control analysis work, the techniques and the apparatus for emanation analysis are being improved, with a resultant increase of accuracy of about 1 percent. Work is continuing on the direct precipitation method for radium analysis.

At 2:30 p.m. in Room 302 of the Social Science Building I attended the opening session of a three-day conference on Atomic Energy Control, held under the auspices of the University of Chicago. There were 46 invited official participants in the Conference: Chester I. Barnard, President, New Jersey Bell Telephone Company; Walter Bartky, Professor of Applied Mathematics, Acting Dean of Physical Sciences Division, University of Chicago; William Benton, Assistant to the Chancellor, University of Chicago; Bernard Brodie, Research Associate, Institute of International Study, Yale University; E. C. Colwell, President, University of Chicago; E. U. Condon, Associate Director, Westinghouse Research Laboratory, East Pittsburgh, Pennsylvania; F. G. Cottrell, former Director, Fixed Nitrogen Laboratory, Washington, D.C.; Farrington Daniels, Professor of Physical Chemistry, University of Wisconsin and Director of Metallurgical Laboratory, University of Chicago; Karl K. Darrow, Bell Telephone Laboratories, New York; Sebastian de Grazia, Instructor in Social Sciences, The College, University of Chicago; Edward Meade Earle, Professor in School of Economics and Politics, Institute for Advanced Study, Princeton, New Jersey; William T. R. Fox, Research Associate, Institute of International Study, Yale University; James Franck, Professor of Physical Chemistry, University of Chicago; Carter Goodrich, Professor of Economics, Chairman of International Labor Office, Columbia University; Reuben Gustavson, Vice-President and Dean of the Faculties, University of Chicago; Philip Hauser, Assistant Director of the Bureau of Census, U.S. Department of Commerce, Washington, D.C.; Selig Hecht, Professor of Biophysics, Columbia University; David Hill, Physicist, formerly California Institute of Technology, now University of Chicago; Thorfin R. Hogness, Professor of Chemistry, University of Chicago; Robert M. Hutchins, Chancellor, University of Chicago; Irving C. Langmuir, Research Laboratory, General Electric Company, Schenectady, New York; Harold D. Lasswell, War Communications

Research, Library of Congress, Washington, D.C.; C. C. Lauritzen, Professor of Physics, California Institute of Technology; David Lilienthal, Director, Tennessee Valley Authority; Adolph Lowe, Professor of Economics, Graduate Faculty of Political and Social Science, New York; Jacob Marschak, Professor of Economics, University of Chicago; Charles E. Merriam, Professor Emeritus of Political Science, University of Chicago; Reinhold Niebuhr, Professor of Applied Christianity, Union Theological Seminary, New York; James J. Nickson, M.D., Metallurgical Laboratory, University of Chicago; Robert Redfield, Professor of Anthropology, Dean of Social Science Division, University of Chicago; Winfield Riefler, Professor, School of Economics and Politics, Institute for Advanced Study, Princeton, New Jersey; Kurt Riezler, Professor of Philosophy, Graduate Faculty of Political and Social Science, New York; Beardsley Ruml, R. H. Macy and Company, Chairman Federal Reserve Bank of New York; Henry N. Russell, Professor of Astronomy, Princeton University; Theodore Schultz, Professor of Agricultural Economics, University of Chicago; Edward A. Shils, Assistant Professor of Sociology, University of Chicago; Eugene Staley, Institute of Pacific Relations, San Francisco; Joyce Stearns, Dean of Faculties, Washington University, former Director of Metallurgical Laboratory, University of Chicago; Leo Szilard, Metallurgical Laboratory, University of Chicago; Oswald Veblen, Professor of Mathematics, Institute of Advanced Study, Princeton, New Jersey; Jacob Viner, Professor of Economics, University of Chicago; Henry Wallace, Secretary, U.S. Department of Commerce, Washington, D.C.; Joseph H. Willits, Director for Social Sciences, Rockefeller Foundation, New York; Louis Wirth, Professor of Sociology, University of Chicago; Harold Urey, Professor of Physical Chemistry, University of Chicago; E. P. Wigner, Professor of Physics, Institute for Advanced Study, Princeton, New Jersey.

The scheduled program for the next three days is the following:

I. The Atomic Bomb: General Evaluation

II. Consequences of the Atomic Bomb Under Conditions of National Sovereignty
- A. Influences on Military Strategy and International Relations
- B. Economic Aspects

III. International Control
- A. Control Through Existing International Organizations
- B. Control by Mutual Inspections
- C. Techniques of Moving Toward World Government

IV. If International Control Is Unachievable: The Alternatives
- A. Dispersal of Cities
- B. Secrecy in Science

V. Individual Statements on Policy

Robert M. Hutchins made the opening remarks and included a message from General Leslie Groves about security. Hutchins said that no statement to the press is planned although this could be changed if the Conference so decides. The question of any documents coming out of the Conference or any continuing work resulting from the Conference will be considered on Friday.

Hutchins then introduced Leo Szilard who made the opening statement. He described the Hiroshima and Nagasaki bombs, and indicated that the latter was the worse of the two. He described the potential for later, more powerful bombs and the advent of additional nuclear weapons powers. He indicated that in ten years, it will be possible to make much more destructive nuclear weapons, that is, weapons whose radius of destruction will be increased from one to ten miles. Russia will be able to develop an atomic bomb with a yield equivalent to 10 million tons of TNT if there is not agreement banning such development. Szilard said he mentioned Russia specifically because Russia is the most important country with respect to inclusion in an agreement. He thought that Russia might be able to produce an atomic bomb in 2½ years. He conceded that the U.S. still has a number of secrets but said that Russia may have uranium deposits and could probably develop the atomic bomb at less cost than was the case for the U.S. Szilard predicted that in six years Russia will have enough atomic bombs to destroy all our cities. He said that it is necessary to have international control of atomic energy and that this should include inspection of mining activities and manufacturing activities to prevent cheating under the international agreement. He thinks that such an arrangement would be a great step forward; under such an inspection arrangement, it would take one-half to one year to make atomic bombs after the agreement was broken. Szilard described the vulnerability of the U.S. to atomic bombs. He suggested a ten-year plan under which 60 million people would be relocated and costing $15 billion per year, but it should be done. The new cities should be one mile wide and 50 miles long and contain 100,000 to 500,000 people. He suggested that the attainment of world government would be a step-by-step procedure taking place over a period of some 15 to 20 years. He said stockpiles of atomic bombs may not be able to keep the U.S. in its former favorable position.

Irving Langmuir asked whether radioactive poisons are worse than atom bombs. F. L. Friedman responded that they cannot cover a larger area and they cannot be stockpiled. Franck and I agreed with Friedman. Someone asked about radioactive products from bombs. I avoided the question of fissionable products from the bomb, but Franck answered by saying the amount of dispersal is not known. Someone else asked if it were possible to reduce the minimum mass. Szilard said he thought not. Another question was how bombs could be brought in, and Szilard said by (1) planting (no ways of detecting), (2) airplane, and (3) rockets. Szilard then said we could make enough bombs within five to ten years to destroy all major cities of the world. Szilard said atomic power is economically possible in ten to 15 years, but in automobiles it would be greater than 15 years because of radiation problems. Brodie asked whether rockets can use atomic power propulsion. Szilard replied he thought it was not needed even to go around earth. Langmuir disagreed; he thought 3,000 miles was the limit with the present fuels.

There was considerable discussion of Hutchins' query of whether peacetime development of atomic energy is limited by science or economics. Franck stressed tracer application.

Lilienthal asked about defense against atomic bombs, and Hill replied there is no defense.

Szilard then said we should use our present bargaining power to force Russia to reciprocal agreement of inspection, otherwise there will be a war in two to three years; although someone said this would be difficult because there is insufficient political cause.

There was a question raised as to whether Russia has industrial capacity, and Langmuir said that, as a result of his visit, he thinks they have tremendous scientific possibilities and the biggest government-supported program in the world. He feels they will catch up with us in five years and in ten years will be way ahead of us.

Veblen made a good statement on secrecy: he said just let the public know; the secrecy stopped when the bomb was used. Hutchins said the Smyth Report asks for public discussion. Ruml suggests publicly issuing of 100 words chosen from the Smyth Report.

Lowe suggested that we agree that all nations remain vulnerable and that dispersion, etc., is causus belli. Staley discussed the consequences of secrecy, and Niebuhr suggested releasing the secrets without bargaining. Veblen also favors giving the secrets to Russia and everybody now. Langmuir suggested we prepare a statement that we are in favor of no secrets (based on the Smyth Report) and to issue it with the consent of the War Department. He is willing to discuss it with the State Department, President, etc.

At 8:00 p.m. I attended the evening session of the Conference in Judson Court Lounge. Hutchins again opened the meeting. Hill reported for the committee composed of Hill, Langmuir, Szilard, Franck, and Niebuhr who suggested preparing a report recommending no secrecy, have it cleared by the Secretary of War, and also prepare a letter to the War Department to find out the basis for its policy. The action about adopting it was deferred until later.

Letters from Einstein, Fermi, and Bridgman were read. Einstein feels there are not scientific questions left, but only political ones. Bridgman's letter was anti-Russian and said that it will take the Russians ten years to develop the bomb. Langmuir, Franck, Rabinowitch, and I disagreed with Bridgman's letter after a strong statement by Hecht.

Brodie then gave a 30-minute paper on the military significance of the bomb. The discussion continued until 10:15 p.m., and we were left the question whether more than one nation possessing bombs will bring on a war or will prevent a war via fear.

Senator Alben Barkley has been appointed chairman of the Pearl Harbor investigating committee, according to this morning's newspaper.

Thursday, September 20, 1945

At 9:30 a.m. I attended the session of the Atomic Energy Control Conference in Room 302 of the Social Science Building. After preliminary announcements by Hutchins, Viner opened the meeting with a statement on the economic implications of nuclear weapons and of the concern of strategic foreign policy. He said that the accomplishments to date indicate that nuclear weapons constitute the cheapest way of killing and that there is no financial limit. Some 15 or 20 countries could easily afford the $2 million or so that is required. He asserted it would not be possible to have world government in a two-power situation with giants like the U.S. and Russia; although it might be feasible among seven or eight powers. He also feels the deterrent factor will make the atomic bomb a peace making factor; the chief deterrent is that the victor himself must bear too great a cost.

Viner said that perhaps small countries could deter the start of war now; that up until World War I, such countries could remain independent and they could again now. Niebuhr challenged this statement saying that small countries are improved. There was much discussion on this point with Canada given as an example. Schultz offered that small countries may get bombs without making them. Marschak spoke for a short-term interest agreement and long-term road toward world government. Earle brought Germany into the discussion, as the power not vulnerable to atomic power. However, Germany will rise again due to coalition of powers.

Fox began speaking at 11:20 a.m. on international control. He feels no country will hesitate to use the atomic bomb against the U.S. for moral reasons since we used it first. Treaties are not good and complete abolition of the manufacture of bombs is the only type of control that should be considered. He does not want the present United Nations to have atomic bombs; however, control scheme should be attached to the United Nations. He feels inspection is worthwhile even without control system. Fox said the U.S. should stop production during the period of discussion provided the other nations stop construction. The United States should make a complete disclosure as soon as the United Nations has responsible system functioning. We should then reveal the size of the present stockpile which should be large enough to do damage but not cause obliteration.

The Conference continued with Shils speaking on the subject of control by general inspection at 2:30 p.m. in Room 302 in the Social Science Building. Shils believes government monopolies are necessary. For effective detectability of disposition and destination of raw material, inspections of research laboratories and manufacturing of atomic bombs are essential. There must be segregation of military uses of research from civilian application. Existing mines must be inspected. Shils suggests inspection in countries that have nuclear laboratories; this must be done by physicists. He feels the United Nations organization should monopolize research on military uses. Shils claims that scientist citizens must have free access to visit other countries, mines, laboratories, etc.

Riezler suggests the U.S. continue to make atomic bombs and announce that we will not use them without approval of the United Nations. He said inspection of the type Shils and Fox suggest would mean complete and radical change of the Russian system, which is impossible. Russia would have to introduce a "Bill of Rights." Franck suggested that it would be better to reach agreement after both Russia and the U.S. have atomic bombs. We need Congress and people to agree before we can approach Russia, and three years is insufficient; we will need 15 years to sell the American and Russian people on the idea.

Szilard said we should not assume that Stalin or the U.S. Congress will not accept this system of complete inspection; we should not be prophets, but let us ask Stalin because Russia has a good record of doing things where her interest lies. Riezler commented that negotiation itself may be dangerous because it may build distrust when counter proposals are rejected. Veblen suggests we join in collaboration with Russia exactly as we now do with the British.

Eventually Niebuhr asked whether we should not drop the inspection idea and work toward mutual trust. Hecht replied that no physicist would be an inspector and the inspection idea is preposterous. He said, "Let's tell Congress and the public tomorrow that there will be no secrets, no defense, etc."

The session ended with Earle expressing the fear that we are talking ourselves into a war with the Russians.

In a weighing experiment today Cunningham determined the specific alpha activity of 95^{241} to be 5.75×10^6 dpm/μg corresponding to a half-life of about 600 years.

James completed the isolation of the plutonium fractions from the three layers milled from sample 28DB (U^{238} metal plus deuterons Berkeley bombardment) that he started yesterday. He measured the alpha-particle activity and ran pulse analyzer curves on the three samples. He found that the 4.3 cm alpha particle of Pu^{236} accounted for a much larger percentage (52 percent) of the total alpha activity in the first layer than in subsequent layers (1.5 percent in the second layer and essentially none in the third layer). He concludes from this that the correct mechanism for the formation of the Pu^{236} is

$$U^{238}(d,4n)Np^{236} \xrightarrow{\text{beta}} Pu^{236},$$

as he proposed in MUC-GTS-1928.

The meeting continued at 8:00 p.m. Lasswell was the first speaker, and he spoke on the World State and the technical possibilities for achieving it. Rabinowitch asked whether a union with Britain is a step toward World Union or against it because of the threat to Russia. Lasswell said he believes it would lead to the latter. There was an intensive discussion of whether we should initiate a campaign to educate the public without consulting the U.S. government. Franck asked how can we make Russians agree to freedom of speech, if it is foreign to them. May it not lead to an explosion? We must educate our own people. Do we weaken the bargaining position of our government by making a lot of noise now? Barnard answered by saying this calls for active political experts of both internal and external politics. Franck then discussed the Interim Committee and Scientific Panel (four men). The question of embarrassing official channels was then again raised. Darrow asked whether the U.S. is not more alarmed than Europeans because we have never been bombed. Both Shils and Szilard said alarm in England is greater than here. Riezler is doubtful whether this Conference should go on a publicity campaign without consulting the U.S. government, but Niebuhr is not certain that we should consult the U.S. government since this Conference has a different perspective.

One of the headlines in this morning's paper reads "Shirley Temple Weds Her Soldier" (John Agar). The event was covered by a long article by Hedda Hopper, including descriptions of the crowd of 10,000 outside the church in Los Angeles, all hoping to catch a glimpse of this former child movie star.

Friday, September 21, 1945

Morgan sent Daniels an abstract of his talk on the chemistry of elements 95 and 96 at the Information Meeting at Clinton Laboratories.

Kohman transmitted to A. Q. Butler of the Mallinckrodt Chemical Works the emanation analysis results that he has obtained since their last report of August 7.

At 10:00 a.m. I attended another session of the Atomic Energy Control Conference, which was presided over by Redfield. He announced that groups under Szilard, Langmuir, Viner, and Niebuhr are to each prepare a statement during the day about (1) trend of national policy, (2) instruments to bring this about, and (3) continuing responsibility of social and physical sciences in this country.

Hauser then spoke about the confused thinking of people in Washington, D.C. There are nine bills before Congress. He feels that most false conclusions are drawn from the concept of secrecy and said that it is not safe to assume that the President has other than military advice on either control of atomic energy or support of research.

During the afternoon the four groups met to prepare reports to present during the evening session. The Szilard group met in Goodspeed 101. Szilard himself stressed that an armed peace is not durable and that permanent peace demands a World Government. We must achieve this in 20-30 years. He said that the later it comes, the worse it will be for us. He also feels that the winner of the next world war will impose World Government. Szilard suggests that we have only 10 percent chance to achieve World Government without war. He feels that we should plead for modesty and humility because of this limited 10 percent chance.

The Langmuir group met on the second floor of the Social Science Building. Langmuir was of the opinion that we cannot establish World Government in two or three years; in five to ten years we may reach a state where we will have some security in the whole situation. However, Russia will have the bomb in two to five years. He believes confidence must be developed between the United States and Russia. We must not start the program with threats or coercion.

The Viner-Niebuhr groups met in the Commons Room of Swift Hall. Niebuhr stated that we know that the scientific principles are broadly known; therefore there is no scientific secret. He feels the problem of informing the public is important, but the Army is tending toward being a political power group. Niebuhr said that inspection, suppression, or control of the atomic bomb (or world government) is not the primary problem; but he feels that effort to control the bomb may accentuate the possibilities for conflict. The tragic fact, according to Niebuhr, is that World Government does not belong to this era though there is a threat of complete annihilation.

Viner said that he agreed with Szilard; he feels that the atomic bomb did not change the problem, but only changed the urgency. He also believes that World Government is not in the picture, especially with large nations like the United States and Russia. He said the conceivably small powers will achieve status and there will be a multi-atomic power situation. The worst case is that only two nations have the power.

Lilienthal then discussed the scientists' political responsibility about fighting the censorship of their utterances.

Redfield said the latest news from Washington, D.C., is that the Army is well on the way to forcing the most stringent legislation limiting even peacetime applications of atomic energy.

The Conference concluded with a session in which summaries were made.

The Senate is still delaying the appointment of Dean Acheson as Undersecretary of State. Two conservative senators (one Republican and one Democrat) are highly incensed because they say Acheson has said that Japanese policy would be made in Washington and not at MacArthur's headquarters. They say Acheson has insulted that "great general."

Saturday, September 22, 1945

Radiation surveys by the Health Division for the week ending September 22 show that the following rooms in the New Chemistry Building have radiation conditions that should be corrected in the near future: Room 1 (used for protactinium work), Room 2, Room 6, Room 7, Room 11, Room 13, Room 33, Room 34, Room 36, Room 37, Room B-2.

I had lunch with Henry Wallace, Secretary of Commerce, and others who attended the Conference.

Emilio Segrè had dinner with Helen and me in our apartment.

Today's newspaper reports that General Eisenhower's command hopes to end American government in Germany within the next 15 months and would reduce U.S. occupation to a token force of a few thousand by that time.

Sunday, September 23, 1945

I played 18 holes of golf at Navajo Fields (123rd Street and Ridgeland Avenue, Worth, Illinois) with J. R., Paul Fields, and Steve Lawroski. Steve and I won a "low ball plus low total" match, 3 and 2 (JR-113, PF-121, GS-109, SL-111).

Today President Truman announced he personally will make the decision, when the time comes, on his administration's policy toward development and use of atomic energy, and the atomic bomb. "He will have to make it," he emphasized.

Monday, September 24, 1945

Opaline Calhoun began working as a technician for Fineman today.

Cunningham determined the specific alpha activity of a further-purified 95^{241} sample as 6.7×10^6 d/m/μg. This corresponds to a half-life of 498 years.

I received a letter dated September 12, from Paul Bartlett of Harvard University asking whether I could provide any information about the availability of a graphite brick from a plutonium pile as a source of carbon enriched in C^{14}.

I read a letter from Dreher dated September 22. He has made no decisions about his future, but he has concluded southern California is not so appealing as it once was. He is considering remaining at Hanford and asked many questions about my opinion about the future of the Hanford plant.

Today's paper reports, under a September 23 dateline, that Stafford Cripps, president of Britain's board of trade, indicated that Britain did not intend to share "the atomic bomb secret" with all major powers. He warned that there must be a world federation within ten years or civilization will be destroyed by "atomic power."

Tuesday, September 25, 1945

At 8:28 a.m. I held a meeting in the New Chemistry conference room of Groups 1 and 3. The meeting was attended by Asprey, Britain, Cunningham, Florin, Ghiorso, Hindman, Hopkins, Hyde, Jaffey, James, Katzin, Kohman, Magnusson, Osborne, Peterson, Robinson, Scott, Simpson, Stewart, Studier, R. Thompson, S. G. Thompson, and Westrum. I opened the meeting with a discussion of sample CW-3 (Hanford-irradiated U^{235}). No measurements are yet available. I observed it is worth hurrying since we are seeking a seven-day decay.

Cunningham reported he has a spectrographic analysis on his latest sample of 95^{241}. It shows two predominant impurities about 3 percent lanthanum and 6 percent platinum. After correcting for these impurities, Cunningham calculates a half-life of 470 years for the isotope mixture (96^{241} and ?). I suggested, as an explanation for this long half-life (compared with the values of 50 years reported by James and 4-5 years reported by Sullivan), the possibility that a small amount of a short-lived isotope, such as 96^{243}, might be present in James' or Sullivan's samples. If this postulate is true, we should be noting a higher order of reaction. I asked James to follow closely differences in the half-life of 95^{241} samples from all gt levels available--25, 170, 800, and 1300. Cunningham will attempt an electrolytic reduction of 95^{241} to search for the +2 state.

Asprey reported he is working steadily on purification of the 13 g of plutonium from the 93^{237} run.

At my request, James described his work on 28DB (U^{238} + 22 Mev deuterons). Analysis of successive layers milled from the target show that the threshold for the d,4n reaction is about 20 Mev.

I asked about the ionium-deuteron target. Studier has found a new peak at 4.0 cm in the protactinium fraction that decays with a 35-hour half-life. Other tests of the protactinium fraction indicate that the reaction occurring is $Th^{230}(d,3n)Pa^{229}$, followed by decay to the 4n + 1 series.

I discussed the processing of the 49NH samples (Hanford-irradiated plutonium from CW-3) by Florin and Westrum (this is being done to prepare metal samples for Site Y). I agreed they could experiment with the use of freon for fluorination. During the discussion I learned the bottom dropped out of their cylinder of purified HF. This led me to observe that it must have been an attractive experience.

In answer to a question from me, Jaffey indicated he will very soon be able to do the bombardment at Argonne of the uranium from 25NA (Hanford-irradiated U^{235} from CW-2) to look for U^{237}.

Hindman mentioned he is in the process of collecting all the neptunium fractions from the big run.

Van Winkle reported on the status of the protactinium work; he has 15 mg through two solvent extraction steps and 10 mg through one step.

I asked Stan Thompson about the element 97 studies. He has processed a portion of the original solution of 49NG (Hanford-irradiated plutonium from CW-2) and prepared some plates of the heavy isotope fractions that Ghiorso reports may contain small amounts of unidentified activity. Stan and Morgan may have more to report at the next meeting.

Peterson talked about his work on radium. He is attempting to get a sample of radium ready for bombardment at Clinton and working out methods of analysis.

Simpson described his work on fission product separation by volatilization, using irradiated, depleted uranium oxide. There appears to be quite good fractionation of some of the elements.

Westrum and I discussed the question of plutonium metal targets and decided to ask Site Y for some 25 mil, gamma-stabilized, plutonium foil.

In O'Connor's absence, I mentioned that the plutonium from 49NH (Hanford-irradiated plutonium from CW-3) seems to contain low energy electrons which are harder than the ones Morgan observed in 49NG (Hanford-irradiated plutonium from CW-2). There is a possibility of a new isotope coming in at the high level of bombardment.

In answer to my question on decay data on 96^{242}, James said it seems to be following the line already drawn, that is, about four to five months half-life.

I commented we must be certain to calculate the amount of 95 activity resulting from growth in plutonium samples produced at various gt levels if there is a new isotope appearing at high gt.

In answer to a question from Stewart, I indicated that 100 g of plutonium (as a source of element 95) should be in at any time. Stewart remarked that he and Asprey would like to rest before attacking the purification of that sample.

James analyzed the alpha decay curves started September 11, on the 93-94 and 95-96 samples derived from a 93 fraction he isolated on August 21 from sample 49αB-15a3 (95-96 fraction from plutonium plus 40 Mev helium ions bombardment of March 1945). He finds that the amount, range, and half-life of the plutonium alpha particles at 4.3 cm that grew in are all consistent with what is known about Pu^{236}. He concludes that the 29-day element 96 he identified on June 7 is 96^{240} decaying to Pu^{236}.

I received Case S-61 and Case S-52 from the Patent Office today. The latter consists of nine separate cases totaling about 100 pages and containing 180 claims. The Patent Office has asked us to give thought to the possibility of corroboration of each of the 180 claims and would like to have the inventorship determined for each of them.

I then wrote to Kennedy enclosing three copies of the agreement between inventors which I have signed. I tell Kennedy I have received Cases S-61 and S-52 from the Patent Office today and will send them on to him through Metcalf's office here after reading them. I explain the desires of the Patent Office about corroboration and determination of inventorship and suggest that it would expedite this time-consuming job if they can come to Chicago or if I go to Site Y for a series of conferences among the four of us. I indicate that it does not make too much difference to me which direction the traveling goes.

L. B. Arnold called me from Boston. He told me that he has been investigating the use of radioactive indicators to test for leaks; for example, the use of C^{14} as the dioxide to test for leaks of nitrogen or air. He has discussed this with Robley Evans. I described for him the type of samples of C^{14} that might be available for such an application.

I telephoned Captain Chapman in the Area Engineer's Office to give him all the essential information on the source of thorium at Monterey Bay as described to me by Cape.

The second edition of a useful document, "A Manual of the Radiochemical Determination of Fission Product Activities" by D. N. Hume, N. E. Ballou, and L. E. Glendenin, was issued today.

In today's paper is a long article dealing with the future of the world due to "the bomb." Senator Sheridan Downey (Democrat from California) said the "secret of the atom bomb" should be turned over to the new League of Nations. Senator Downey said,

> It (the bomb) demands international control, or it will mete out international chaos. No other nation would agree to stop research as long as we--and we only--held the secret. If, on the other hand, we committed the secret to the world council, then there would be every reason to encourage it, with the hope that atomic energy would be exploited and controlled for the good and not the evil of mankind.

Wednesday, September 26, 1945

G. L. Martin and A. Q. Butler of Mallinckrodt Chemical Works in St. Louis are again visiting the Met Lab today and tomorrow to discuss the uranium ore control analysis work with Kohman and others.

A letter arrived for me dated September 18 and addressed to the Department of Chemistry, University of California. S. C. Lind, Dean, Institute of Technology, University of Minnesota, Minneapolis, said that he did not know whether I had returned to Berkeley or was still in New Mexico [sic]. Dean Lind is looking for an addition to the School of Chemistry. Names that have been referred to him are A. C. Wahl, S. G. English, R. B. Duffield. Lind asks for my opinion of these men and the names of any others who might have an interest in inorganic chemistry and an interest in nuclear chemistry.

I replied to Bartlett's letter of September 12 about the availability of C^{14} and mention the information he requested may still be in the class of restricted information. However, I give him an estimate of 10^3 d/m of C^{14} beta particles per milligram of graphite in a brick from the Hanford plutonium pile. I also point out the possibility of forming high specific activity C^{14} by irradiating nitrogen-containing compounds in piles. I promise to look into the availability of such materials.

Manning prepared an abstract of the paper he gave at Clinton Laboratories on September 17, entitled "Summary of Some Recent Work on Heavy Isotopes in Section C-I of the Metallurgical Laboratory." He reviews the work on new isotopes of neptunium and plutonium by James, Florin, Hopkins, and Ghiorso, saying that Np^{236} has been found in U^{235} and U^{238} targets after bombardment with deuterons from the Berkeley cyclotron. Plutonium-236 has been found in plutonium manufactured in the Hanford pile. Plutonium-236 has also been produced in helium ion bombardments of uranium. We also have evidence of other new plutonium isotopes.

Manning then describes the work of James on the growth of plutonium from the alpha decay of isotopes of element 96.

The work of Hyde, Studier, Jaffey, and Ghiorso on the Pa^{230},U^{230} decay chain is reported:

$$U^{230} \xrightarrow[3\ \text{wks}]{\alpha} Th^{226} \xrightarrow[30\ \text{min}]{\alpha} Ra^{222} \xrightarrow[0.5\ \text{min}]{\alpha} Em^{218} \xrightarrow[1\ \text{sec}]{\alpha} Po^{214} \xrightarrow[10^{-4}\ \text{sec}]{\alpha} \text{RaD}.$$

Manning also covers the fission measurements of Ghiorso, Studier, Cunningham, and O'Connor. He states that the thermal neutron fission cross section of Th^{229} is about 50 b, that of 95^{241} is less than 3 b, while that of Pu^{240} is less than 100 b. (Manning states that the accuracy of the results is low because the plutonium sample was only 4.2 percent Pu^{240}.)

I answered a recent letter from W. A. Felsing of the Department of Chemistry at the University of Texas. He asked a number of questions about the abilities of George Watt. I said that Watt had the confidence of his group, supervised both the work and the reports of the group, cooperated with his peers, was amenable to suggestion although he had a mind of his own, and is experienced in a field that is bound to be great in the future.

I replied to Gordon Leader's letter of September 7. I say that we still want very much for him to join the Met Lab staff, but that presently our budget is oversubscribed. However, I am certain that a number of people will soon leave to return to former jobs or to enroll in graduate school. Therefore, I think there will be an excellent chance that we will be able to offer him a position within a few months for the interval ending June 30, 1946. We do not know the future of the laboratory after that date, but, I say, that he will be in a better position to find the type of job he wants (such as teaching) if he is in Chicago.

Blaedel sent Daniels the abstract of the paper he gave at Clinton Laboratories Information Meeting on September 17, entitled, "Redox Solvent Extraction Process as of September 15, 1945." He reviews the runs made in the columns in the West Stands beginning August 1, which consisted of 13 runs with only uranium in the solution and two runs with uranium plus tracer plutonium. Blaedel indicates a third series of runs will be made with uranium, tracer plutonium, and fission products. So far, results are a little better than anticipated on the basis of batch data.

At the meeting of the foreign ministers of five nations in London, according to today's paper, Britain and the Soviet Union are pressing for a quarter share in control over Japan. Secretary of State Byrnes has told Molotov (Soviet Foreign Commissar) that the Pacific is not on the agenda of the London meeting.

Thursday, September 27, 1945

Hamilton notified me in a note dated September 24 that he is sending the two water-cooled plates to which the plutonium targets (on platinum plates) should be fastened. He also tells me the U^{233} sample is well on its way to adequate bombardment.

"Army Rule Clamped on Argentina; Leaders Arrested" is one headline in this morning's newspaper. President Farrell of Argentina has clamped an "iron state" of siege on Argentina and suspended constitutional guarantees of free speech and other freedoms.

Friday, September 28, 1945

The Solvent Extraction Group met at 8:28 a.m. in the New Chemistry conference room. The meeting was attended by Ader, Blaedel, Gaarder, Gilbreath, Goeckermann, Hausman, Hyman, Kelley, Lawroski, Manning, Post, Schraidt, and me. Gilbreath reported rather encouraging results of four runs with uranium and plutonium and one run with uranium, plutonium, and fission product elements.

Hyman said that during the past few weeks he and Goeckermann have been studying what is happening on column 1B. They have concluded that hydrazine complexes plutonium into a hexone-soluble state in the absence of uranium; at the same time the hydrazine is a necessary constituent since in its absence the ferrous ion quickly disappears and the distribution ratio of the plutonium falls. Increased sulfate improves the distribution ratio in the extraction process.

Lawroski reported the second cycle has been tested with simulated solutions and the mechanical operation appears to be satisfactory. A run will be made Monday with the product from first cycle run 17U4P (plutonium, but no fission products).

The Chicago Cubs made sure of a tie for the National League pennant. One victory by the Cubs or one defeat for the St. Louis Cardinals will give the title to the Cubs.

Saturday, September 29, 1945

Louis Werner wrote me from Oak Ridge in a letter dated September 24 expressing his pleasure at my offer of a research fellowship at the University of California. He would like to know when I plan to return to Berkeley and said he would like to return to Berkeley as soon as possible. The possibility of transferring to the Radiation Lab particularly intrigues him.

I promptly replied to Werner's letter, explaining that the Regents have not had time to act on my proposal so I do not know the date at which the move to Berkeley will take place. However, Latimer, Lawrence, and Sproul approve of the plan. I estimate the move will occur within the interval of two to eight months although I shall know something more definite within a month. I also say he may show my letter to Bill Knox, Dan Miller, and Dan Koshland.

High winds hit Chicago this afternoon. High waves flooded sections of the Outer Drive. Some trees were blown down, and underpasses were flooded.

Sunday, September 30, 1945

Stan Thompson, Steve Lawroski, Iz Perlman, and I played eleven holes of golf at Timber Trails Golf Course. (ST-55, SL-55, GS-51, IP-58 for nine. ST-66, SL-69, GS-65, IP-68 for eleven.)

The Cubs won 5-3 over the Pirates in their final game of the season. They clinched the pennant yesterday.

Monday, October 1, 1945

A. T. Cape called me from Columbus, Ohio. He told me that they have analyzed the thorium ore from the Monterey Bay area with the following results: ThO_2 - 70.40 percent, U_3O_8 - 11.18 percent, SiO_2 - 18.22 percent. Cape wants to know whether we have any interest in this ore. People are considering purchasing the surrounding land, and this may make it difficult to buy the critical area later on.

An organization chart of Section C-I shows the following personnel as of October 1, 1945:

Glenn T. Seaborg - Section Chief
Ruth P. Rogers - Secretary to Seaborg
Kathleen Florin - Clerk

Winston M. Manning - Associate Section Chief
Isadore Perlman - Associate Section Chief
Jane Horwich - Secretary
Dorothy Black - Secretary, part-time from Information Office
Mildred A. Bolden - Secretary, on loan from Information Office
Norma D. Shaw - Draftsman, on loan from Information Office

Group 1, Heavy Isotopes,
Seaborg, Glenn T. - Group Leader
Cunningham, Burris B. - Research Associate
Ghiorso, Albert - Research Associate
Hindman, J. Clark - Research Associate
Hyde, Earl - Research Associate
Katzin, Leonard I. - Research Associate
Jaffey, Arthur H. - Research Associate
James, Ralph - Research Associate
Magnusson, Lawrence - Research Associate
Morgan, Leon - Research Associate
Osborne, Darrell W. - Research Associate
Peterson, Sigfred - Research Associate
Robinson, Herman - Research Associate
Simpson, Oliver S. - Research Associate
Studier, Martin [SED] - Research Associate
Thompson, Roy C. - Research Associate
Thompson, Stanley G. - Research Associate
Westrum, Edgar F. - Research Associate
Florin, Alan E. - Research Assistant
Hausman, Eugene [SED] - Research Assistant (half-time)
Hopkins, Horace H. [SED] - Research Assistant
O'Connor, Paul - Research Assistant
Scott, Benjamin - Research Assistant

Van Winkle, Quentin - Research Assistant
Walsh, Patricia - Research Assistant
Erway, Norman - Technician
Thomson, Helen - Technician

Group 2, Control Analysis,
Kohman, T. P. - Group Leader
Anderson, Herbert H. [SED] - Research Associate
Ames, Donald P. [SED] - Research Assistant
Fineman, Phillip [SED] - Research Assistant
Sedlet, Jacob - Research Assistant
Weissbourd, Bernard [SED] - Research Assistant
Calhoun, Opaline - Technician

Group 3, Recovery,
Stewart, Donald C. [SED] - Research Assistant
Asprey, Larned B. [SED] - Research Assistant
Britain, J. W. [SED] - Research Assistant

Group 4, Solvent Extraction,
Lawroski, Stephen - Group Leader
Gilbreath, James R. - Assistant Group Leader
Blaedel, Walter J. - Research Associate
Hagemann, French T. - Research Associate
Hyman, Herbert H. - Research Associate
Schaffner, Irwin J. - Research Associate
Ader, Milton [SED] - Research Assistant
Gaarder, Sydney - Research Assistant
Goeckermann, Robert - Research Assistant
Hausman, Eugene A. [SED] - Research Assistant (half-time)
Kelley, Alec [SED] - Research Assistant
Post, Roy [SED] - Research Assistant
Schraidt, John H. [SED] - Research Assistant
Boykin, Pearline - Technician
Giacchetti, Olga - Technician

According to this morning's paper the meeting of foreign ministers in London is drawing to a close. Another meeting is scheduled for two months from now; meanwhile their deputies will carry on the work.

Tuesday, October 2, 1945

At 8:28 a.m. I held a meeting in the New Chemistry conference room of Groups 1 and 3 of Section C-I. In attendance were Asprey, Britain, Cunningham, Florin, Ghiorso, Hindman, Hopkins, Hyde, Jaffey, James, Katzin, Kohman, Magnusson, O'Connor, Osborne, Peterson, Robinson, Scott, Simpson, Stewart, Studier, R. Thompson, S. G. Thompson, and Westrum. I opened the meeting by asking Britain and Ghiorso about the CW-3 samples. Britain has separated and decontaminated 150 mg of 25NB

(Hanford-irradiated U^{235}). He has found a hard component that has not decayed over the last four days and was not observed in 25NA (Hanford-irradiated U^{235} from CW-2). I mentioned I have agreed to send Allison 50 mg of U_3O_8 from 25NB. Manning said he has recalculated the cross section of $U^{236}(n,\gamma)U^{237}$ from the 93^{237} and 94^{238} yields in the 25NB sample; he obtains the value of 16 barns. I suggested four other calculations worth making on 25NA and 25NB to finish the job and then asked for the latest information on 49NH (Hanford-irradiated plutonium from CW-3). Ghiorso gave the ratio of the alpha-particles found for 95^{241}, 96^{242}, 94^{239}, 94^{240}.

$$\frac{\text{4.1 cm alpha particles}}{\text{3.7 cm alpha particles}} = 0.54 \text{ percent}$$

$$\frac{\text{4.75 cm alpha particles}}{\text{3.7 cm alpha particles}} = 3.26 \text{ percent}$$

Isotope	Approx. Range (cm)
95^{241}	4.1
96^{242}	4.75
94^{239}	3.7
94^{240}	3.7

Florin indicated they could make the fluoride for the metal for Site Y out of 49NH tomorrow (he has temporarily abandoned the use of freon for fluorinating). I told him to use about 35 mg of plutonium, and I then summarized what has to be sent to Site Y:

50 mg of U_3O_8 from 25NB

10 mg plutonium solution from 49NH

8-10 mg plutonium metal pellet from 49NH (for mass spectrograph)

Ghiorso will measure the fission cross section of 94^{240} as soon as Westrum is finished preparing the samples.

I asked Morgan to look at 49NG to find the beta component that Paul found in 49NH--perhaps this will tell us whether a cube or a fourth power relationship is the most likely, in order to determine which 94 isotope corresponds to this activity.

Stewart reported that Asprey is working on the milking of 95^{241} from the 10 g of 250 gt material.

Stan Thompson said that the 40 c/m of alpha activity isolated from the cerium fraction from 49NH may simply be 94^{239} (he is looking for element 97).

I asked about the neptunium work and Magnusson indicated that TTA seems effective in separating 93(IV) from 94(III).

Van Winkle reported that all the extraction of protactinium should be finished by Saturday. There are 24 mg in the main line.

Peterson is arranging for the bombardment of 1 mg of radium at Clinton in order to produce some actinium.

Simpson intends to test the fractionation (by volatilization) of 94 and 95 using 200-500 micrograms of 49NG metal from Westrum that has been growing 95 for 2½ months.

Cunningham reported on the solubility of 95 hydroxide. There was some discussion about the future program of separations.

Hogness prepared a summary of the manpower distribution in the Chemistry Division as of October 1, which shows the following for Section C-I:

	Seaborg, Heavy Isotopes	24
Section C-I	Kohman, Control Analysis	6
Seaborg	Stewart, Services	3
	Lawroski, Solvent Extraction	12
	Administration - Seaborg, Manning, Perlman	3
	Total	48

I also read a chart issued by Tregillus of the academic organization of Argonne Laboratory as of October 1:

Division of Argonne Laboratory

Enrico Fermi, Laboratory Director, Chief Physicist
Walter H. Zinn, Acting Laboratory Director, Principal Physicist
Jane H. Hall, Assistant to Laboratory Director, Associate Physicist

Group 1

Group 2
Langsdorf, A. - Group Leader, Physicist
Arnold, W. - Junior Physicist
Kubitschek, H. - Junior Physicist
Purbrick, R. - Junior Physicist

Group 3
Lichtenberger, H. - Group Leader, Junior Physicist
Best, R. [SED] - Research Assistant
Lawhead, L. [SED] - Research Assistant
Monk, G. - Junior Physicist
Nobles, R. - Research Assistant
Rylander, E. - Research Assistant

Group 4
Wattenberg, A. - Group Leader, Associate Physicist
Fields, R. [SED] - Junior Physicist
Russell, B. - Junior Physicist

Group 5
Brill, Thomas - Group Leader, Associate Physicist
Thomas, G. - Research Assistant

Group 6

Group 7
Hill, D. L. - Group Leader, Associate Physicist
Kanner, H. [SED] - Research Assistant

Group 8
Hughes, D. J. - Group Leader, Senior Physicist
Cahn, A. - Junior Physicist
Eggler, C. - Junior Physicist
Goldstein, N. - Research Assistant
Hall, D. - Associate Physicist
Spatz, W. - Associate Physicist

Group 9
Voskuyl, R. - Group Leader, Consultant
Fischer, R. - Junior Chemist
Potter, R. [SED] - Laboratory Technician

Group 10
Maurer, R. - Group Leader, Physicist
Sturm, W. [SED] - Junior Physicist
Turkel, S. [SED] - Research Assistant

Group 11
Dancoff, S. - Group Leader, Senior Physicist
Young, G. - Group Leader, Senior Physicist
Friedman, F. - Physicist
Goldberger, M. [SED] - Junior Physicist
Redman, C. [SED] - Junior Physicist
Way, K. - Physicist
Wilkins, E. J. - Associate Physicist

Technical, Non-Academic Personnel under W. H. Zinn
Boula, H. - Secretary B
Brown, E. - Laboratory Assistant B
Lichtenberger, A. - Clerk B (librarian)
Miller, M. - Draftsman C
Ostapowicz, J. - Glassblower A
Riddick, J. - Draftsman A
Rottman, R. - Technician B
Wimunc, E. - Technician C

I sent Colonel Metcalf a memo saying that I read cases S-52 and S-61 and, with minor corrections, am ready to sign them. I go on to refer to the problem to be solved about the inventorship on the various cases and say that I believe this should be settled first before actually signing. I suggest that a conference involving the four inventors and a representative of his office be arranged in order to solve this problem.

In a letter to Friedlander at Site Y I ask whether he would be interested in a position in connection with our nuclear chemistry research program planned for the Department of Chemistry and the Radiation Laboratory at Berkeley.

I called Joe Hamilton in Berkeley. He told me that the target of U^{233} bombarded with helium ions is not ready but should be ready for shipment by air about October 10. The U^{233} target will be back-stopped by a target of impure thorium. Hamilton mentioned that he will be in Chicago a week from next Monday, that is, about October 15. I then asked Hamilton to tell Lawrence that Cape has called to inquire whether we might have any possible interest in the thorium ore from Monterey Bay.

I replied to Dean Lind's letter of September 18, saying that his letter has been forwarded to me and that I am neither in New Mexico nor Berkeley. In answer to his questions about Wahl and Duffield, I say that both of them probably rank among the top half dozen nuclear scientists in the country, at their age level. Wahl has probably had better training in nuclear chemistry while Duffield's recent work has tended to be more toward chemical engineering.

I also suggest H. S. Brown, who has an excellent personality and probably the best theoretical knowledge in nuclear physics of any nuclear chemists of his age level. R. W. Stoughton is another name I mention.

I go on to say that some very good predoctorate men will be available after they obtain their doctorates in a couple of years.

The foreign ministers are still meeting; after three weeks they are still unable to agree on the pattern for European peace treaties, which is the main purpose of the conference.

<u>Wednesday, October 3, 1945</u>

President Truman sent the following message to the Senate today:

> Almost two months have passed since the atomic bomb was used against Japan. That bomb did not win the war, but it certainly shortened the war. We know that it saved lives of untold thousands of American and Allied soldiers who would otherwise have been killed in battle.
>
> The discovery of the means of releasing the atomic energy began a new era in the history of civilization. The scientific and industrial knowledge on which this discovery rests does not relate merely to another weapon. It may some day prove to be more revolutionary in the development of human society than the invention of the wheel, use of metals, or the steam or internal combustion engine.

Never in history has society been confronted with a power so full of potential danger and at the same time so full of promise for the peace of the world. I think I express the faith of the American people when I say that we can use the knowledge we have won, not for the devastation of war, but for the future welfare of humanity.

To accomplish that objective we must proceed along two fronts--the domestic and the international.

The first and most urgent step is the determination of our domestic policy for the control, use, and development of atomic energy within the United States.

We cannot postpone decisions in this field. The enormous investment which we made to produce the bomb has given us the two vast industrial plants in Washington and Tennessee, and the many associated works throughout the country. It has brought together a vast organization of scientists, executives, industrial engineers, and skilled workers--a national asset of inestimable value.

The powers which the Congress wisely gave to the Government to wage war were adequate to permit the creation and development of this enterprise as a war project. Now that our enemies have surrendered, we should take immediate action to provide for the future use of this huge investment in brains and plant. I am informed that many of the poeple on whom depend the continued successful operation of the plants and the further development of atomic knowledge, are getting ready to return to their normal pursuits. In many cases these people are considering leaving the project largely because of uncertainty concerning future national policy in this field. Prompt action to establish national policy will go a long way toward keeping a strong organization intact.

It is equally necessary to direct future research and to establish control of the basic raw materials essential to the development of this power whether it is to be used for purposes of peace or war. Atomic force in ignorant or evil hands could inflict untold disaster upon the Nation and the world. Society cannot hope even to protect itself--much less to realize the benefits of the discovery--unless prompt action is taken to guard against the hazards of misuse.

I therefore urge, as a first measure in a program of utilizing our knowledge for the benefit of society, that the Congress enact legislation to fix a policy with respect to our existing plants, and to control all sources of atomic energy and all activities connected with its development and use in the United States.

The legislation should give jurisdiction for these purposes to an Atomic Energy Commission with members appointed by the President with the advice and consent of the Senate.

The Congress should lay down the basic principles for all the activities of the Commission, the objectives of which should be the promotion of the national welfare, securing the national defense, safeguarding world peace, and the acquisition of further knowledge concerning atomic energy.

The people of the United States know that the overwhelming power we have developed in this war is due in large measure to American science and American industry consisting of management and labor. We believe that our science and industry owe their strength to the spirit of free inquiry and the spirit of free enterprise that characterize our country. The Commission, therefore, in carrying out its functions should interfere as little as possible with private research and private enterprise, and should use, as much as possible, existing institutions and agencies. The observance of this policy is our best guaranty of maintaining the pre-eminence in science and industry upon which our national well-being depends.

All land and mineral deposits owned by the United States which constitute sources of atomic energy, and all stock piles of materials from which such energy may be derived, and all plants or other property of the United States connected with its development and use should be transferred to the supervision and control of the Commission.

The Commission should be authorized to acquire at a fair price, by purchase or by condemnation, any minerals or other materials, which are not already owned by the United States.

The power to purchase should include real and personal property outside the limits of the United States.

The Commission should also be authorized to conduct all necessary research, experimentation, and operations for the further development and use of atomic energy for military, industrial, scientific, or medical purposes. In these activities it should, of course, use existing private and public institutions and agencies to the fullest practicable extent.

Under appropriate safeguards, the Commission should also be permitted to license any property available to the Commission for research, development, and exploitation in the field of atomic energy. Among other things such licensing should be conditioned, of course, upon a policy of widespread distribution of peacetime products on equitable terms which will prevent monopoly.

In order to establish effective control and security, it should be declared unlawful to produce or use the substances comprising the sources of atomic energy or to import or export them except under conditions prescribed by the Commission.

Finally, the Commission should be authorized to establish security regulations governing the handling of all information, material, and equipment under its jurisdiction. Suitable penalties should be prescribed for violating the security regulations of the Commission or any of the other terms of the act.

The measures which I have suggested may seem drastic and far-reaching. But the discovery with which we are dealing involves forces of nature too dangerous to fit into any of our usual concepts.

The other phase of the problem is the question of the international control and development of this newly discovered energy.

In international relations as in domestic affairs, the release of atomic energy constitutes a new force too revolutionary to consider in the framework of old ideas. We can no longer rely on the slow progress of time to develop a program of control among nations. Civilization demands that we shall reach at the earliest possible date a satisfactory arrangement for the control of this discovery in order that it may become a powerful and forceful influence toward the maintenance of world peace instead of an instrument of destruction.

Scientific opinion appears to be practically unanimous that the essential theoretical knowledge upon which the discovery is based is already widely known. There is also substantial agreement that foreign research can come abreast of our present theoretical knowledge in time.

The hope of civilization lies in international arrangements looking, if possible, to the renunciation of the use and development of the atomic bomb, and directing and encouraging the use of atomic energy and all future scientific information toward peaceful and humanitarian ends. The difficulties in working out such arrangements are great. The alternative to overcoming these difficulties, however, may be a desperate armament race which might well end in disaster. Discussion of the international problem cannot be safely delayed until the United Nations organization is functioning and in a position adequately to deal with it.

I therefore propose to initiate discussion, first with our associates in this discovery, Great Britain and Canada, and then with other nations, in an effort to effect agreement on the conditions under which cooperation might replace rivalry in the field of atomic power.

I desire to emphasize that these discussions will not be concerned with disclosures relating to the manufacturing process leading to the production of the atomic bomb itself. They will constitute an effort to work arrangements covering the terms under which international collaboration and exchange of scientific information might safely proceed.

The outcome of the discussions will be reported to the Congress as soon as possible and any resulting agreements requiring congressional action will be submitted to the Congress.

But regardless of the course of discussion in the international field, I believe it is essential that legislation along the lines I have indicated be adopted as promptly as possible to insure the necessary research in, and development and control of, the production and use of atomic energy.

Manfred Lindner, in a letter dated October 1, wrote from Site W asking whether I plan to return to academic work and inquiring about the possibility of doing his graduate work under my direction. He also inquired about scholarships, fellowships, or assistantships. Lindner also sent his regards to Iz Perlman, John Willard, and to Truman Kohman on his impending marriage.

I wrote to Jerry Howland. I say that, reading between the lines of his September 13 letter, I have the impression he and some of the others may be wondering whether I can make any definite suggestions as to possibilities for future positions. I indicate I have nothing to offer at the present time but am keeping such matters in mind in connection with the various places which are beginning to formulate plans for work in the nuclear field. I say most places cannot proceed very far without governmental action. I mention that I have given his name to the Bell Telephone Laboratories.

I tell him of my success in obtaining tickets for the World Series and that I have been able to play golf every Sunday, even though it has been raining here almost continuously for the last few weeks.

After work I played 13 holes of golf at Evergreen with Iz Perlman, Stan Thompson, and Leonard Katzin. (GS and LK won a 13-hole "low ball plus low total" match, 1 up. IP-63, ST-51, GS-55, LK-68 for nine. IP-89, ST-75, GS-75, LK-91 for 13.)

"Parley Folds Up; Failure" reads today's top headline. Secretary of State Byrnes, in polite language, put the blame at the doorstep of the Soviet Union. The Soviet Union wants the treaties drafted by the Soviet Union, Britain, and the U.S., excluding France and China, except that France would participate on the Italian treaty. Britain and the U.S. remain firm about having France and China at the peace table.

The Cubs won the opening game of the World Series today 9-0 with Borowy pitching for Chicago; the game was in Detroit.

Thursday, October 4, 1945

I answered Dreher's recent letter pointing out that no one in the country, not even President Truman, knows the answers to the questions he asked me about the future plans for Hanford and the general prospects for peacetime applications of atomic energy. I offer (1) the guess that Hanford will operate at least five years and possibly much longer, (2) there is not much likelihood there will be production of other fissionable isotopes at Hanford on a production scale, (3) I do not know whether du Pont will continue to be the operator at Hanford. I indicate that, among the choices he has enumerated, I think his best choices are to stay with Hanford or go with California Research Corporation. I then tell him of my decision to return to Berkeley and that should the program there encompass development work I will certainly ask him to give us consideration.

Westrum sent some comments to Brewer in Berkeley on heats of solution measurements of $PuCl_3$.

Today's paper carries an item that the U.S. has been recommended to be the seat of the new international organization by the United Nations preparatory executive committee.

The Tigers won the second game of the World Series 4-1 in Detroit today, tying the series 1-1.

Friday, October 5, 1945

I read a notice from W. M. Branch, announcing that on October 15, Claude A. Young will assume duties of Director of Security and Safety Division. He succeeds John O. Pyle.

At 4:00 p.m. in Room 209, Eckhart Hall, I attended a meeting of Section Chiefs and higher officials, called by Daniels to formulate a Laboratory Council to help him with policy matters formerly handled by the discontinued Project Office. Others present were Branch, Cole, Dancoff, Daniels, Dempster, Hilberry, Hogness, Hughes, Jesse, Lapp, Mulliken, Nickson, Willard, Zachariasen, Zinn, and Zirkle. Those present, plus Foote, Jacobson, and Stone, were appointed to the Council. Meetings were scheduled for the first and third Mondays of each month at 9:00 a.m. in Room 209, Eckhart Hall.

Subjects discussed at this first meeting were (1) the understanding between General Groves and the University that the Laboratory will continue on its present basis until July 1, 1946, (2) the plans for the ceremony to be held in Mandel Hall on October 10 when Chancellor Hutchins will receive a scroll from Groves honoring the University for its services to the Project, (3) the decision to delay action on the evacuation of Eckhart Hall, (4) the plans for the meeting on new piles on October 16 and 17 in Chicago, (5) a proposal for a 35-hour work week and 4-6 weeks vacation for those working with radiation, (6) the announcement that administrative procedures will be streamlined, (7) a report that Colonel Peterson is now responsible for the Metallurgical Project Record and a suggestion that since the cloak of security has been lifted, the name be changed to "Plutonium Project Record," (8) the date of the next Laboratory Council Meeting will be October 15.

Passeau pitched a one-hit shutout for the Cubs, who won 3-0 at Detroit today to stun Detroit and lead the series 2-1.

Saturday, October 6, 1945

I wrote to Paneth in Montreal, about the naming of the rare or missing elements. I indicate we are now in agreement with the term "neptunium series" for the 4n + 1 series, and that individual members of the decay chain should have the common prefix "neptuno."

I mention my discussion with Segrè about the renaming of elements 43, 61, 85, and 87 and suggest that, eventually, a "letter to the editor" by a disinterested person like himself could propose that Mlle. Perey suggest a name for element 87; Corson, MacKenzie, and Segrè should suggest a name for element 85; Perrier and Segrè should suggest a name for element 43. I say that Segrè is not clear as to who should name element 61 since the earliest work involved himself and co-workers and the Ohio State group. [For security reasons I am unable to mention to Paneth the work of Coryell's group.] I then ask Paneth's reaction to the name Emanation, symbol Em, for element 86.

The Tigers won the game in the World Series today, defeating the Cubs and tying the series 2-2.

Sunday, October 7, 1945

Perlman and I played 16 holes of golf at Jackson Park. (For nine, IP-51, GS-47; for 16, IP-91, GS-87.)

The Tigers won again today at Wrigley Field. We were at the game and saw the loss, leaving the series 3-2 in favor of the Tigers.

Monday, October 8, 1945

An October 1 letter arrived from Lavender in Washington about agreement W-28-094 eng-30. He indicates Colonel Nichols executed the original agreement on behalf of the Government and all copies are being held by Lavender pending advice from the Regents of the University with respect to disclaimer. He encloses a supplement for me and the other inventors to execute, covering a "covenant against contingent fees" and an "officials not to benefit" provision. He also sends a voucher, Form 1034, for execution by the inventors. Lavender indicates that Case 52 would be filed after we have completed our examination of it, even if the University of California has not executed the certificate of disclaimer.

I mailed, after signing them, the items mentioned in Lavender's October 1 letter to Kennedy at Site Y. I confirm that the present plans call for him, Art, and Emilio to come here for conferences with me and the attorney. I also ask that they find out how far Friedlander can go toward serving as a corroborating witness.

Earl Hyde called me from Voelkel's Glacier Lodge, Estes Park, Colorado, (telephone number 03R5) to give me a contact point in case I want to reach him. He said he will be back in Chicago Friday morning.

In two separate memos Daniels requests that Hilberry appoint Seitz and Urey as consultants to the Laboratory.

Report CS-3237, "Chemistry Division Summary Report for September 1945," was issued today. All Section C-I information in the report appeared in the Perlman-Manning memo of September 19 to Hogness.

Also issued today was the Met Lab Report for September 1945. The summary of research results includes reference to the following accomplishments of Section C-I: (1) Redox solvent extraction process runs have been made with uranium-plutonium mixtures with results meeting all expectation. (2) In the work on heavy isotopes, samples of Th^{232} bombarded with 22 Mev deuterons have been found to contain Pa^{230}, whose decay chain has been identified and constitutes a new 4n + 2 series.

The report shows that academic personnel at the Met Lab and Argonne total 255, including 55 SED men. Total personnel, including administration and security is 1,229.

The Cubs won in an exciting twelve-inning game, 8-7, to tie the series, 3-3.

Tuesday, October 9, 1945

I attended the meeting of Groups 1 and 3 at 8:28 a.m. in the New

Chemistry conference room. Present were Asprey, Britain, Cunningham, Florin, Ghiorso, Hindman, Hopkins, Jaffey, James, Katzin, Magnusson, Manning, Morgan, O'Connor, Osborne, Perlman, Peterson, Scott, Simpson, Stewart, Studier, R. Thompson, S. Thompson, Van Winkle, Weissbourd, and Westrum. I opened the discussion by asking Britain about the progress of the CW-3 sample. He said he is taking decay and absorption curves and has found 16-day and 60-day activities.

I then brought up the status of the plutonium from 49NH, saying that there has been a set-back in preparing the fluoride for metal production. Florin explained the material melted at about 67°C and ran up the sides of the crucible. A discussion followed about the best method to recover the 35 mg of fused plutonium.

Morgan described his absorption curves for plutonium of varying gt levels in the very soft regions; he finds an increase in the very soft component with increasing gt levels that is intermediate between that expected for a first and second order reaction.

James reported he has milked 5 mg of 49NH plutonium for americum. (We have been using the name "americum" for element 95, in recognition of its eka-europium character.) He obtained the anticipitated amount assuming a second power dependence on the neutron bombardment of Pu^{239}.

I summarized the present situation about the americum problem. The amount of alpha activity obtained is always proportional to the second power of neutron irradiation intensity. However, that obtained from high gt material has a shorter half-life; this might indicate the presence of two americum isotopes--both having the same range within limits of resolution. Possibilities are (1) Pu^{239} decays to a small extent by beta emission forming Am^{239} with a half-life of 400-2,000 years, the short half-life isotope being Am^{241} formed by beta decay of Pu^{241}; (2) Am^{241} is the 400-2,000 year half-life isotope and the short-lived isotope is Am^{243} (or perhaps 96^{243} which is formed from it), these being derived from Pu^{243} by beta emission; or (3) and most favored, nuclear isomers for Pu^{241} and Am^{241} exist, with the lower energy isomer of Am^{241} having the longer half-life, with the similarity in ranges explained by the large difference in the spins of the two isomers. What seems more probable is that the half-life for alpha emission of the two isomers would be similar but that the higher energy isomer would be converted to the lower energy isomer with a half-life of three years or less.

Manning reported the results of his calculations made using admittedly somewhat inaccurate data from 25NA and 25NB (Hanford-irradiated U^{235} from CW-2 and CW-3). He obtains a cross section of 6.7 barns for the $U^{238}(n,\gamma)$ reaction.

Studier is continuing with the purification of protactinium from the big thorium plus deuterons bombardment; it is still very hot.

Cunningham reported on an attempt to prepare americum chloride with Fried--there is evidence (the melting point is too low) some oxychloride may be present. The color is greenish-yellow.

Ghiorso suggested we start using the name "americum" regularly for element 95; this met with general approval. I pointed out the suitability of the name in view of the eka-europium character of element 95.

Hopkins reported on his attempt to improve the procedures for decontaminating 95 and 96 through precipitation of cerous fluoride as a scavenger.

Manning announced the completion of the hot lab (Room 12) last week except for installing the centrifuge and piling up the lead.

Hindman reported he has 128 mg of Np^{237} in the main solution. An alpha particle of range between that of Pu^{239} and Np^{237} is present and might be Np^{235} or another range alpha particle from Np^{237}.

Van Winkle said he has precipitated 21 mg of protactinium as the peroxide. There will be a smaller meeting later in the week on protactinium chemistry.

Simpson discussed his results of high temperature distillation of fission products and alpha activity from pile-irradiated uranium. He has obtained three peaks, strontium, an unknown, and cerium.

Peterson reported that Stewart has shipped 1 mg of radium to Clinton for bombardment. When the material comes back, he will do the separation of actinium in Room 36.

Lawroski, Manning, and I sent a memo to Daniels about the future plans for solvent extraction. We point out that if serious consideration is to be given to installation of a full-scale solvent extraction set-up at Hanford to be ready for operation before the end of 1946, decisions must be made immediately in order to permit the work to proceed rapidly toward that goal. Personnel should be increased by four or five men, and design engineers should be available by November 1. A decision must also be made whether a pilot plant should be constructed first. We also ask for a decision as to whether the solvent extraction group should direct some of its effort toward an adaptation of the process to other separations, such as those required for breeder piles. We give some rough cost estimates and manpower requirements for full-scale production units.

Today's paper reports that 400,000 Karelians fled their homeland and property to escape residence under the Soviet Union when their country was ceded to Russia by Finland. They will be paid for their property by the Finnish treasury despite its depletion.

Wednesday, October 10, 1945

General Groves is in town to present a scroll to Chancellor Hutchins in recognition of the valuable services performed by the Metallurgical Laboratory. I attended the ceremony which was held at 2:30 p.m. in Mandel Hall.

Groves also visited the West Stands and inspected the solvent extraction columns. Lawroski later reported to me that the General was very nervous about the plutonium and fission products being pumped through columns made of glass and was reluctant to even look in the mirror set up to view the columns through the labyrinth shielding arrangement.

The Cubs unhappily lost to the Tigers 9-3 today to lose the series.

Thursday, October 11, 1945

The Solvent Extraction Group met at 8:28 a.m. in the New Chemistry conference room. The meeting was attended by Ader, Blaedel, Gaarder, Gilbreath, Goeckermann, Hyman, Lawroski, Manning, Perlman, Post, Schaffner, Schraidt, and me. Blaedel reported that three more first-cycle runs and two second-cycle runs have been completed. In the last run, plutonium losses were 0.5 percent through the second cycle, with decontamination being 4×10^6 for gamma-rays and 4×10^5 for beta particles. A run with Hanford levels of plutonium is underway.

Goeckermann talked about his studies of effect of hot dichromate treatment on decontamination in Column 1A, and on the use of thiocyanate to reduce waste losses in Column 1B (by removing Fe^{+3}).

There was a discussion of ways of improving decontamination by changes in reagents and in column operating conditions.

In a teletype dated October 10, Kennedy agrees with me that a meeting of the four of us on the patent claims is desirable, but he would prefer the meeting be held at Site Y. He then asks about the availability of reprints of my Reviews of Modern Physics "Table of Isotopes" and Chemical Reviews article on "Artificial Radioactivity."

I immediately wired Kennedy that Security wants the meeting to be held here and, if he wants to change it, he should take the initiative. I say I am sending him copies of my Chemical Reviews article and will ask Miss Kittredge to send him reprints of the "Table of Isotopes."

I received a letter dated October 8 from Latimer. He says that he discussed the question of titles with Ernest Lawrence who favors Research Professor for those men to whom we want to give pernanent status without teaching duties. Lawrence believes President Sproul will agree. Latimer suggests I offer Perlman a Research Associate Professorship and Cunningham a Research Assistant Professorship.

Latimer also asks for a statement of the history of U^{233} in order to keep the University informed.

I called Jack Roof at Oregon State University. He told me that he is still an assistant professor at Oregon State University. He returned there about a month ago from Edgewood Arsenal where he worked during the war. We talked about whether he would be interested in a position somewhere in the nuclear field.

The following telegram arrived, addressed to me, from Robert Redfield, Dean, Division of Social Sciences, University of Chicago.

> There is now great danger that ill-considered atomic energy legislation will be rushed through Congress. Important that Congress become familiar with all relevant facts and considerations before holding hearings on specific bills in standing committees. Will you authorize Hutchins in name of Chicago conference to present this opinion to President Truman or other members of national administration and to urge administration support for hearings in both open and executive sessions before some new bi-partisan committee set up for the purpose in House or Senate.

I replied to Redfield indicating I authorize him to proceed as he requests.

Today's paper carries an item on Stalin's health and the struggle for power upon his death. Competitors include Zhukov and Molotov.

Friday, October 12, 1945

Earl Hyde returned to work from a vacation in the Colorado Rockies.

I answered the letter I received from Latimer yesterday, enclosing a copy of a statement on the history of U^{233} which I prepared earlier for another purpose. I tell him the basic patents on the use and on the preparation of U^{233} have been prepared and applied for in the names of Seaborg, Gofman, and Stoughton.

I also inform him that I made the offers to Perlman and Cunningham as he suggested, and that I feel confident they will accept. I add that it looks as if Perlman, Robinson, and Morgan will comprise the first contingent to Berkeley and will probably come out next month if arrangements can be made in time. I plan to come with them on a visit.

I mention I will have the people who are interested in doing graduate work in nuclear chemistry at Berkeley get in touch with him so he can determine whether they meet Department standards for admission.

I tell Latimer of the publicity about the stand that some of the Chicago scientists are taking in connection with the proposed legislation about atomic energy which is under discussion in Washington. I say I am in complete disagreement with this approach to the problem and my advice, so far not heeded, to the men here (mostly men in Eckhart) has been that they should not go so far as to support openly in the newspapers one particular bill as against another. I add that I do believe that the nature of the whole complicated problem is such that the help of the scientists is needed, but that there are other ways in which we can help and that this one is not the proper approach. (I have in mind that it is better to work directly with the Congressmen involved than through the newspapers.)

I also wrote to Lindner at Site W, to tell him I will return to Berkeley, probably early next spring. I say if he wants to do graduate work under my direction, he should get in touch with Latimer to see if he can be admitted to graduate standing on the basis of his previous record in chemistry; if successful, I will be happy to have him work with me as a graduate student. I also mention that the stipend for teaching assistantships is $900 for a nine-month year with some possibility of additional compensation during the summer.

According to this morning's paper the House of Representatives passed a bill to cut taxes for the first time in sixteen years.

<u>Saturday, October 13, 1945</u>

Today I received the following letter dated October 10 from General Groves:

> My dear Dr. Seaborg:
>
> The major final factor which determined the surrender of Japan was the Atomic Bomb. Of course, surrender was an ultimate certainty in any case; yet the war would have continued for weeks and perhaps months longer had it not been for the completion and use of our bomb. That weapon, therefore, helped beyond question in the saving, from further death, destruction and misery, of friend and foe alike.
>
> The contribution which you have made to the development of the atomic bomb and to the consequent attainment of the historic results for which the whole nation will always be grateful is appreciated by the War Department and the Manhattan Engineer District, and by me personally, more than words can convey. The chemical research work and the fundamental studies

of the properties of new materials needed for the project, which you carried out at the Metallurgical Laboratory at Chicago, were essential to our success. Your energy and ingenuity, your scientific skill and judgment, and your self-sacrificing devotion to our cause are beyond praise. I want you to know how I feel about this.

While praying that the forces of nuclear energy, which you helped so signally to develop for use against the enemy, may be wisely controlled in the days to come, for the service of a world at peace, we must realize that no future events can detract from the splendor of the results attained in the immediate past through our ability to make military use of these forces. For your indispensable part in this attainment, on behalf of the War Department as agent for the American people, I thank you.

Hamilton called me from Berkeley to tell me that the target from the bombardment of U^{233} with helium ions will arrive in Chicago on United Airlines flight number 40, at 6:28 a.m. tomorrow. He will remove it from the cyclotron about 11:00 a.m., Berkeley time, and it will leave the airport about 3:30 p.m., Berkeley time. The target had a total of 10 microampere-hours over a period of two or three weeks plus another 20 microampere-hours, five microampere-hours of which were put on during the last 24 hours. He said that the U^{233} will be off the target and possibly dissolved in water.

Joe said that our bombarded thorium target will arrive on Tuesday on the "City of San Francisco." He then mentioned that he will leave tomorrow on United Airlines flight number 12 due in Chicago at 11:40 p.m. and will stay at the Palmer House.

Cunningham, Hindman, Katzin, Manning, Osborne, Perlman, R. C. Thompson, Van Winkle, and I met to outline a broad program of research on the chemistry and the transmutation products of protactinium now possible with the supply of protactinium available.

I suggested that Thompson and Van Winkle work on the solution chemistry of protactinium and that Osborne and possibly Al Florin work on the dry chemistry of protactinium peroxide, and protactinium fluoride. This includes the solubilities in the presence of nitric acid, hydrochloric acid, etc., and will include a study of hydrolysis properties.

There will be an attempt to make protactinium chloride and redistill it in order to determine its melting point first using 0.1 mg and then 1 mg.

Other research will include a determination of the specific activity using protactinium oxide; this will be precise to only about 3 percent because of uncertainty in the formula of protactinium oxide. Research will encompass a search for oxidation states lower than V in hydrochloric acid solution, using a mercury cathode and a spectrophotometer.

I suggested that R. C. Thompson and Osborne work on the transmutation program; Van Winkle will help if he is needed. A total of 6 mg of Pa^{231} will be used for this program with 4 mg devoted to the bombardment with helium ions and 2 mg to the bombardment with deuterons.

Wayne Johnson wrote to J. R. Barbour in the Area Engineer's Office to request that he allow some exceptions to our usual arrangements on the transportation and moving agreement for Isadore Perlman. Johnson explains that Perlman had difficulty getting train reservations for himself and his family and therefore they were forced to stay in a hotel. In addition, living quarters were not available when Perlman arrived in Chicago necessitating a storage charge for some of his furniture in Clinton for three months, instead of 60 days.

This evening in Bond Chapel on the University of Chicago campus, Truman Kohman married Jane Sievers, whom he met in Richland, Washington.

The Allies have confiscated the entire holdings in Germany of the I. G. Farbenindustrie and have outlawed the Nazi party, according to this morning's newspaper.

Sunday, October 14, 1945

The U^{233} plus helium ions Berkeley target arrived in Chicago at about 6:30 a.m. on United Airlines flight 40. The target has had a total of about 30 microampere-hours over a period of several weeks.

Stan and I played 18 holes of golf at Evergreen. (ST-110, GS-101.)

Pierre Laval was executed this morning by a firing squad.

Monday, October 15, 1945

I attended the second meeting of the Laboratory Council at 9:00 a.m. in Room 209, Eckhart Hall, along with Cole, Dancoff, Daniels, Dempster, Foote, Hogness, Hughes, Jacobson, Lapp, Mulliken, Nickson, Stone, Willard, Young, Zachariasen, Zinn, and Zirkle. The meeting opened with a discussion of the need to complete the Plutonium Project Record. Daniels would like to complete the work by March 1, even if research activities have to be curtailed. The 44-hour work week was discussed, and everyone agreed it is highly desirable provided there are no reductions in salary. The request for the 44-hour week will be taken up again with the University. It was announced that Lieutenant Mahoney at the Armory is now handling the censorship of the release of technical information.

The next meeting of the Council will be at 9:00 a.m. on Monday, November 5.

Some of the scientists of our section and I often have lunch at the 1004 Club, which is situated on 55th Street near Ingleside Avenue (northeast corner). One of the favorite topics of conversation on Mondays, particularly among the golfers of the group, is the results of the Professional Golfers Tour. There is always an element of competition in the discussions since some of the men prefer Sam Snead, others are fans of Ben Hogan; I personally admire Byron Nelson. Today we talked about the Seattle Open held at the Broadmoor Golf Club in Seattle this past weekend. Nelson finished first with a fantastic total of 259, Snead finished 17th with a total of 288, and Hogan finished in ninth place with a total of 279.

Perlman and I talked with Hamilton who is in Chicago for meetings. We scheduled bombardments of Np^{237}, Pa^{231}, U^{233}, and plutonium metal--all to be bombarded with both helium ions and deuterons. Hamilton agreed to take a Np^{237} and a Pa^{231} target back with him for bombardment with about 500 microampere-hours of deuterons.

The American army will make a complete investigation of conditions in German prisoner of war camps in France and report to Allied headquarters, according to the paper today.

Tuesday, October 16, 1945

At 8:28 a.m. I held a meeting in the New Chemistry conference room of Groups 1 and 3. Asprey, Britain, Cunningham, Florin, Fried, Ghiorso, Hindman, Hyde, James, Magnusson, Morgan, O'Connor, Osborne, Perlman, Peterson, Robinson, Scott, Simpson, Stewart, Studier, R. Thompson, S. G. Thompson, Van Winkle, Weissbourd, and Westrum attended. I asked that we first talk about the new U^{233} plus helium ions target (15 mg of U^{233} as oxide, containing 5 percent natural uranium). Hyde has separated the material into uranium, neptunium, and plutonium fractions. He and James have been looking at neptunium and plutonium fractions with the pulse analyzer and the G-M counter. After a discussion of the results so far, I said they should (1) follow the decay of all samples, (2) identify the peaks of the pulse analyzer curves, and (3) possibly look at the uranium and protactinium fractions.

Perlman and I described the arrangements we made with Hamilton yesterday for additional bombardments. I said the targets will be handled by the following groups: (1) James, S. Thompson, and Hopkins - the Np^{237} plus deuterons target, (2) R. Thompson, Osborne, and Van Winkle - the protactinium plus deuterons target, (3) Morgan, S. Thompson, and Hopkins - the neptunium plus helium ions target. Perlman gave Hamilton's identification numbers for these bombardments:

12a - protactinium plus helium ions

12b - protactinium plus deuterons

13a - Np^{237} plus helium ions

13b - Np^{237} plus deuterons

8a-2 - U^{233} plus helium ions

8b - U^{233} plus deuterons

14a - Pu^{239} plus helium ions

14b - Pu^{239} plus deuterons

Hamilton is to take back to Berkeley the Np^{237} along with a protactinium target prepared by R. Thompson. The second protactinium target prepared by R. Thompson will be sent out later. Two U^{233} targets of 20 mg each will be prepared by Studier and Hyde. A plutonium metal plate will be prepared when material is available from Westrum. The U^{235} fluoride we have will be ignited to the oxide by Stewart and sent to Hamilton for preparation of the target.

Britain reported he has carried out another decontamination cycle on the uranium fraction from CW-3. I asked him to run further purifications until the sample is completely decontaminated, and then ship 50 mg to Site Y.

Westrum said he has received the plutonium fluoride from 49NH from Florin. I asked him to prepare the metal and ship it to Site Y on Friday along with 10 mg of solution and Britain's 50 mg of uranium.

O'Connor is planning to bombard with neutrons 5 mg of plutonium from 49NH at Argonne on Friday.

I suggested that Katzin, Studier, Hyde, and Hagemann begin thinking about the apparatus and other preparations needed for handling the thorium that will be shipped from Hanford between November 6 and November 13. I then asked about plans for fluoride work. Florin said he would like to try to prepare protactinium fluoride after first making trial runs with tantalum. The program will be to study PuF_6, fluorides of protactinium, and fluorides of neptunium.

Van Winkle has set aside 6 mg of protactinium for bombardments and is purifying 14 mg of protactinium which has 1.5 percent impurities.

Morgan then described his experiment to look for the growth of Pu^{238} in 10^6 c/m of 96^{242} that has decayed for one month. He found only 125 c/m of 94^{238} instead of the 1,000 c/m expected. One possible cause is that Pu^{238} has a longer half-life than 60 years. I observed that it is now quite possible that Pu^{236} is present in the sample that Jaffey is following, meaning that the half-life is longer than he is observing.

Peterson announced that he has sent the sample of radium to Clinton for neutron irradiation. I pointed out that this is a feasible way of making pure actinium. Pile bombardment of ionium-thorium mixtures should be an easy method of preparing pure protactinium.

Simpson reported an experiment with high gt plutonium, in which 95^{241} has been allowed to grow. He noted that 95^{241} tends to concentrate in the cold end of his fractionating column by possibly a factor of 10.

The first session of the two-day meeting on new piles began at 9:00 a.m. in Room 209, Eckhart Hall. I arrived late after the adjournment of our meeting. Others present were Aebersold, Allen, Brown, Burton, Captain Chapman, Cole, Connick, Dancoff, Daniels, Dempster, English, Foote, Franck, F. Friedman, Goldberger, Hogness, Huffman, Hughes, Hutchison, Jesse, W. Johnson, Lapp, Leverett, Lauletta, Manning, Ohlinger, Perlman, Rabinowitch, Rubinson, Soodak, Spedding, Urey, Way, Willard, Wollan, G. Young, Zener, Zinn, and Zirkle. Soodak of Clinton began the meeting with the results of further pile calculations on their pile which is to consist of U^{235} salt dissolved in heavy water, surrounded by a heavy water reflector, by a row of thorium rods, and finally by a cylindrical graphite reflector. Critical experiments are now being planned.

At 2:00 p.m. I was the first speaker at the afternoon session with a discussion of nuclear reactions in high energy piles, and I pointed out that, because of the high flux, second and third order reactions will play a considerable role in the future high energy enriched piles. I showed the most important transformations to be expected for the isotopes U^{238} and U^{235} by the addition of from one to four neutrons. Reactions of U^{233} are similar to those of U^{235} since absorption of two neutrons converts the first isotope into the second one. I mentioned that the beta activity attributed to the isotope 96^{242}, starting with U^{238}, actually has been found to be proportional to the fourth power of the total flux. I said that methods for the chemical separation of elements 95 and 96 have now been developed, despite their great apparent similarity. I then announced we have been using the name americum (symbol Am) for element 95. I also described the nuclear transformations occurring in thorium, and the new Pa^{230}-U^{230} decay chain. In answer to a question by Daniels, I said that if Pu^{240} is not fissionable, then the k value of a pile fueled with Pu^{239} will decrease with time.

Hogness presented the results of work on the Redox solvent extraction process. He stated that a one ton per day plant for Hanford material could be built for $2,000,000 and that a pilot plant can be built for about $100,000.

English spoke on the composition of homogeneous pile solutions at 90°C. $NaHCO_3$ solutions are favored at present.

Burton discussed bubbling problems in homogeneous piles--they want no bubbles. The answer seems to be in the use of catalysts and operation at a higher temperature--80° or 90°C.

Leverett reported on the present state of homogeneous pile technology, saying that the catalyst problem is holding up planning. They have found that sodium silicate suppresses corrosion.

M. D. Peterson then talked about the semiworks experiments on the treatment of pile solutions. They are considering hexone for recovery of the uranium. Stripping with water or evaporation (if a volatile solvent is used) are other possibilities for removal of the uranium from the organic extract. Little work has been done on recovery of U^{233} from thorium.

At 11:00 a.m. F. Friedman of Chicago presented calculations on the high temperature beryllium oxide pile; they find difficult control problems.

Willard discussed the preparation and properties of beryllium oxide bricks--the pile is to be built of hexagonal bricks of BeO stacked into a tight structure. In the center of each brick is a cylindrical hole which will contain a central rod of beryllium oxide mixed with uranium dioxide. The unit is intended to operate at 1,300°C with steam as the coolant.

Ohlinger reviewed the pumping problems in connection with the Clinton high power pile, the contemplated high temperature pile, and in power piles in which liquid metal pumps might be required.

In the concluding talks today Zinn and G. Young discussed control devices in high pressure piles and new types of piles, respectively.

Back in my office I read a letter dated October 11 from George Everson of the Radiation Laboratory at Berkeley. Everson enclosed 20 personnel security questionnaires to be filled out and returned by each of the men I am contemplating bringing with me to Berkeley.

This morning's newspaper says that two groups of Congressmen just home from investigative tours abroad report that the Soviets have been looting in the Balkan countries and are threatening to grab Iran in order to obtain a Persian Gulf port.

Wednesday, October 17, 1945

The thorium plus helium ions target (Berkeley bombardment 10a) arrived.

The morning session of the New Piles Meeting devoted to the Argonne Program was held at 9:00 a.m. in Room 209, Eckhart Hall. Dancoff spoke on the use of sodium as coolant in a fast pile. The problem of power dissipation appears tremendous but not impossible, according to Dancoff. The pile will be equivalent to five Hanford piles in the rate of fission and to eight Hanford piles in the rate of production of new plutonium.

Hughes talked about cross sections for fast neutrons; this is a new method based on the utilization of fission neutrons.

M. L. Goldberger gave some calculations of resonance piles; these are at a less precise stage than those of Soodak for fast piles. For Pu^{239}, the value of eta, which is 2.05 for thermal neutrons, rises to as much as 2.95 for neutrons of several kev energy.

Kennedy teletyped me from Site Y to say that he has been unsuccessful in setting up a conference with me on the patent matter at Site Y. Therefore Wahl, Segrè, and he will be in Chicago for three days starting October 18 if they can arrange suitable transportation. Otherwise, he has informed Lavender the meeting will have to be at Site Y or postponed until after January 1. Kennedy then thanks me for the reprints.

During the afternoon discussions on the Health Program were held: "Plutonium Toxicity" was discussed in two papers from K. S. Cole's section, and papers by E. R. Russell and J. G. Hamilton. At 3:20 p.m. H. J. Curtis, Albert Tannenbaum, J. G. Hamilton, and two speakers from Cole's section spoke on "General Radiobiology." Later speakers were L. H. Hempelman, members of Section H-I (J. G. Allen, S. Schwartz, and E. S. G. Barron), and K. Z. Morgan of Health Physics.

Two hundred Indonesians proclaimed independence and formed themselves into the first Indonesian national assembly, which will serve the country until an election can be held. This appeared in this morning's paper under an October 16 dateline.

> A Policy Meeting was held at 11:30 a.m. in Room 209, Eckhart Hall. Compton was not present because he was attending a meeting of the Scientific Panel in Washington. Hamilton urged the immediate declassification and publication of the research published in the "Health Division." This is needed by researchers outside the D.S.M. projects. Daniels appointed a committee of Hogness (chairman), Dempster, Franck, Mulliken, and Stone to make recommendations.
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> The decision to give first priority to the writing of the Project Record was discussed and approved--the writing in Chemistry should be finished by December 31, and the rest of the writing by March 31, 1946.

Thursday, October 18, 1945

In an official memo to Captain Chapman, Manning lists ten bombardments to be made in the Berkeley cyclotron, noting that we have already discussed the bombardments with Hamilton.

"Vapor Pressure Measurements on Plutonium Metal and Plutonium Compounds," (CN-3223) by T. E. Phipps, R. L. Seifert, and O. C. Simpson was issued today. It is a 145-page report covering work done by Erway, Gilpatrick, Jasaitis, F. D. Johnson, T. E. Phipps, G. W. Sears, R. L. Seifert, and O. C. Simpson. The report concludes with a table of the results obtained with plutonium metal, plutonium trifluoride, plutonium trichloride, and plutonium tribromide.

I received and read a paper prepared by Paul Fields, "The Preparation and Decontamination of Np^{239} in Trace Concentrations." It describes methods for the preparation of pure Np^{239} activity from neutron-irradiated uranium compounds. The methods fall into two classes: co-precipitation methods and solvent extraction methods. This paper is based on Report CN-2689.

Another summary paper, "Solvent Extraction Methods for Purifying Plutonium with Respect to Light Elements," by Brody, Stein, Jensen, Reinhardt, and Orlemann, was received in my office. It is based on reports CK-1072, CK-1169, CK-1221, CK-1372, CK-1512, CK-1586, CK-1702, and CK-2086.

Juan Peron, who was forced out of power in Argentina a short time ago (October 9), returned to power today and formed a new cabinet of his friends.

Friday, October 19, 1945

O'Connor purified 5 mg of the plutonium from 49NH (Hanford-irradiated plutonium from CW-3) and then carried out a one-hour irradiation of the sample in the Argonne pile. The material was purified by ether extraction four hours after bombardment. The beta activity in the plutonium fraction is about the same as for low gt plutonium. In the 95-96 fraction there are 200,000 c/m that was mostly Pu^{239}. On the basis of lack of additional beta activity in the plutonium fraction, the half-life of Pu^{243} should be less than 20-30 minutes or greater than several days.

Kennedy sent me another teletype, dated October 18, stating it now appears the patent meeting will occur either at Site Y or nearby. He asks that Lavender's representative be reminded to bring all of Wahl's notebooks.

Pierce Selwood, in a letter dated October 17, asks whether I have been able to have my talk for the Northwestern University Symposium approved. If so, he would like to have a title in the near future. Selwood mentions that Coryell and Spedding have agreed to speak.

I wrote to George Everson in Berkeley and enclosed personnel security questionnaires for Cunningham, Ghiorso, James, Magnusson, Morgan, O'Connor, Perlman, Robinson, Seaborg, R. Thompson, S. G. Thompson, and Westrum. I indicate there may be additional questionnaires. I explain we should like to have a number of the men move to Berkeley as soon as possible and ask that offers be made promptly to Perlman, Morgan, and Robinson.

In a letter to Cyril Smith at Site Y, I enclose drafts of the four chapters on plutonium metallurgy that we plan to include in Volume 14A of the MPR, "Chemistry and Metallurgy of the Transuranium Elements." I ask for his criticism and comments.

Manning and Perlman sent a jointly-authored memo to Hogness summarizing the work of Section C-I for the period September 15 to October 15, 1945. They report the following: (1) A new protactinium isotope (Pa^{229}) was produced in the deuteron bombardment of Th^{230}. It is an alpha emitter of 4.0 cm range and has a half-life of about 31 hours. (2) Deuteron bombardment of U^{238} demonstrated that the yield of the d,4n reaction falls off extremely rapidly as the deuteron energy falls below 22 Mev; the threshold is about 20 Mev. (3) Further measurements on neptunium produced by deuteron bombardment of U^{235} shows a long-lived x-ray activity that is tentatively attributed to Np^{235}. (4) About 130 mg of Np^{237} have been isolated from material obtained by a special run at Hanford. There has been found, in addition to the Np^{237} alpha particles, another alpha activity of 3.6 cm range that follows neptunium chemistry. Its production must be by a reaction at least two orders higher than that for Np^{237} production. (5) When a sample of plutonium containing about 25 percent Pu^{240} was used, the first indication was obtained that the Pu^{240} alpha particle range appears to be less than that of Pu^{239}. (6) Further measurements have been made on the soft beta-rays attributed to Pu^{241} that were found in pile plutonium and in the plutonium fraction of uranium targets bombarded with helium ions. The energy of the beta-ray is about 20 Mev. (7) Comparison studies have been made of the yields of 95^{241} and 96^{242} produced in pile plutonium receiving two different exposures. The difference in yield of 96^{242} was somewhat less than expected from the high order of reaction necessary for its formation if no corrections are made for the disappearance of intermediate isotopes. Further evidence has been found to justify the assignment of 96^{242} to the 4.75 cm range alpha emitter found as a result of helium ion bombardment of plutonium and in pile material: Pu^{238} was identified in a sample of transplutonium activity that was allowed to decay for four weeks. (9) The neutron capture cross section for U^{236} was calculated to be 15 barns, based on the Np^{237} and Pu^{238} produced in U^{235} bombarded for several months in the Hanford pile. (10) The heats of solution of $PuCl_3$ in solutions of different HCl concentrations have been determined at 25°C and at low plutonium concentrations (about 5 x 10^{-4} M). (11) Ten micrograms of 95^{241} were prepared in pure form and converted to the oxide (black) and the anhydrous trichloride. In solution, element 95 is pink to violet in color. Spectrographic analysis showed 51 new spectral lines for element 95. (12) Studies of the relative volatility of the metals of plutonium and element 95 were made by heating some plutonium metal containing 95. At 1,100°C, the vapor pressure of element 95 appears to be at least 100 times that of plutonium. (13) Volatility of fission product oxide distilled in high vacuum showed that cerium condensed principally at the hot end, strontium at the cold end, and unidentified members in the middle. (14) The Redox solvent extraction process first cycle has been tested successfully at one-twentieth of full Hanford levels of fission products and up to full Hanford level of plutonium. Only two second-cycle runs have been made, but results so far indicate that plutonium losses will be less than 0.1 percent.

This morning's paper has a front page article headed "Secrecy Spurs A-Bomb Race, Scientists Say." Harold Urey and H. J. Curtis objected to secrecy regulations proposed for an atomic energy commission. Oppenheimer testified at a House military committee hearing that he "favored general provisions of an administration bill" for a commission.

> At 9:00 a.m. there was a meeting between the Committee on Release of Scientific Information (appointed by Daniels on October 17) and Captain John King and Mr. C. I. Campbell of the Manhattan District Security Division. The Committee is composed of Hogness (chairman), Mulliken, Stone, and Franck. There was agreement that the details of fission product chemistry should be published, together with tables of isotopes including the newly discovered isotopes, although cross sections should not be published for the fissionable elements. There was also agreement on publication of information on production of artificial isotopes by n,γ reactions and by cyclotron methods.
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> There was a divergence of opinion on the publication of the chemistry of plutonium; Army people believe that such publication would assist any potential producer. It was agreed to consult the chemists concerned with separations processes to see which particular data could be determined to be the bottleneck type and, if feasible, publish everything but these data.

<u>Saturday, October 20, 1945</u>

I wrote to Latimer to tell him that Perlman, Morgan, and Robinson from here, and Werner from Clinton Labs are ready to move as soon as it can be arranged. I mention that I have sent Everson a number of personnel security questionnaires, filled out by the men, and will send more next week. I say, "I presume that Everson will offer them positions in the Radiation Laboratory in the near future and that they will transfer to Berkeley and start work in connection with the Radiation Laboratory in positions of the war-time type, under the assumption that they will take on their other statuses at a later time." I tell Latimer, Perlman has assured me he will accept our offer of a Research Associate Professorship and that Cunningham also says he will accept our offer of a Research Assistant Professorship and that he has already turned down Chicago's offer of Assistant Professor at $5,000. I ask if, in the case of English, it would be satisfactory to transfer first to the Radiation Laboratory staff as in the case of the other men and then shift later to his teaching position. I say that Chicago, Johns Hopkins, and probably Washington University are also interested in English and will probably make good offers to him.

More than 50 people were reported killed and 100 wounded in an uprising in Caracas. The president of Venezuela was ousted, and a seven-man junta was formed to control the nation until a new president can be elected, according to a report in today's paper with a dateline of October 19.

Sunday, October 21, 1945

Perlman and I played 18 holes of golf at the Evergreen Golf Club. (IP-111, GS-97.)

The Venezuela junta announced much opposition has been quieted and they are in good control.

Monday, October 22, 1945

I spent most of the morning preparing and dictating my talk to be given at the symposium on nuclear chemistry at the Fiftieth Anniversary Technical Conference of the Chicago Section of the American Chemical Society at Northwestern University. The title is "The Chemical and Radioactive Properties of the Heavy Elements." The paper will have to be cleared by the Security Office before I can present it.

I received a letter from Ray Stoughton in which he describes a number of samples of ours now in the Clinton pile. Stoughton says the people in charge of the pile want to know what we want done with them.

A carbon of a letter dated October 19, from J. R. Oppenheimer to Mr. Raymond M. Martin of Richmond, California, arrived for me. Oppenheimer suggests that Martin contact me for discussion of methods of prospecting for uranium.

France has voted to support General de Gaulle's proposal for the creation of a fourth republic in the first French general election since 1936, according to this morning's newspaper.

Tuesday, October 23, 1945

At 8:28 a.m. I attended the meeting in the New Chemistry conference room of Groups 1 and 3. Also present were Asprey, Britain, Cunningham, Florin, Ghiorso, Hindman, Hopkins, Jaffey, James, Katzin, Kohman, Magnusson, Manning, O'Connor, Osborne, Perlman, Peterson, Robinson, Scott, Simpson, Stewart, Studier, R. Thompson, S. G. Thompson, and Westrum. I opened the meeting with a discussion of the fact that the Manhattan District Security Office is against release of information on the basic chemistry of plutonium. I asked if there are "key secrets" which could be retained as a compromise. Stan Thompson asked if it might be a better idea to release all the information in the CN reports without editing, adding that such an action should be confusing enough.

I mentioned the letter I received from Ray Stoughton yesterday; we decided to request the plutonium fluoride, the U^{238} oxide, and the thorium carbonate.

Simpson presented his tabulation of the relative half-lives to be expected at different values of angular momentum change in terms of J and the energy of alpha emission. The data were calculated for an isotope of atomic number 95. He also talked about the results of pulse

Alpha Half-Life Ratios

Angular Momentum Change J	4 (Mev)	5 (Mev)	6 (Mev)
0	1.4×10^{14}	9.5×10^{5}	1
2	4.44×10^{14}	3.0×10^{6}	2.97
4	6.07×10^{15}	2.5×10^{7}	24.2
6	7.87×10^{16}	4.53×10^{8}	421

analysis of various fractions from his high temperature distillation of plutonium metal containing 95^{241}; this gave good separation. He would like to try separating a mixture of 95^{241} and 96^{242} next if material will be available. Manning raised the possibility of using Simpson's distillation method for element 95-plutonium mixtures on a large scale, i.e., 100 grams.

Westrum indicated he has spent most of his time making samples of metal--171 gt with 90 percent yield, 240 gt with 94 percent yield, and 49NG with a 96 percent yield. With regard to our plutonium metal cyclotron targets, he mentioned that the people at Site Y are willing to mount the foils by soldering them directly to the target plates.

I asked about the fluorination experiments, and Florin described his preliminary work with columbium, tantalum, and protactinium, using Freon.

Ghiorso reported the latest results on the pulse analyzer. He has found what may be a hump on the low energy side of the 96^{242} peak in a thin sample of a 95^{241}-96^{242} mixture prepared by Thompson and Hopkins. I speculated that the activity may be 96^{243}, which could have a half-life of several years, but Ghiorso cautioned the peak may not be real.

We talked about our plans for the fission measurements on the plutonium from 49NH (Hanford-irradiated plutonium from CW-3); the measurements will be carried out as soon as Ghiorso arranges for use of the Argonne pile.

O'Connor described the results of his neutron bombardment at Argonne of plutonium from 49NH. He has not yet run a pulse analysis on the purified 95-96 fraction. I mentioned Manning and I have calculated that if the half-life of 96^{243} is two or three years, it would be very hard to see it in this bombardment--we might have to bombard it for several days.

Stan Thompson reported on a practice run on his fluosilicate method for separating 95 and 96 from rare earth elements that was only partially successful; they were able to get a 25 percent yield of 95 and 96 with a decontamination factor of 10^5 and a 1 percent yield with a decontamination factor of 10^8, using cerous hydroxide-zirconium phosphate precipitations, followed by fluosilicate cycles with cerous fluoride by-product precipitations. The fluosilicate method is erratic for unknown reasons. They may have to go ahead with the large-scale operation, starting with the salt layer from the solvent extraction of 49NH. In the meantime, they are trying selective elution with fluosilicate from IR-1 resin. Thompson then reported nothing very promising has shown up in his search (based on the assumption that it follows cerium chemistry) for element 97.

Morgan gave the results of calculations based on the low energy particles attributed to 94^{241} that indicate the neutron capture cross section of 94^{241} is probably relatively high--10^3 or 10^4 barns.

James reported that element 95 removed from 2 gt material gave 8,000 c/m. He would have expected several hundred thousand c/m if 95 rose from beta branching of Pu^{239} and around 4,000 c/m if the activity were 95^{241}. He is going to carry out decay measurements.

Van Winkle said that he has submitted a sample of protactinium for spectrographic analyses and is proceeding with the determination of the specific activity.

Magnusson has made some progress on the rapid separation of neptunium and plutonium by solvent extraction, but he is not yet completely confident of the method.

Britain has re-extracted the uranium fraction from 25NG and obtained the same absorption curve (which resembles zirconium) for beta activity as before extraction.

Peterson reported that the IR-1 column method for the separation of radium and actinium gives a factor of 10^5 for radium separation.

I wrote a note to Pierce Selwood to inform him that I submitted the manuscript, title, and abstract of the talk I want to give at Northwestern University to the Security Office today. I will let him know immediately when I receive the necessary information--possibly within a week.

Daniels sent Captain McKinley a memo dated October 22 (with a copy to me) stating that, upon my request, he is arranging for the addition of Willard F. Libby (new member of the Institute of Nuclear Studies, University of Chicago) as a consultant to the Met Lab.

I sent a proposal to Daniels that the 30-year alpha-emitting U^{232} would be admirably suited for use in connection with the trigger mechanism of the bomb, in place of the short-lived (140 day) polonium. I discuss the feasibility of its production by neutron bombardment of Pa^{231} that could be obtained as a by-product by extraction and recovery from the uranium during the processing of uranium ores. I say that Pa^{231} also could be produced by neutron bombardment of Io^{230}, also found in uranium ores, but in an abundance 60 times that of Pa^{231}.

Finally I point out that the production of large amounts of Np^{237} would permit the production of another useful alpha emitter, Pu^{238}, through neutron bombardment of the Np^{237}, and that, in the future high energy piles, the transplutonium isotopes, especially isotopes of element 96, will be produced in such amounts that they might be used for this and other purposes.

At 1:01 p.m. I left Chicago on the Santa Fe Chief for Santa Fe, New Mexico, where Kennedy, Segrè, Wahl, and I will meet to discuss patent matters.

Dr. W. F. Ogburn and Professor Sewell L. Avery of the University of Chicago have suggested a long-range inquiry into the social consequences of nuclear energy and the "atomic bomb," according to this morning's paper.

Wednesday, October 24, 1945

I arrived in Lamy, New Mexico, at 2:00 p.m., where I was met by Kennedy, Wahl, and Segrè. We were joined by Captain Scott and Commander Johnson and drove to Bishop's Lodge, a motel about four miles north of Santa Fe, where we immediately started working on patent cases S-61 and S-52.

I spent the night at Bishop's Lodge after writing Helen a postcard.

The Senate has voted a cut in taxes, as did the House.

Thursday, October 25, 1945

Kennedy, Wahl, Segrè, Captain Scott, Commander Johnson, and I worked all day at Bishop's Lodge on the patent cases.

At dinner we were joined by the Garners and the Friedlanders. I again spent the night at Bishop's Lodge.

It was announced in Stockholm today that Dr. Alexander Fleming, discoverer of penicillin, will receive the Nobel Prize with two of his colleages.

Friday, October 26, 1945

Kennedy and Wahl worked with me on the patent cases all day at Bishop's Lodge.

The Segrès, Mary Wahl, Adrienne Kennedy, and Ralph Carlisle Smith drove down from Los Alamos for a dinner party at the Lodge. I again stayed at Bishop's Lodge.

Dr. Robert Ley, tagged No. 4 Nazi, hanged himself while awaiting trial for war crimes.

Saturday, October 27, 1945

At 9:15 a.m. I left Santa Fe by bus for Lamy where I boarded the Santa Fe Chief for Chicago.

Sunday, October 28, 1945

In a special stop I got off the Santa Fe Chief at Joliet, Illinois, at about 12:30 p.m. Dorothy and Winston Manning and Helen met me at the railroad station, and we had lunch together. Manning and I then played 18 holes of golf at Cog Hill No. 2 (WM-126, GTS-105) while Dorothy and Helen talked. It was a lovely Indian summer day reaching 75°F--the warmest October 28 in this area in history. We went back to central standard time and off wartime daylight saving time.

At 5:00 p.m. Perlman left with Hilberry for Site W. He will return November 3.

The Np^{237} plus deuterons target arrived from Berkeley (Berkeley 13b).

On October 27 and 28 a conference was held at Rye, New York, to organize opposition to the May-Johnson Bill for the control of atomic energy. Attendees were Frank Altschul, Stamford, Connecticut; Ben M. Cherrington, Chancellor, University of Denver; Norman Cousins, Editor, The Saturday Review of Literature; R. G. Gustavson, Vice President and Dean of Faculties, University of Chicago; R. M. Hutchins, Chancellor, University of Chicago; C. D. Jackson, New York City; John K. Jessup, New York City; Warren C. Johnson, Chairman, Department of Chemistry, University of Chicago and formerly of Oak Ridge, Tennessee; Irving Langmuir, Associate Director, Research Laboratory, General Electric Company, Schenectady, New York; Edward H. Levi, Professor of Law, University of Chicago; Jacob Marschak, Professor of Economics, University of Chicago; Edgar Ansel Mowrer, Washington, D.C.; Robert Redfield, Dean, Division of Social Sciences, University of Chicago; Theodore W. Schultz, Professor of Agricultural Economics, University of Chicago; John A. Simpson, Jr., Chairman, Executive Committee, Atomic Scientists of Chicago; John T. Tate, Professor of Physics, University of Minnesota; Glen H. Taylor, United States Senator from Idaho, Washington, D.C.; Harold C. Urey, Professor of Chemistry, University of Chicago; Eugene P. Wigner, Professor of Mathematical Physics, Princeton University.

The Conference adopted a resolution that the bill should be withdrawn on the basis that it does not protect the vital interest of the United States.

The group agreed that the amendments have not corrected the essential defects of the bill. They retain the objectionable features of the original bill. The bill, if enacted, would (1) weaken the national defense; (2) jeopardize the democratic institutions of the American people; (3) frustrate fundamental research necessary to the maintenance of American leadership in the development of atomic energy, (4) set up an authority within the United States responsible to no one--not even to the President or the Congress, (5) take atomic energy away from the American people, where the President says it belongs, with the danger that it may become the exclusive property of the military, (6) make it more difficult, perhaps impossible, to stop the present drift towards an atomic armaments race; (7) block the normal negotiation of international agreements in the field of atomic energy, and interfere with attempts to set up vitally necessary international controls.

The resolution states: In our judgment, any legislation in this field should, as a minimum, provide the following:

First, the Commission and its administrator should be responsible to the President and to Congress. They should be removable by the President in the same manner as Cabinet officers.

Second, any secrecy regulations should be applicable only to the disclosure of the design features and laboratory tests pertaining to the plant and to details of the atomic bomb itself; that is, only to such matters as are normally kept secret when the manufacture of munitions or weapons of war is involved. We note that the President of the United States had distinguished between the scientific knowledge and the design features, tests, plants, and details of the bomb itself. Other regulations should be limited to removing hazards to safety.

Third, prior to promulgation, all secrecy regulations should be reviewed by a special Cabinet committee to make sure that the public interest in full disclosure is protected whenever possible.

Fourth, to protect individual research, the Commission should be required to provide sufficient materials, under proper safeguards, to make independent research possible. It should be made perfectly clear that the Commission has no power to control research.

Fifth, the Commission should be required, after an appropriate interval, to recommend to the Congress a program setting forth the standards which should apply to the government regulation of the industrial use of atomic energy. Until such a program has been recommended and enacted by the Congress, the Commission should not have any power to permit the use of atomic power for industrial purposes. Such a program should deal specifically with such problems as patents, exclusive rights, and those steps to be taken to encourage private enterprise to assist in this development. Research and experiments looking toward the industrial use of atomic power should be permitted prior to the submission of the industrial program.

Sixth, we believe that the free exchange of scientific information with the United Nations as proposed by the President is of the utmost importance, and is not entirely separate from the development of a domestic program. Atomic power has been made possible only by the work of scientists all over the world. Many of the most eminent of these scientists are not in the United States and are citizens of foreign countries. Legislation must be framed by the Congress so as to take advantage of such international agreements as may be achieved, and to permit the free exchange of scientific information, not only within our own country, but with foreign scientists.

Monday, October 29, 1945

Last Wednesday (October 24) Manning answered the letter I received last Monday from Stoughton. Manning suggested, at Stoughton's convenience, that the following items be removed from the Clinton pile and sent to us: (1) 46 mg product fluoride, (2) 5 mg 23 oxide plus 5 g natural uranium as oxide, and (3) 50 g $ThOCO_3$.

Last Thursday (October 25) there was a meeting at 8:28 a.m. in the New Chemistry conference room of the Solvent Extraction Group. It was attended by Ader, Blaedel, Gaarder, Gilbreath, Hagemann, Hyman, Lawroski, Manning, Perlman, Post, Schaffner, and Schraidt. Blaedel summarized the results of two first-cycle runs, one with uranium and plutonium and one with uranium, plutonium, and fission elements. Very poor decontamination occurred in the second run due to poor hexone obtained from Carbide and Carbon. The Shell hexone is a much purer product and will be now used exclusively. Furthermore, Lawroski is considering establishing specifications for purity.

Hyman reported on batch experiments to investigate a few points in connection with the oxidation of the first column feeds for the first and second cycles.

Manning wrote to Hamilton on Saturday to tell him that we are sending a sample of U^{233} for a short bombardment with deuterons (Berkeley bombardment 8b) before Hamilton changes over to helium ions. A courier is scheduled to leave here by plane Tuesday and should deliver the sample by Wednesday, October 31. Manning indicates the material for bombardment 12a (protactinium plus helium ions) will probably be delivered at the same time.

Also on Saturday Farrington Daniels issued a memorandum to academic personnel. He quoted the following statement from Robert P. Patterson, Secretary of War.

> Public discussion of great issues such as the dissemination and regulation of knowledge of atomic science is one of the basic principles on which democratic government is founded. In it all citizens have a right to participate. American scientists in particular, because of their knowledge of the technical matters involved and because of their comprehension of the full social significance of the achievement, can contribute powerfully to it. Security, of course, still requires that nothing beyond the specific subject matter contained in the Smyth Report be brought into discussion, and the use of due care that matters outside the content of this report and still under security regulations be not inadvertently encroached upon. With this sole restriction, however, which applies to all citizens, our scientists should feel that it is proper for them as citizens to join actively in public consideration of this question.

Daniels infers from this recent statement that it is not necessary to submit for review those manuscripts which are concerned exclusively with political and social discussions.

Daniels also says that the organization known as the "Atomic Scientists of Chicago" has done important service in educating the public to the implications of the atomic bomb and the need for international cooperation and control. He would like to make an informal suggestion to the effect that use of the name of this association in press releases should be used sparingly, particularly when non-technical information is involved, in order that the influence of the organization may remain effective.

He then states the Metallurgical Laboratory has important scientific work to do for the welfare of the nation and meetings, which are not directly connected with the Metallurgical Laboratory should, in general, be carried out in the evenings and on Sundays.

* * *

Stewart sent a memo to Furney giving the quantities of neptunium (135 mg) and plutonium (13.7 g) found in the solution from the special neptunium run, received from Hanford on August 28, 1945. He requests formal transfer of the materials to our section.

English called me from Clinton Laboratories. We discussed the dates and conditions for the moving to Berkeley of Louis Werner, Dan Miller, and Bill Knox, who have accepted research assistantships at the Radiation Laboratory in Berkeley, in order to carry on their work toward their Ph.D. degrees. We talked about the possibility of English himself moving to Berkeley as an assistant professor. I suggested December 1 as a date for Werner to move to Berkeley. Miller and Knox have decided to accept positions at Berkeley but they cannot move to Berkeley until about March 1.

Dan Koshland, to whom I talked directly, wants more time to think it over before deciding whether he wants to do his graduate work at Berkeley. He thinks that it might be better to go to another university because he did his undergraduate work at Berkeley.

I told English that I am trying to arrange an assistant professorship at Berkeley for him at a total of somewhat more than $4,000 a year, including the summer salary. He said that he has an offer of an assistant professorship at the University of Chicago at a salary of $5,000 a year. English mentioned that he is going to Washington, D.C., tomorrow for some business appointments.

"Open Trial of Yamashita" is today's big headline. General Yamashita may face a death sentence for wartime atrocities for which he was responsible.

Tuesday, October 30, 1945

Groups 1 and 3 met at 8:28 a.m. in the New Chemistry conference room. In attendance were Asprey, Britain, Cunningham, Florin, Ghiorso, Hindman, Hopkins, Hyde, Kohman, O'Connor, Peterson, Simpson, Stewart, Studier, R. Thompson, S. Thompson, Van Winkle, Westrum, and I. I announced that there will be a meeting of group leaders with Hogness at 11:00 this morning to discuss the reports. There is a deadline of December 1 on the writing, including both the survey volumes and the collected papers; however, the survey volumes should have priority. I observed this should mean no research activities for us, but it is hard to forego work on the various targets so we will try to do both research and writing.

I described what I learned at Site Y about the conditions of their 95 fractions that can be made available to us. For each 160 g batch there are 10 liters of salt solution containing 100-200 mg plutonium with the lanthanum and the element 95. We discussed the equipment needed to process ten batches of the material.

I mentioned that we may be given the fourth Farmer's sample and then went on to say that the Np^{237} deuteron target was received Sunday, the Pa^{231} deuteron target is due Thursday, and the U^{233} deuteron target is scheduled to arrive at the end of this week or the beginning of next. Then, I made a few general statements about the type of measurements to make in working up these targets.

Stan Thompson said the neptunium target has been dissolved, and persulfate and bromate cycles run. Ghiorso has samples of the original solution and the persulfate-treated fraction. He has run pulse analysis curves and described the results at the meeting.

Roy Thompson told about the plans for the Pa^{231} target. He will be looking for x-rays in the protactinium fraction; this will require considerable repurification because of the decay series growing in. He observed that if the problem was a search for U^{231}, a lower energy bombardment would have been better.

I mentioned that the remainder of the old protactinium has been received from Agruss and that the time for another bombardment of protactinium depends on when material could be made ready. Answering a question from Jaffey, I said that the Bureau of Standards has checked our measurements on the Agruss material. Roy Thompson added that our analysis gave 5.18 mg while the Bureau found 5.2 mg, much less than originally claimed by Agruss. Roy Thompson hopes he can find the d,p3n yield in the protactinium target by measuring Pa^{229}. I added it would be possible to get also the d,p and d,n yields.

Hyde told about his plans for the U^{233} deuteron target. I suggested looking for the five-day so-called Np^{234} and said there should be nothing in the plutonium fraction from uranium impurities in the target except Pu^{238}. I pointed out it will be necessary to work fast in order to observe Np^{232} and Np^{233}.

O'Connor discussed his neutron bombardment of plutonium at Argonne--he found a longer half-life this time, probably because of better decontamination. The 95-96 fraction showed only 95^{241}. I mentioned that 10 mg of 49NH (Hanford-irradiated plutonium from CW-3) will be bombarded for a full day at Argonne in order to look for 96^{243}.

I asked about other 49NH measurements, and Stan Thompson said a 93 fraction separated from decontaminated 95^{241}-96^{242} shows that less than 0.7 percent of the alpha activity could be due to 95^{243}.

Florin reported another attempt to make PaF_5. A fluoride apparatus is nearly ready which he will use with neptunium and protactinium. He will try neptunium first.

Van Winkle is still working on the specific activity of protactinium. I mentioned the optical spectrum of protactinium has been observed sufficiently to find, from the hyperfine structure, that the nuclear spin of Pa^{231} is 3/2. I asked about the possibility of using the spectroscopic method to determine the nuclear spin of Np^{237}.

Westrum volunteered that during the last 15 minutes, he has found it would be possible to measure the neutron fission cross section of 49NG (Hanford-irradiated plutonium from CW-2) using the solutions now on hand; I commented that that shows good use of meeting time.

Kohman told about a letter saying the 95 sample at Hanford is still decaying with a four-year half-life.

The meeting adjourned at 10:10 a.m. Someone noted that the clock in the room has not been reconverted to standard time.

I called Ernest Lawrence and suggested that Bruno Pontecorvo, now working at Chalk River Laboratory in Canada, would be a good man to add to the staff at the Berkeley Radiation Laboratory. Lawrence asked that I explore Pontecorvo's interest in such a position, the position that he would expect, whether he is a Canadian citizen, and whether he is in a position to leave his job at the Chalk River Laboratory. I told Lawrence that I would report on my findings on these issues. We then discussed the building of a pulse analyzer for Ghiorso to be ready when Ghiorso moves to Berkeley. I will send drawings to Don Cooksey in order to get this started as soon as possible.

We also talked about the arrangements for the moving of the various people from the Metallurgical Laboratory to Berkeley. Lawrence told me that George Everson is out of town for about ten days and will act upon these matters when he returns.

I also phoned Hamilton who told me that bombardment 12b (Pa^{231} plus deuterons) has been broken down since Sunday, but they are starting up again today. He said, by Thursday or Friday, there will be a total of about 300 microampere-hours and the sample will be sent either by air or the "City of San Francisco," on Friday.

Hamilton told me that bombardment 8b (U^{233} plus deuterons) will be finished in a few days and again will be sent either by air or by the "City of San Francisco."

We also discussed how to package the protactinium target for its shipment to Chicago.

A courier left for Berkeley by plane with two cyclotron targets for deuteron bombardment: U^{233} (Berkeley 8b) and Pa^{231} (Berkeley 12a).

Professor Ogburn of the University of Chicago who proposed studies on social consequences of nuclear energy and the "atomic bomb" has suggested, according to today's paper, that we might break up our big cities.

Wednesday, October 31, 1945

I again called Hamilton in Berkeley. He described for me the lead spheres being made in the Radiation Laboratory machine shops for radiation shields for bombarded samples.

Hamilton indicated that bombardment 12b (Pa^{231} plus deuterons) will arrive in Chicago on Friday or Saturday by air. I told him that the target material for bombardment 12a should arrive in Berkeley by air today.

Hamilton told me that he is giving a talk on the heavy isotopes that have been produced in the cyclotron bombardments in Berkeley on November 15. He asked that I send him latest information on the results so that he could prepare his talk.

The text of President Truman's radio address last night appeared in this morning's newspaper. He has laid down a new wage and price policy designed to stem the tide of recent strikes threatening successful reconversion of industry from war to peacetime production.

NOVEMBER 1945

Thursday, November 1, 1945

This morning I attended the meeting of the Control Analysis Group (Group 2). Also present were Ames, Anderson, Fineman, Kohman, Manning, Sedlet, and Weissbourd. Ames described some carrying experiments with polonium, ionium, actinium, protactinium, and RaD + E using the process precipitation method. The highest percentages observed to be carried were 0.2, 0.2, 9.7, 0.3, and 1.4 percent, respectively. The precipitation from the Mallinckrodt plant filtrate (which is about 40 percent UNH with small amounts of Th^{232}, lead, and H_2SO_4) can be accomplished within an allotted time of 45 minutes. The filtrate can be counted within two hours of the first filtration. Ames, Jim Schoke (from the Physics-Instruments Section), and Kohman will go to Mallinckrodt in St. Louis next week to set up run analyses for radon in the Mallinckrodt process filtrates. I suggested that after writing a report on the radon and process precipitation work, we should focus attention on (1) the recovery of protactinium and ionium, (2) the search for Em^{221}, and (3) the determination of the half-life for radium.

Fineman talked about his work related to standardizing the glass and metal detector chambers to be used in the emanation portion of the "St. Louis problem." Two radium capsules from the National Bureau of Standards were used to provide radium standards from which aliquots were withdrawn for the tests. Also, aliquots of high grade ore supplied by NBS were dissolved and radon emanations run. In response to a question, I was informed by Fineman that all the ores went into solution. I said, "I thought that by dissolving a kilogram of the ore, we might be able to show that Pu^{239} is associated with natural-occurring uranium."

I announced that when the present St. Louis problem is finished, all those attending the meeting will become part of the "heavy isotope group." Sedlet and Anderson will work on the protactinium and ionium problem, Ames on the radium half-life determination, and Fineman on the Em^{221} studies.

In a memo to J. R. Gibson of Personnel, I request a $3 per week merit increase and promotion to the class of "Secretary A" for my secretary, Mrs. Ruth P. Rogers.

Don Ames and Truman Kohman describe in detail, in a memo, a tentative procedure for the rapid approximate analysis of radium in Mallinckrodt process filtrates from the uranium extraction process. The procedure, which is for use on clarified lead sulfate-gangue filtrates, should be used within one hour (and as soon as possible) after the filtration is performed. The procedure requires about 45 minutes to accomplish by an experienced operator.

In reply to Hamilton's telephone call of yesterday, I sent him the latest information on our heavy isotope work and enclosed a copy of a memo summarizing last month's work. I answer questions concerning the yield data which were reported in past monthly reports CS-3072, CS-3131, and CS-3237. I make the following additional observations on our current work and future plans:

> You asked about the results obtained with the thorium target which had been bombarded with helium ions. The sum total of our useful information on this is summarized at the top of page 4 of report CS-3237. We could not tell whether U^{230} was formed directly in this bombardment because so much of it had grown from Pa^{230} by the time of our chemical experiments, but we feel sure that the $\alpha,6n$ reaction to form U^{230} must have occurred.
>
> We don't have a great deal of information on relative yields with helium ions. However, I will give you some estimates, which at best are very rough, based on the results obtained on the bombardment of natural uranium with 44 Mev helium ions. The following table refers to the yield from U^{238} of the isotopes Pu^{241}, Pu^{240}, Pu^{239}, and Pu^{238} in the first 0.002 inch (that is, a layer of thickness about 100 mg/cm^2, corresponding to a reduction in energy from about 44 to 40 Mev) of this natural uranium metal target.
>
> | Reaction | Rel. Yield |
> |---|---|
> | α,n | 0.15 |
> | $\alpha,2n$ | 2.5 |
> | $\alpha,3n$ | 30 |
> | $\alpha,4n$ | 1 |
>
> That Pu^{241}, Pu^{240}, Pu^{239}, and Pu^{238} which comes indirectly via beta-decaying Np^{241}, Np^{240}, Np^{239}, and Np^{238} is included in these yields because, unfortunately, we have not yet had a chance to separate the two yields. I presume that in general the α,n type reactions have higher yields than the α,p type reactions. The above listed relative α,n yield actually comes from the experimentally found equal, thin target, yields of the reactions $U^{233}(\alpha,n)Pu^{236}$ and $Pu^{239}(\alpha,n)96^{242}$, and corresponds to a half-life of about twelve years for Pu^{241} when taken together with the observed yield of beta particles from Pu^{241} formed by $U^{238}(\alpha,n)Pu^{241}$. Hence, this may be our best value for the half-life of Pu^{241}, but it disagrees with some of our other data. In order that you might calculate some absolute yields for helium ions you might use our experimental results of 2 x 10^5 disintegrations per minute of Pu^{238} alpha particles from the 890 microampere-hours of 44 Mev helium ion bombardment in this experiment.

We have only a slight amount of additional information that doesn't appear in either the CS reports or the enclosed memorandum. There seems to be some preliminary evidence for the five-day K-electron-capturing Np^{234} or Np^{233} in the U^{233} which was bombarded with 44 Mev helium ions; this same radioactivity was formed in the enriched U^{235} which was bombarded with 22 Mev deuterons and which is referred to near the bottom of page 3 of report CS-3131. There is also some evidence in this U^{233} bombardment for a 10-hour alpha-particle-emitting plutonium isotope which would presumably be due to an isotope down in the neighborhood of mass 234, but it is a little premature to say anything about this. It is too early to give you much of a report on the results obtained from the bombardment of $Np^{237}(d,3n)Pu^{236}$ with an indicated thin target yield for the d,3n reaction which is about the same as that for the d,2n reaction for 22 Mev deuterons.

Unfortunately, we don't have any information on yields relative to fission products; we will go over our data and see if we can find something for you.

I will make a few statements as to future plans, together with some predictions. We believe that strongly pile-irradiated plutonium will contain the isotope Pu^{242}. This will surely be an alpha-emitter (that is, will be beta-stable), but probably of such a long half-life that the alpha-particles won't be seen for a long time. The next isotope up the line, Pu^{243}, will certainly be a beta-particle emitter. We have experiments under way in which we are irradiating plutonium containing the highest hypothetical content of Pu^{242} to look for the Pu^{243} beta-particles and for the 95^{243} and 96^{243} daughters. Our betting slightly favors 95^{243} to be a beta-particle emitter and of course we feel confident that 96^{243} will be an alpha-emitter with a half-life ten to 100 times longer than that of 96^{242}. Neutron-capture in 96^{243} will lead to 96^{244}, surely an alpha-emitter, and another neutron-capture will lead to 96^{245} which has some possibility of being a beta-emitter going to 97^{245}. The isotope 96^{247} will certainly be a beta-emitter, but this will be difficult to make by successive neutron-capture reactions. We intend to isolate a sufficient quantity of 95^{241} to bombard with helium ions to produce isotopes like 97^{244}, 97^{243}, 97^{242}, etc., which will probably all decay by K-electron-capture with perhaps some branching decay by alpha-emission. As you know, we also propose to bombard Pu^{239}-Pu^{240} mixtures with beryllium ions to form such isotopes as 98^{247}, 98^{246}, 98^{245}, etc., some of which will undoubtedly decay by K-electron-capture to corresponding isotopes of 97--also some isotopes of 97, 96, etc., will presumably be made directly. We also intend to study the properties of the isotopes of 95^{240}, 95^{239}, Pu^{245},

Pu^{244}, Np^{234}, Np^{233}, etc., these isotopes being made by methods which will be apparent to you on the basis of your knowledge of our bombardment schedule.

I had a conference with Donald A. McPherson of the John Wiley Publishing Company about the possibility of my writing a book on nuclear chemistry. We discussed royalty and other author arrangements. I told McPherson that Prentice-Hall is also trying to get me to write a book on nuclear chemistry, and we discussed the differences in author contracts between the two companies.

In a note to Roy Heath, who is now with the Wyandotte Chemicals Corporation in Wyandotte, Michigan, I accepted his invitation to speak to the Detroit Section of the ACS in January on some phase of nuclear chemistry. I mention that I will have to have the draft of the speech cleared, and ask Roy to inform me of the time and place of the meeting.

In a letter dated October 31, J. P. Magnusson (Larry Magnusson's father) of Augustana College in Rock Island, Illinois, invites me to speak at the college on December 6.

Today's paper says that President Truman, by his negative statements about Congress, blasted his chances for passage of two bills--one guaranteeing employment through federally financed work projects and the second the unemployment compensation bill. The Senate has passed both bills, but they are held up in committees in the House.

Friday, November 2, 1945

Hamilton called me from Berkeley to inform us that the Pa^{231} plus deuterons target (bombardment 12b) will be removed from the cyclotron late this afternoon with about 400 microampere-hours exposure and will be sent to Chicago via an Air Transport Command (ATC) plane leaving Sacramento at midnight tonight with three stops along the way.

Hamilton said the U^{233} plus deuterons target (bombardment 8b) will remain in the cyclotron until November 6 to accumulate 350 microampere-hours after which time it will be sent to us also via ATC.

Two weeks on each of the helium ion bombardments will follow the U^{233} bombardment.

I phoned Professor Magnusson at Augustana College in response to the invitation I received yesterday. I told him I would be glad to come and give a talk and that my title would be "A Discussion of Some of the Principles of Nuclear Energy." I informed him that either December 13 or 14, would be satisfactory dates and that I would be pleased to accept his invitation to dinner preceding my talk.

I mailed Pierce Selwood the title and abstract of the talk I will give at Northwestern University on November 16, saying that the text has now been cleared through Security.

"Japan-Washington Hop!" reads this morning's headline. Four B-29's flew nonstop from Japan to Washington in 27 hours, 29 minutes.

Saturday, November 3, 1945

I wrote to H. D. Smyth about what I consider to be the unfair treatment of chemical accomplishments compared with those attributed to physics in the Smyth Report. I make a general suggestion as to how the report could be revised to correct the marked imbalance. The text of my letter, in its entirety, is as follows:

> A large number of chemists, both on and off the Manhattan District program, have pointed out to me the extraordinarily brief and undetailed treatment, compared to the treatment of physics problems, given to chemical problems and accomplishments in the "Smyth Report." It is only recently, within the last week or so, that I have had a chance to read and study this Report and I must say, as one who has been in a position to watch a good deal of the chemical development take place, that I also am struck by the imbalance between your treatment of the physical and chemical aspects of this great accomplishment.
>
> I am certain that this represents no conscious effort on your part to belittle the accomplishments in the chemical phase of this work. Perhaps it reflects, rather, an understandable unfamiliarity with the details of the chemical work that was done. If this is the case it is, of course, particularly disappointing to the chemists that discussions with them were not held during the period of the preparation of the Report. It seems to me that this feeling is justified because it is now apparent that a person's contribution to this development is being judged and will continue to be judged to a large extent on the basis of this Report, which now enjoys official and authoritative status. In fact, in one large University the administration is discriminating against its Chemistry Department in the allocation of research funds because this administration, incorrectly, was led by the Report to feel that the contribution of the chemists was insignificant. I have decided to write to you because I feel that you will be interested to learn of this attitude and also in order to indicate to you, broadly, some of the ways in which we feel that the Report might be changed in order to remedy this situation, insofar as the Plutonium Project is concerned, in case a revision seems necessary or in case it should be decided that a complementary "Smyth Report," which emphasizes the chemical aspects and would be perhaps written by a chemist, should be issued.

For example, the chemical program, which was carried on at the University of California during 1940 and 1941, and which laid the basis for the plutonium extraction program, was dismissed with about one sentence. True, this program was not represented in that long series of administrative conferences whose history is interwoven throughout many of the chapters of the Report, but it nevertheless played as vital a role as did the physical programs at Columbia and Princeton which are more completely described.

The later development of the separations processes and the basic chemistry program, which took place in parallel with the pile development program is described in a manner which, it seems to us, leads to the impression that this vital development merely played the role of an appendage to the program. In particular, apart from some mention of a few of the chemists who took part in the early administrative history, there is no credit given to the men who made the vital discoveries and did the work (in contrast to your treatment of the physics field). For example, not even a mention is made of Cunningham and Werner, the first men in the world to isolate in a pure state weighable quantities of a synthetic element! This work, on the ultramicrochemical scale, was of inestimable importance to the Metallurgical Project and will surely be recorded as a historic event. And no mention at all was made of S. G. Thompson, the man who is largely responsible for the conception of the chemical process used at Clinton and Hanford, whose accomplishments on the basis of this alone, surely rank far above those of many of the people who are mentioned! No mention is made of Burton, who contributed so vitally to the solution of a number of radiation problems.

I might point out a few rather minor errors which could easily be corrected in a revision. In Chapter VI, paragraph 6.34, the wrong isotope and method of production is described in connection with the discovery of element 93 by McMillan and Abelson; their discovery of element 93 resulted from the production of 93^{239} from uranium bombarded with neutrons as described in their article which was published in The Physical Review (Phys. Rev. 57, 1185 (1940)). In paragraph 6.35 of the same chapter the new rare-earth series is regarded as beginning with uranium, a pre-project view, whereas from the chemical program on the project it has been clear for a year or two that this series begins several elements earlier. In Chapter VII, paragraph 7.3, you refer to studies of separation processes using uranium as a stand-in for plutonium; I don't see how such experiments could be performed and know of none that were.

There is another point which I should like to bring to your attention. Segrè, Kennedy, and others have discussed with me your treatment of the fissionability of Pu^{239} which you picture as a predictable property, largely on the basis of Turner's considerations. We knew of these predictions at the time that the demonstration of a slow neutron fisssionability of Pu^{239} took place in Berkeley but felt that the predictions were too broad to be at all dependable. For example, Turner (in Phys. Rev. 57, 950 (1940)) predicted that practically all transuranic nuclei would be slow neutron-fissionable. Since that time tests have been made on Np^{237}, Pu^{239}, Pu^{240}, and 95^{241} (all predicted to be fissionable) and three out of the four have been found to be non-fissionable with slow neutrons.

Another consequence of the Report's brief treatment of the chemists' contribution is that it is very difficult to prepare, on a chemical phase of this work, even a fundamental talk, which will have any significant content and at the same time be acceptable to the censors, because the security people rely so heavily upon the Report. I wonder whether you would support us, for such talks, in the clearance of such additional chemical information as would be commensurate and consistent with the physics information in the Report?

Iz Perlman returned to Chicago by train from Pendleton, Oregon, this afternoon at 1:00 p.m.

Zachariasen, using x-ray diffraction analysis, has determined that the structure for the oxide of element 95 prepared by Cunningham is that of the dioxide, thereby establishing the IV state for element 95. He reported this information to Daniels with a carbon to Cunningham and me.

From my observations and inferences from circumstantial evidence, I believe that Helen is pregnant.

There is heavy fighting in the Chinese civil war according to a Chinese communist dispatch.

Sunday, November 4, 1945

Kohman, Ames, and J. A. Schoke (of Physics-Instrumentation in Eckhart Hall) left Chicago for St. Louis to assist in the Mallinckrodt Chemical Company process filtrate analysis problem.

Winston Manning, Stan Thompson, and I played 18 holes of golf at the Evergreen Golf Club. Our scores were WM-113, ST-99, and GS-104.

Helen and I took Margaret Smith (Edrey Albaugh's sister) to Wrigley Field to see the Chicago Bears-Green Bay Packers football game. The Bears won 28 to 24. Afterwards we went out to dinner.

Elections were held in Hungary today. They are the first national elections to be held in central Europe since the end of the war.

Monday, November 5, 1945

The organization of Section C-I, as of November 5, 1945, is as follows:

Glenn T. Seaborg - Section Chief
Ruth P. Rogers - Secretary to Seaborg
Kathleen Florin - Clerk

Winston M. Manning - Associate Section Chief
Isadore Perlman - Associate Section Chief
Jane Horwich - Secretary
Mildred A. Bolden - Secretary, loaned from Information Office
Norma D. Shaw - Draftsman, loaned from Information Office

Group 1, Heavy Isotopes,
Seaborg, Glenn T. - Group Leader
Cunningham, Burris B. - Research Associate
Ghiorso, Albert - Research Associate
Hindman, J. Clark - Research Associate
Hyde, Earl - Research Associate
Katzin, Leonard I. - Research Associate
Jaffey, Arthur H. - Research Associate
James, Ralph - Research Associate
Magnusson, Lawrence - Research Associate
Morgan, Leon - Research Associate
Osborne, Darrell W. - Research Associate
Peterson, Sigfred - Research Associate
Robinson, Herman - Research Associate
Simpson, Oliver S. - Research Associate
Studier, Martin [SED] - Research Associate
Thompson, Roy C. - Research Associate
Thompson, Stanley G. - Research Associate
Westrum, Edgar F. - Research Associate
Florin, Alan E. - Research Assistant
Hausman, Eugene A. [SED] - Research Assistant (half-time)
Hopkins, Horace H. [SED] - Research Assistant
O'Connor, Paul - Research Assistant
Scott, Benjamin - Research Assistant
Van Winkle, Quentin - Research Assistant
Walsh, Patricia - Research Assistant
Erway, Norman - Technician
Thomson, Helen - Technician

Group 2, Control Analysis,
Kohman, T. P. - Group Leader
Anderson, Herbert H. [SED] - Research Associate

Ames, Donald P. [SED] - Research Assistant
Fineman, Phillip [SED] - Research Assistant
Sedlet, Jacob - Research Assistant
Weissbourd, Bernard [SED] - Research Assistant
Calhoun, Opaline - Technician

Group 3, Recovery,
Stewart, D. C. [SED] - Group Leader
Asprey, Larned B. [SED] - Research Assistant
Britain, J. W. [SED] - Research Assistant

Group 4 - Solvent Extraction,
Lawroski, Stephen - Group Leader
Gilbreath, J. R. - Assistant Group Leader
Billheimer, J. S. [SED] - Research Assistant
Blaedel, Walter J. - Research Associate
Hagemann, French T. - Research Associate
Hyman, Herbert H. - Research Associate
Schaffner, Irwin J. - Research Associate
Ader, Milton [SED] - Research Assistant
De Rose, F. S. [SED] - Research Assistant
Gaarder, Sidney - Research Assistant
Goeckermann, Robert - Research Assistant
Hausman, Eugene A. [SED] - Research Assistant (half-time)
Kelley, Alec [SED] - Research Assistant
Murray, S. A. [SED] - Research Assistant
Post, Roy [SED] - Research Assistant
Schraidt, John H. [SED] - Research Assistant
Boykin, Pearline - Technician
Giacchetti, Olga - Technician

(Recently we were given three additional SED men to work in this group: Billheimer, De Rose, and Murray.)

Figures 12 and 13 show Marty and Chris Studier and Don Stewart on a recent trip to the Indiana Dunes.

I attended a 9:00 a.m. meeting of the Laboratory Council in Room 209, Eckhart Hall. Others present were Branch, Cole, Daniels, Dempster, Furney, Fussler, Hogness, Hughes, Jacobson, Jesse, Lapp, Mulliken, Nickson, Wilkinson, Willard, Zachariasen, and Zirkle. Daniels read a letter from General Groves responding to Daniels' and Gustavson's letter of October 8. Groves recommended that Argonne Laboratory, the Information Division, and the Patent Division, should be considered a part of the Metallurgical Laboratory and that the existing staff level should be maintained pending Congressional action. At present, no jobs can be assured beyond June 30, 1946.

Daniels then announced that Major McKinley will be transferred to Oak Ridge and will be succeeded by Major Stowers. Ralph Lapp has taken a leave of absence until January 1, 1946; many of his duties will be taken over by L. C. Furney.

Figure 12. Marty and Chris Studier, Indiana Dunes, Autumn 1945.

Figure 13. Don Stewart, Indiana Dunes, Autumn 1945.

Daniels reported that Groves has appointed a committee to study declassification; the local committee consists of Hogness, Franck, Dempster, Stone, and Mulliken.

I received a letter dated November 1 from Emilio Segrè saying that due to a present lack of "appropriate convoys" this shipment of material would be delayed a few weeks. He also asked if there was "anything new about (y)our candidate for the pile"--[Pontecorvo].

I phoned P. W. Selwood at Northwestern University to discuss the talk I am going to give at the ACS symposium at Northwestern University on Friday, November 16. I told him I intend to announce the discovery of element 95 and 96 and other results of our research here. The program will start at 10:30 a.m., and I will be the first speaker.

In a call to George Everson at Berkeley, I arranged for Perlman, Morgan, Robinson, and Werner to be placed on the Berkeley Radiation Laboratory payroll as of December 1. In general, Everson agreed that the salaries of people transferring from the Met Lab to Berkeley will remain at the levels they were in Chicago; he will discuss the matter with Latimer. I told Everson that good people are transferring to Berkeley.

I then sent Everson Personnel Security questionnaires filled out for Blaedel, Miller, and Werner. Four individuals, Perlman, Morgan, Robinson, and Werner, will transfer to Berkeley with the first contingent and will start work there on December 1. I say that I do not know what moving arrangements he will want to make. In the case of Werner, he will be transferring to Berkeley from Clinton Laboratories, now operated by the Monsanto Chemical Company. Since Perlman's point of origin was Berkeley, I say that probably the Met Lab should pay his moving expenses. Robinson and Morgan have other points of origin, and it would be proper for the Radiation Laboratory to pay their moving expenses.

In an informal letter to Bruno Pontecorvo at Montreal, I inquire about his future plans and ask if he would be interested in joining our staff at Berkeley which will engage in a future proposed program of nuclear investigations. I inform him that Emilio Segrè plans to return to Berkeley in the near future and is enthusiastic about the possibility of having him become a member of the staff.

I wired Gordon Leader at Richland a reply to his telegram that I received today. I say, "In answer to your telegram nature of work solvent extraction or heavy isotopes salary about same as you now receiving and late this month satisfactory time to report anxious to see you during proposed trip to Wilmington."

I also received a letter from John H. Mahoney, Security Officer, defining the sections of my talk for Northwestern University disapproved for public release by the Washington Public Relations Office.

Daniels sent Colonel Nichols a request for the loan of 25-40 kilograms of enriched U^{235} and the authorization to build a Chemistry Division proposed experimental high-temperature pile at Argonne. He refers to the initial research on two other pile types (the homogeneous, heavy-water pile proposed by Clinton, and the all metal, fast-fission pile proposed by Argonne) and suggests that work be pushed vigorously on these two types in addition to our proposed high-temperature pile. Our pile will use beryllium oxide and enriched uranium oxide (containing 20 percent of U^{235}). Daniels makes the point that this design appears promising for a quick demonstration of the practical utilization of power with the minimum investment, while at the same time giving experience in the conversion of thorium into U^{233}. Daniels expresses the need for new reactors as follows:

> We are now in competition with other nations in the field of nucleonics, and we must develop actively all possible piles, particularly enriched piles. Tests of breeder piles are necessary from a military standpoint, and there is a public demand for power piles and peacetime applications of nuclear energy. To obtain full public support we believe that a power pile should be demonstrated at the earliest possible moment.

Daniels reviews the current status and presents a program plan, schedule, and budget. Arguments are given for the immediate construction of such a pile at the Argonne site. As a practical matter, Daniels feels that if Met Lab staff members can be assured of an active program for the development and test of a high-temperature pile, they may be encouraged to stay on until the future of the Met Lab is resolved.

In another memo to Colonel Nichols, Daniels suggests a two-day conference with invited representatives of the Navy and certain industrial groups on the use of nuclear energy in operating engines for useful power. Daniels says that General Electric; Westinghouse; and Solberg, Gunn, and Abelson of the Department of Navy are specially interested in nuclear power for driving ships and have already had conferences with us.

Prime Minister Attlee is scheduled to meet President Truman next week. His objectives are to internationalize the "atomic bomb" and to secure a five billion dollar loan from the U.S., according to the newspaper today.

Tuesday, November 6, 1945

I attended an 8:28 a.m. meeting of Groups 1 and 3 which met in the New Chemistry conference room. Others present were Asprey, Britain, Cunningham, Florin, Ghiorso, Hindman, Hopkins, Hyde, Jaffey, Katzin, Magnusson, Morgan, O'Connor, Osborne, Peterson, Sedlet, Simpson, Stewart, Studier, R. Thompson, S. Thompson, Van Winkle, and Westrum. I opened the meeting by saying Simpson has some interesting work to report. He talked about fractionation studies for the separation of elements 95 and 96, using a 95-96 sample from Hopkins containing about 9,360 c/m of 95^{241} and about 70,640 c/m of 96^{242}. It was concluded that 95 sesquioxide decomposes to monoxide, accounting for a greater volatility than 96 sesquioxide which does not decompose to monoxide. Because of this, it appears possible to separate readily 95 from the 96 and lanthanum using the fractionation process.

Stan Thompson next reported on the alkaline fusion method for separation of element 95 and 96. If one assumes that 95 has a higher +4 state under sodium nitrate fusion, it should be carried on the praseodymium dioxide used as a carrier. As the intial experimental results showed a greater 95:96 ratio in the water-soluble fraction than in the insoluble oxide fraction, I suggested that the experiment be repeated.

Osborne talked about the protactinium target (bombarded with deuterons) that we recently received. The target material was difficult to dissolve but he finally was able to get about 60 percent of the total alpha activity into solution. He used ether extraction to separate the uranium fraction and diisopropyl ketone to separate the protactinium fraction. Alpha pulse-height analysis for the uranium fraction showed only the characteristic peaks for the U^{230} series and no detectable new activities. However, Geiger decay curves run by R. Thompson indicated a new x-ray activity with a half-life of about 4-5 days. Several energies corresponding roughly to the L and K x-rays of a heavy element, plus a few harder x-rays were detected. All these x-ray components were observed to have the same half-life. It is thought that the heavy isotope is U^{231}.

Van Winkle said he has separated a "thorium fraction" from the purified uranium fraction and obtained only about 4,000 c/m (about 0.02 percent of the total uranium fraction activity); this was followed down to about 400 c/m. The half-life corresponded closely to that for Th^{226} which would have resulted from the U^{230} series. Protactinium-229 did not appear to be present. Alpha pulse-height analysis indicates daughters of U^{230} only. Fewer than 5 c/m of x-ray activity was found in this fraction.

Osborne said that the original protactinium fraction was analyzed on the pulse-height analyzer for a long period of time and that pulse amplitude distribution peaks for the U^{230} series could be detected when the Pa^{231} peak was absorbed out.

I noted that after the Pa^{232} decays sufficiently we will have the first pure source of Pa^{230}; I suggested that we carefully study its radiation characteristics.

Cunningham reported that samples of the chloride and oxide of element 95 were sent to Zachariasen for x-ray diffraction analysis. The observed structures were those for the trichloride and dioxide. The oxide sample was retrieved, and Cunningham and Fried reduced it with hydrogen and gave it back to Zachariasen. Results have not yet been obtained on this sample.

Cunningham briefly talked about preliminary experiments that give some evidence for a +4 state of 95 in solution. The initial precipitate formed by adding NH_3 to a peroxide solution of 95 has a dark reddish color that fades to the usual salmon pink color upon standing. It is thought that the dark color can be due to a +4 ion or to a relatively unstable peroxide structure which is destroyed upon standing. There is very little evidence for increased extraction of 95 into hexone under strong oxidizing conditions. I suggested we may consider the use of terbium and samarium as stand-ins for 95 for studies of the +4 and +2 states, respectively. Florin suggested that he try fluorination of the +4 oxide of 95 to yield a +4 fluoride.

Asprey, Westrum, and Ghiorso described the decay measurements being made on various 95 samples. A growth in the plutonium fractions from sample 49NG is being observed which appears to be identical to the growth observed by James in similar samples derived from milking experiments. Similar experiments will be run on a second sample, 49NH.

Westrum reported on fission measurements made on eight "95 samples" at the Argonne pile a few days ago.

I announced that Site Y is going to make a mass spectrographic analysis of plutonium from a small amount of our 49NH sample which was sent to them (we hope to obtain the percent of Pu^{241}).

I commented on the conjectures we have been making concerning the beta particle attributed to Pu^{241}. There seems to be a discrepancy between half-life values for 95^{241} obtained by Cunningham and those obtained by James over an eight-month decay period. Calculations of the half-life of 95^{241} made from beta-particle measurements (thought to be Pu^{241}) seem to check James' value of 35-50 years. Cunningham, in weighing experiments, observes a half-life of about 300 years. Morgan then outlined four possible explanations for the above discrepancy which requires systematic investigation.

I said that Kohman's group should soon finish their Mallinckrodt plant process work. Fineman expects to work on the emanation of atomic weight 221. Ames will work on the specific activity of radium. Sedlet and Anderson will work on the distribution of protactinium and ionium in the Mallinckrodt process.

I reported that the alpha activity in the Np^{237} sample seems to be growing with range of 4.1 to 4.4 cm. This as yet unidentified activity appears to be a function of bombardment.

Magnusson described what he thinks is a fast and efficient method for separating uranium, neptunium, and plutonium. It depends upon the separation of plutonium and neptunium in the +4 state from uranium. Neptunium could be extracted first, keeping the plutonium in the +3 state and then oxidizing the plutonium to the +4 state and re-extracting. Recovery appears to be very good.

Scott reported on a new quenching circuit he has developed for use with xenon-filled G-M tubes. By using a cathode follower after the usual Neher-Harper circuit, the effective capacity of the anode wire is reduced and the coincidence loss is substantially reduced in high counting rate applications.

I announced that the U^{233} plus deuterons target (Berkeley cyclotron bombardment 8b) should be in tomorrow and should be processed as soon as possible.

I wrote a note to Hamilton to remind him that we need several target mounting units for the plutonium targets being prepared for us at Site Y. I say our understanding is that we will mount the targets in the target mounting units and will ship them to him, each in a special lucite box.

I wrote to Don Cooksey at the Radiation Laboratory in Berkeley to inform him that I am sending, under separate cover, diagrams and photographs for the alpha pulse analyzer which we would like the electronics group to build for us. I tell him that Robinson will be in Berkeley about December 1 and will be available for advice. If the electronic men have any questions, they may contact me, or preferably, Robinson or Ghiorso.

In a letter to Compton, Daniels made a plea for authorization to begin building a high temperature pile at Argonne for demonstrating the power production capabilities of nuclear energy; he thinks this can be accomplished before the end of the present contract period on June 30, 1946. He said it appears clear that no national policy on a nucleonic program will be established for several months and that the Met Lab is faced with the decision of either "marking time" or going ahead with some useful interim program. Daniels feels strongly that we must go forward in the interest of national welfare and in order to hold project scientists.

A bill, S. 1557, "For the Development and Control of Atomic Energy," was introduced by Senator Ball today in the 79th Congress, First Session. The bill, which would establish an Atomic Energy Commission composed of the Secretary of State, the Secretary of War, the Secretary of the Navy, the Secretary of the Interior, the Secretary of Commerce, and four members at large, was read twice and referred to the Special Committee on Atomic Energy.

Lieutenant General Lucius D. Clay has asked that 300,000 tons of food be shipped to the American zone of Germany. He said that food is not available locally in any substantial quantities. He asked that the U.S. Army finance shipments, and Germany could pay for the supplies later.

Wednesday, November 7, 1945

I received a letter from Charles K. Hunt, Program Committee Chairman of the Detroit Section of the ACS. He said that they were happy to learn that I accepted the invitation to address the Detroit Section extended by Roy Heath. The meeting is presently planned for Janaury 8. Dinner will be at 6:30, and he hopes that I will be able to bring my wife. He would like me to send him a small photograph, a biographical sketch, the title, and a short summary by December 1.

The U^{233} plus deuterons sample (Berkeley cyclotron bombardment 8b) arrived today and is now being processed.

Hamilton wrote on November 5 to say that a radio-phosphorus sample has been sent to us by Railway Express for calibration. Berkeley's values were 1.0 millicurie P^{32} (1.3 mg Na_2HPO_4) in 10 cc of 0.1 N HCl. Hamilton previously asked me if we would check this calibration for him and I agreed we would.

Kohman and Ames returned from their trip to the Mallinckrodt plant in St. Louis. Schoke, who accompanied them left St. Louis on November 5 after setting up alpha counters for use in the tests. Kohman and Ames completed the tests of their rapid, approximate radium analysis procedure at the Mallinckrodt plant in St. Louis using actual process solutions in the uranium extraction plant. The precipitation analyses were made by Ames, with assistance from Frank Edwards of Mallinckrodt. Emanation analyses were run by Edwards and company analysts under his direction.

The Mallinckrodt people in charge of the uranium plant and radium control expressed satisfaction with the results and felt the methods we developed will be adequate for their purposes. They asked that we continue our investigations on an analytical method for excess-barium solutions but agreed to assume responsibility for any other future development and application work. This means the joint program with Mallinckrodt can be terminated soon.

Kohman was assured that samples of materials from the pilot plant or main plant can be easily obtained through the Area Engineer Office should we desire them for our work. Kohman will prepare a complete report on this work.

Today's paper reports that Foreign Commissar Molotov asserted that the atomic bomb cannot be kept secret and that the Soviet Union will have "atomic energy and many other things."

Thursday, November 8, 1945

I attended the meeting of the Solvent Extraction Group this morning. Others in attendance were Billheimer, Blaedel, Gaarder, Gilbreath, Goeckermann, Hyman, Lawroski, Manning, Murray, and Schaffner. Blaedel reported on a recent extraction run which gave poor decontamination because inferior hexone was used. An improvement in decontamination resulted when properly pretreated hexone was used.

Hyman discussed work over the last two weeks that has been centered on ruthenium behavior in the extraction process. The distribution ratio between the aqueous sample solution of ruthenium and hexone is 150 in favor of the aqueous phase. Upon oxidation of the ruthenium using our new procedure (with 0.5 M HNO_3 and 0.1 M $Na_2Cr_2O_7$ added in the cold, heating the solution to boiling for one hour, cooling, and final addition of 0.1 M $Na_2Cr_2O_7$), the distribution ratio with hexone was reduced to about 15. Although the oxidation states of ruthenium are not known, the results thus far obtained indicate that at least three forms of ruthenium exist with different distribution ratios. The advisability of beginning to think in terms of a pilot plant for ruthenium recovery was discussed. It was thought that Hanford would probably be the best place for such a pilot plant. The use of the hot semiworks dissolver was also discussed in relation to operational and mechanical difficulties as well as radiation hazards.

I received a reply dated November 6 from Everson in response to my telephone call and letter of November 5, about the transfer of Perlman, Morgan, Robinson, and Werner to the UC Radiation Laboratory payroll as of December 1, 1945. Everson requested information regarding the salaries, classifications, work week, and the status of the University of Chicago contract for these individuals. Upon receipt, he will make formal offers of employment and get the machinery going concerning their transfers. There is still some uncertainty about reimbursement for transportation.

I have been invited by John Lewellen to be a guest on the Quiz Kids Program. Today I received a phone call from Maggie O'Flaherty of the program. She discussed plans for an appearance as a guest this coming Sunday, November 11. John Lewellen is in charge of the program and will call me. She said the program is broadcast nationwide on the

Blue network of the ABC from 6:30 to 7:00 p.m. The broadcasting studio is on the 20th floor of the Merchandise Mart. She asked that I be there by 4:30 p.m. so that there would be time to prepare for a warm-up session at 6:00 p.m. just before we go on the air.

Later on in the day I received a telephone call from John Lewellen of the Quiz Kids Program. He said he would have the Quiz Kids, Sheila and Patrick Conlan, Robert Burke, Harvey Fishman, and Richard Williams, think up some questions and then discuss their appropriateness with me tomorrow morning. He explained the procedure that would be followed on the program and said that Joe Kelly will introduce me. Following the introduction they would like to have me use some of the material that was suggested by Maggie O'Flaherty at the beginning and also at the end of my participation on the program.

I called Mark Fred at the Armour Research Foundation about my forthcoming talk to the Physics Club of Chicago on December 1. I told him that the title of my talk would be, "A Discussion of Nuclear Energy." He requested that I send him some biographical information.

I sent a summary of our plans and our reasons for requesting plutonium foils to T. S. Chapman. My memo is as follows:

> These foils will be used as targets for bombardment with 22 Mev deuterons and 44 Mev helium ions in the Berkeley 60-inch cyclotron. These bombardments will lead to the production of isotopes of elements 95 and 96, making it possible to study the important nuclear properties of these isotopes. This is important because many of these isotopes will be formed in the high-energy piles which are planned for the future and a knowledge of the properties of these isotopes is important in planning for these piles. The plutonium foils will consist of the isotopic mixture Pu^{239}-Pu^{240}-Pu^{241}, hence the deuterons might yield the isotopes 95^{242}, 95^{241}, 95^{240}, 95^{239}, 95^{238}, and 95^{237}. The helium ions might yield the isotopes 95^{244}, 95^{243}, 95^{242}, 95^{241}, 95^{240}, 95^{239}, and the 96 isotopes of these same masses.
>
> It is also planned to bombard these foils with heavier particles, such as beryllium ions, with the possibility of obtaining isotopes of elements of such high atomic number as 97 and 98, as well as new isotopes of elements 95 and 96.
>
> It should be borne in mind that there is always the possibility of producing some transuranium isotope with very unusual and useful fission properties and this can come about only as the result of a thorough study of the whole transuranium region.

In an official notice W. M. Branch announced that Monday, November 12, 1945, would not be observed as an official holiday for Armistice Day, November 11.

In addressing the House of Commons, Churchill urged Britain to support the U.S. in refusing the "secret of atom bomb production" to the Soviet Union. Churchill declared that if the Soviet Union possessed the secret alone she would not share it.

Friday, November 9, 1945

John Lewellen called me again this morning. He read to me a list of questions that the Quiz Kids might possibly ask. We then discussed the appropriateness of these questions. He said these questions were the range of possibilities and in no sense an exact preview.

I replied to George Everson's letter of November 6, in which he requested additional information to facilitate the transfer of Perlman, Robinson, Werner, and Morgan, to the University of California Radiation Laboratory. Information on the job classification, salary ranges, present salary, and a comparison of the Met Lab salary scale with the Berkeley salary scale is included in my letter. With regard to the possible complications of moving expenses, I suggest that Everson contact Wayne Johnson of our Personnel Office and work out arrangements for each of the four men.

I received a letter dated November 6 from Gordon Leader. He is anxious to come to Chicago to work rather than go to Wilmington. However, so far there seems to be considerable opposition to his release. Leader says that he will let me know when he can get away as soon as he is in a position to know.

Daniels asked Lum of the Monsanto Chemical Company at Dayton about the availability of about ten kilograms of very pure bismuth.

The temperature fell 15° in 50 minutes this morning, dropping to 43°F. The prediction, according to the paper, is that the temperature will stay between 51 and 20°F tonight.

Saturday, November 10, 1945

William Knox wrote me from Clinton Laboratories, saying that he is happy to accept the proposed chemistry graduate student fellowship offered by the University of California and the arrangement in which he might work for the Radiation Laboratory until such time as the fellowship could be started. Because of work at Clinton Laboratories and other personal reasons, Knox said he would be unable to leave Oak Ridge until some time next spring; he would like to work out some arrangement as to the exact date of his transfer. He expressed his appreciation for my role in making the fellowship available to him.

A reply arrived from Stoughton about our request (Manning to Stoughton, October 24) for removal of our samples from the Clinton pile. Stoughton says the samples will be sent in about a week. He includes irradiation data on each sample.

In a letter to Hamilton, Manning states that the sample of 51 which Hamilton requested several weeks ago has been sent and should have arrived by the time Manning's letter reaches him. The sample contains approximately 10^6 c/m which, he says, need not be recovered after Hamilton's experiment.

Manning requested additional information about the total micro-ampere-hours, effective mid-point time, and the time at end of bombardment, for the three Berkeley cyclotron dueteron bombardments 8b, 12b, and 13b. He said these three bombardments have provided plenty of work for our Section C-I people.

The proposal to unite the Army and Navy into one department took the lead spot in this morning's newspaper. Hearings are now being held in Congress, and the Army is supporting and the Navy opposing the merger.

The predicted cold wave did not arrive.

<u>Sunday, November 11, 1945</u>

Winston Manning, Stan Thompson, and I played 18 holes of golf at the Evergreen Golf Club. Our scores were WM-112, ST-93, and GS-107.

Helen and I went to the ABC broadcasting studio on the 20th floor of the Merchandise Mart at 4:30 p.m. Following a get-acquainted session at about 5:00 p.m. with John Lewellen, Joe Kelly, and others there was a warm-up session at 6:00 p.m. with Joe Kelly and the Quiz Kids, Sheila Conlan, Patrick Conlan, Robert Burke, Harvey Fishman, and Richard Williams. (See Figure 14.) The program was broadcast nationwide on the ABC Blue network from 6:30 to 7:00 p.m.

Joe Kelly: Thank you, Bob Murphy, and good evening, everyone. Well, children, we have the great honor to present as our guest observer on this Armistice Day program, a most distinguished scientist, Dr. Glenn T. Seaborg. Dr. Seaborg was a co-discoverer of the new element, plutonium, at the University of California and he was closely concerned with the development of the atomic bomb at the University of Chicago. Because the whole world is rightfully curious about the atomic bomb and its grave implications for world peace, or world annihilation, it seems a very appropriate subject for our Armistice Day discussion.

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Figure 14. Glenn Seaborg with Sheila Conlan and Bob Burke on Quiz Kids Program, November 11, 1945.

Joe Kelly: (cont.) We are going to reverse roles in our final question session to let you children quiz Dr. Seaborg on the atomic bomb. I hope you'll have some good solid questions ready for him.

Now let's get roll call started. Harvey?

Harvey: I'm Harvey Bennett Fishman. I'm 15 years old and I'm a sophomore at the South Shore High School in Chicago... (fade out)

Joe Kelly: And now, children, it's time to introduce the guest of honor on our Armistice Day program and time to let you ask some questions.

Today we commemorate the Armistice of World War I. Since then we've gone through another devastating war and with the secrets of atomic energy discovered, God help us if we don't see to it that there is no chance for a third world war.

As I've told you earlier, our guest observer is one of the foremost men of science in the world today. As professor of chemistry at the University of California, he was a co-discoverer of the new element plutonium. About three years ago he disappeared into the University of Chicago's Metallurgical Laboratory to work on the atomic bomb project. The subject of atomic energy and the answers Dr. Seaborg will give to your questions may well be the most important message of your life.

And now I have the honor to introduce Dr. Glenn T. Seaborg. (Applause)

GTS: Thank you, Mr. Kelly.

When I was invited to take part in your Armistice Day program I said I would on one condition, that I was not in competition with the Quiz Kids. Even though I am not being scored tonight I am still in competition with their ability to think up provocative questions but I'll do my best to think up the right answers.

Now I'm ready if they are.

Joe Kelly: All right, children, fire away. Let's start with five-year-old Sheila.

Sheila, what would you like to know about the atomic bomb?

Sheila: What do you put together to make the atomic bomb? (Laughter)

GTS: Well, Sheila, you can use either plutonium or U^{235}.

Joe Kelly: Well now, you see, Sheila, we know how to put one together, don't we?

All right, Pat, how about your question?

Pat: Uhm.

Joe Kelly: Talk in your microphone, son, that's it.

Pat: How far can the explosion of the atomic bomb be felt?

GTS: Well, Pat, the explosion can probably be felt for a distance of 50 or 100 miles. Let's say if one was dropped in Chicago, the explosion could be felt up in Milwaukee. I believe that's where you live, Bob.

Bob: Yeah. (Laughter)

GTS: I think, though, what's more important is that the whole downtown area of Chicago could be obliterated by one atomic bomb, including the Merchandise Mart where we are now, here.

Joe Kelly: Bob, how about you? Do you have a question?

Bob: Well, Doctor, if there is another war, in what ways do you think the atomic bomb will be used against the enemy?

GTS: Well, Bob, they could be dropped from an aeroplane like they were at Hiroshima or at Nagasaki. But in another war they'll probably use much more efficient ways than that. They might be shot over in the V-2 type rockets. I understand that in the future, it will probably be possible to shoot rockets clear across the ocean and maybe the accuracy will be great enough in time, to hit any desired city. Or, even a worse possibility, the atomic bomb might be brought in disassembled, but brought into this country in the cities in suitcases. And then it might be assembled and planted in a backyard, maybe in your backyard, or maybe in somebody's cellar and then the saboteur can leave with a time mechanism, cause it to explode the next day, the next week, the next month, or maybe the next year. So you see, Bob, that it's up to you Quiz Kids and also up to the listeners to see that there is absolutely no possibility that there be a war with atomic bombs. We'll have to have international controls which might include inspections to be sure that they're never used. If they were ever used it would just be a world catastrophe.

Joe Kelly: Thank you, Dr. Seaborg. Harvey, let's have your question.

Harvey: Well, Doctor, do you really think they'll be able to harness the energy of the atom and use it for driving ships or planes, in the post World War?

GTS: Well, Harvey, yes. I think they will, not right away but sometime in the future, say ten or 15 years.

Joe Kelly: Richard, how about your question?

Richard: Well, Doctor, how long do you think it will be before some of the more stable elements will be split, that is on a practical scale?

GTS: Oh, on a practical scale. They've been split on a small scale. On a practical scale, I don't know. A new principle would have to be discovered or a new type of reaction. However, it's probably--, I wouldn't say that it would be impossible--maybe in 50 years or so.

Richard: On other thing now. Have there been any other new elements discovered, like plutonium and neptunium?

GTS: Oh yes, Dick. Recently there have been two new elements discovered--elements with atomic number 95 and 96--out at the Metallurgical Laboratory here in Chicago. So now you'll have to tell your teachers to change the 92 elements in your schoolbooks to 96 elements.

Joe Kelly: Pat, do you have another question?

Pat: Uh, yes, how did you discover plutonium and can it still be exploded like U^{235}?

GTS: Yes, plutonium can be exploded about like U^{235}. Plutonium was discovered out at the University of California about five years ago. We used the big cyclotron out there, you've probably heard about that, invented by Professor Lawrence, and bombarded uranium with deuterons and found a radioactivity which behaved different chemically from all the other 93 elements. So we decided that it had atomic number 94. Arthur Wahl and Joseph Kennedy and Edwin McMillan participated in this research and discovery.

Joe Kelly: Sheila, you look like you have another question you'd like to ask.

Sheila: How big is an atomic bomb and how big a piece will it take to blow up this big building that we're in now? (Laughter)

GTS: Well, Sheila, I can't tell you precisely how big an atomic bomb is. I can say though that the Smyth Report gives an upper limit to the amount of U^{235} in the atomic bomb, and that is, a chunk about as big as a football. About the size of the piece that would be needed to blow up this building, well, you have to have a certain size, a critical mass, for it to explode at all. And

GTS: (cont.) that size gives an explosion as big as the one at Hiroshima, which would have, of course, destroyed this building.

Joe Kelly: Richard?

Richard: Do you think, Doctor, that if we keep this atomic bomb secret that it will hold our nation back industrially and scientifically?

GTS: Well, Richard, there are no fundamental secrets about the atomic bomb. Most scientists believe that other countries can have atomic bombs in a few years if they desire them. I'll try to answer your question this way though, if there are restrictions placed upon scientists in this country, so that they can't exchange information and tell each other about what they discover and so forth, definitely it will hold us back scientifically.

Joe Kelly: Harvey?

Harvey: Well, I was wondering, do the people who are working in the scientific research laboratories, were they in great danger of being vaporized at any moment while they are working on the bomb?

GTS: (Laughter) Being vaporized?

Harvey: Yeah, well?

GTS: Well, Harvey, yes, there was some danger of accidents occurring, but I want to say that every precaution was taken in all the laboratories and practically no serious accidents of this, or any other type, took place with the scientists.

Joe Kelly: Bob?

Bob: Well, Doctor, I want to know, back in 1905, Dr. Einstein said that the potential energy of the atom was mass times the velocity of light squared expressed in ergs and do you think that this has yet been fully realized in the atomic bomb?

GTS: No, Bob, not in the bomb that works on the fission of plutonium or U^{235}. Only about 1/10 of a percent of the mass of the plutonium or the U^{235} is changed into energy.

(Bell rings.)

Joe Kelly: Well, I'm just sorry that we didn't have Dr. Seaborg at the microphones the entire half hour. We certainly thank you, Dr. Seaborg, for taking time from your many pressing commitments to be with us tonight.

GTS: Mr. Kelly, it was really a pleasure, and it will have been most worthwhile if, through your splendid radio program, we can encourge all your millions of listeners to think seriously about the dangers of atomic energy, and to do something constructive about it. Of course, speaking for myself, I can say it was most worthwhile to come down here and meet these swell Quiz Kids, who are doing so much to encourage a respect for knowledge and straight thinking in all school children. You have an excellent radio program and you have my sincerest wishes for its continued success.

(Applause)

Joe Kelly: Thank you, Dr. Seaborg. Well, children, Dr. Seaborg's answers were so interesting I almost forgot about the business on hand tonight. First, you each get a $100 Victory Bond from the makers of Alka-Seltzer, and we have a little scoring to do to see which three of you come back again. While that's going on, here's Bob Murphy.

Bob Murphy: Joe, I'd like to remind you listeners to keep on sending in questions for the Quiz Kids. If we use your question on the air, the makers of Alka-Seltzer will send you a certificate good for either a $50 Victory Bond right now or for a Zenith portable radio in the near future. It will be the Zenith trans-oceanic portable with standard broadcasting and five short-wave bands. Just address all your questions to: Quiz Kids, Chicago. Now, Joe, let's have the names of tonight's winning Quiz Kids.

Joe Kelly: Well, the judges say that, as a class, you kids missed two questions tonight and Richard was first, Harvey second, and Pat and Bob tied for third. You four winners will be back at your desks two weeks from tonight. I say that because next Sunday, American Broadcasting Station WFMJ is bringing Bob Burke, Richard Williams, Joel Kupperman, and Judy Graham to Youngstown, Ohio, for a special Victory Bond show. This is Joe Kelly dismissing the Quiz Kids class until the same time next Sunday when we'll broadcast from Youngstown. Good night, kids.

Quiz Kids: (in unison) Good night, Mr. Kelly.

(Applause and music with overdub of: "Listen to the Quiz Kids every Sunday and listen to your old friends, Lum and Abner, every Monday through Thursday. Folks, check your family supply of Alka-Seltzer and remember, when your tablets get down to four, that's the time to buy some more. This is Bob Murphy speaking." Music grows louder. "This is the American Broadcasting Company.")

Afterwards, Helen and I had dinner with Maggie O'Flaherty and others connected with the program at El Gaucho Restaurant on the north side of Chicago.

Prime Minister Attlee of Great Britain and President Truman are continuing to confer. The conferees seem to be feeling that the only safety from the atomic bomb lies in the abolition of war through development of a new League of Nations into an agency capable of preserving peace.

Monday, November 12, 1945

Jeanette Johnson, my sister Jeanette's friend from Ishpeming who now lives in Chicago, called to give me her greetings. She heard me on the Quiz Kids radio show last night.

I sent another memo to J. R. Gibson about my request for a merit increase for Ruth Rogers, my secretary. Gibson indicated in his reply to my memo of November 1 that Mrs. Rogers is not eligible for a merit increase because of an understanding that she would not be considered for further wage adjustment until June, 1946. I suggest that Gibson might have this case confused with some other case since I know of no such understanding. In any event, I state that Ruth Rogers' responsibilities are such and her work is of such high caliber that she definitely must have a merit increase at this time.

Report CS-3312, "Chemistry Division Summary Report for October, 1945" was issued today by the Division Director's Office. All Section C-I information in this report appears in the Perlman-Manning October 19 memo to Hogness (MUC-GTS-2037) summarizing the work for the period September 15 to October 15, 1945.

According to today's paper American and British specialists are in Nagasaki to gain information on the bomb effects. The bomb hit just half way between two ammunition works.

Tuesday, November 13, 1945

I attended the 8:28 a.m. meeting of Groups 1 and 3 in the New Chemistry conference room. Others present were Asprey, Britain, Cunningham, Florin, Ghiorso, Hindman, Hyde, James, Katzin, Magnusson, Manning, Morgan, O'Connor, Osborne, Perlman, Peterson, Sedlet, Simpson, Stewart, R. Thompson, S. G. Thompson, Van Winkle, and Westrum. Osborne reported on the progress made on the protactinium deuteron bombardment. Little has been done with the protactinium fraction since we must wait until the Pa^{232} has decayed out.

Van Winkle reported that he has separated at intervals, two thorium fractions from the uranium fraction, in an attempt to detect the daughter of U^{231} alpha decay. About 60 c/m were observed in the first fraction separated. This has subsequently grown to about 100 c/m, the behavior to be expected from Th^{227}. The second thorium separation was made using a TFA extraction procedure in order to obtain a thin plate. However, no Th^{227} seems to be present in this second fraction. Additional fractions will be removed in an attempt to explain this discrepancy. Preliminary results on the half-life of Pa^{231} indicate a value of about 39,000 years.

Roy Thompson reported that the x-rays associated with the uranium fraction of the protactinium deuteron bombardment (presumably from U^{231}) are still decaying with an approximate five-day half-life. However, some lengthening seems to be occurring in more recent counts.

Hyde discussed the latest results from the U^{233} plus deuteron bombardment, received from Berkeley on November 7. He said that the first solution was first ether extracted to remove the uranium, neptunium, and plutonium from the bulk of the fission activity. The neptunium was then separated using the TTA method of Magnusson. The method worked quite well. Lots of x-rays were found in the neptunium fraction, but nothing with half-life shorter than three or four days. Such isotopes as Np^{231} and Np^{232} were probably already dead. A hard gamma-ray was also found in the neptunium fraction. X-rays were found also in the uranium fraction and exhibited an apparent half-life of about 18 days. These may be from U^{231}, the long half-life observed caused by the contribution of U^{233} x-rays. A protactinium fraction was removed from the original salt layer to look for daughters of neptunium alpha decay. X-rays and alpha activity (mostly U^{233}) were found in the protactinium fraction. In attempts to purify further the fraction chemically, most of the activity was lost, but a 30-hour x-ray activity remains. Perhaps this is associated with Pa^{229} that would result from alpha decay of Np^{233}. Too few alpha-particle counts remain in the protactinium fraction to permit pulse analyses to be made. No alpha-particle can be definitely attributed to the neptunium fraction. The plutonium fraction has not yet been studied.

James reported on the Np^{237} plus deuteron bombardment and said that interest currently centers on the 40-day x-ray activity in the plutonium fraction. While this activity has generally been attributed to Pu^{237}, there is no evidence for a new Pu^{237} alpha activity. He speculated that these alpha particles may have the same range as Pu^{236} or Pu^{238} and are masked in their peaks. Florin suggested that the x-rays might be due to Pu^{235} rather than Pu^{237}.

Stan Thompson discussed his recent work on the chemistry of element 95 and element 96. Using a nitrate fusion method of oxidation followed by leaching with an acetate buffer (which dissolves the elements in the +3 state and leave behind those which have been oxidized to the +4 state), he found that nearly three-fourths of the total activity remained in the residue. However, it was determined that the ratio of 95 and 96 in the residue definitely changed. Thompson plans to try to obtain the +2 state of 95 or 96 by using samarous sulfate precipitation.

Cunningham reported that the sulfide of element 95 has been prepared using CS_2 and H_2S at 1,000°C. Element 95 dioxide has also been reduced using hydrogen at 1,000°C. Zachariasen is analyzing the samples, and additional experiments will be performed this week.

Westrum discussed his latest specific activity measurements on plutonium. This involves the measurement of the percent increase in specific activity due to Pu^{240} in plutonium samples of varying pile irradiation gt levels. The most accurate current specific activity value for Pu^{239} is 70,660 ± 35 disintegrations per minute per microgram. I suggested that the half-life of Pu^{240} might be calculated from the specific activity increase of the 171 and 240 gt materials. Using this value for the half-life, he should be able to calculate a cross section for the destruction of Pu^{240} from the specific activity of 800 and 1300 gt material.

Peterson reported that he is having difficulties with his radium plus neutron bombardment experiments. There seems to be an unexpected amount of beta-particle activity present in the actinium fraction. The radium seems to follow the actinium through the ion exchange column in larger concentrations than has been expected.

Groves issued a directive today which removes the names of Compton and Hilberry and substitutes the name of Daniels (Director of the Metallurgical Laboratory) as the authorizing signatory under the existing "Rules of Interchange for Information between Chicago and Site Y" (dated January 20, 1945). Groves also substituted the name of Dr. Norris E. Bradbury (the new Director) for the name of Dr. Oppenheimer at Site Y. The "Rules for Interchange" are to be amended to state "it is the responsibility of the two Directors, Dr. F. Daniels and Dr. N. E. Bradbury, to see that these instructions are enforced."

In a memo to Hogness, Wayne Johnson, Director of Personnel, presents the following data with regard to the age distribution of scientific men working at the Metallurgical Laboratory:

Age Range	Number of Employees	Percent of Total
20-25	183	21.5
25-30	324	38.1
30-35	170	20.0
25-40	88	10.4
40 and over	85	10.0

Helen and I were invited out to dinner.

Cordell Hull has been awarded the 1945 Nobel Peace Prize.

Prime Minister Clement Attlee spoke before a joint session of Congress today.

Wednesday, November 14, 1945

The Met Lab was to start on a 44-hour week instead of the 48-hour week today. This arrangement has been postponed and will probably go into effect near the end of the month.

I received a call from Hamilton who reported on the status of bombardment 13a (Np^{237} plus helium ions). It has a total of 50 microampere-hours. The Np^{237} is on an interceptor target, which is placed in front of a back-up target containing depleted uranium, that has received 150 microampere-hours. He will bombard the Np^{237} until it has received 150 microampere-hours.

We discussed in some detail the talk on heavy isotopes that he is going to give tomorrow. I told him, in particular, some of the latest results that we have obtained on the decay products in the 4n + 1 series, namely, that we think we have found the isotope 89^{225} decaying by alpha-particle emission to the isotope 87^{221} as part of a decay chain. I then told Hamilton the content of the talk that I am planning to give at Northwestern University at the ACS Symposium this Friday.

He then said that the target of bombardment 13a (Np^{237} plus helium ions) may leave Berkeley somewhere between noon and 2:00 p.m. on next Monday. He will then start the bombardment of Pa^{231} with helium ions; he will use U^{235} as a larger backing target.

Sidney Fox of Iowa State College (a fellow chemistry student when I attended UCLA) commented, in a letter dated November 12, on my Quiz Kids program appearance. He expresses concern over the present public apathy about science and the national atomic energy program. He asks if I would be interested in other conceivable plans of action which have occurred to him as offering hope of being more effective than writing letters to Congressmen.

I received a letter from J. H. Owen of Miami, Florida, who heard the Quiz Kids program presentation. He asks for any information or suggestions I might have that would assist him in planning an appropriate college curriculum to lead to a Masters Degree in chemical engineering relating it to the atomic field.

Herb Hyman called to give me details on a meeting of the Education for Democracy group to be held at Mrs. Buchbinder's, 70 East Walton at 2:30 p.m.

Manning wrote to Hamilton that the radiophosphorous sample which we received from Berkeley was analyzed on November 12 at 4:30 p.m. and was found to have 0.623 millicurie of activity. This value is in fair agreement with Hamilton's value when an extrapolation is made back to the time the sample was analyzed in California.

According to this morning's paper, air service from Chicago direct to Europe will start next Monday and will be a weekly run.

Thursday, November 15, 1945

A meeting of the Solvent Extraction Group of Section C-I was held this morning in the New Chemistry conference room. In attendance were Ader, Blaedel, Goeckermann, Hyman, Kelley, Lawroski, Manning, Perlman, Post, Schaffner, and Schraidt. Blaedel reported that data obtained from the last of two sets of runs are considered sufficient to prove definitely the feasibility of the Redox Process. Greater than 98 percent recovery of plutonium, with decontamination factors greater than 10^7, has been attained. Over 99.5 percent recovery of uranium, with a decontamination factor of greater than 10^4, has also been achieved.

I attended a meeting on Pile Evaluation held at the Metallurgical Laboratory today. Nordheim spoke about the importance of breeder reactors in meeting the future pile requirements of the U.S. Fermi also underlined the future significance of breeder reactors. He said that he thinks many 1,000 kw reactors would be better to have than a few very large reactors. He thinks that thermal reactors will be better for the production of electric power while fast breeders may be best for the production of plutonium.

Zinn gave a description of power reactors and breeder reactors. He included among the latter, fast breeder reactors with metal coolants such as bismuth, mercury, and lead. Zinn pointed out that these have the highest fast neutron absorption cross section and indicated that perhaps sodium or sodium potassium alloy might be the best possibilities for use as a coolant. He sketched a 2,000 kw unit that might start with U^{235} but eventually switch over to Pu^{239}.

Willard described a high temperature pile in which beryllium oxide would be mixed with fissionable material in a steam-cooled system surrounded by thorium rods.

Weinberg talked about the heavy-water cooled reactor working on the thorium-U^{233} cycle.

Fermi indicated that he thinks that the decisions as to the best course of action should be delayed. He said that the possibility of generating electricity by nuclear power looks good and that breeding is clearly a possibility. He believes that the production of radioactive substances will be one of the most important things; this will be so even if nuclear power becomes an important source of energy. He pointed out that biological tracers such as C^{14} will be very important and should have free distribution. He considers this as a duty of this project to science and this nation. He suggested that an announcement should be made very soon that such isotopes will be available.

I received a letter dated November 9 from J. H. Peterson of the du Pont Company of Wilmington. He said that, prior to the war, the Chemical Department of du Pont was considering some relatively simple systems with tracer elements. The Department would now like to resume actively this program. However, for preliminary work, he thinks it would be preferable to purchase the tracers. He asks whether I know if radioactive tracers are available commercially or whether such production is contemplated. Peterson is also interested in obtaining an updated "Table of Isotopes."

In a letter dated November 12, Gordon Leader states that he will report for work at the Met Lab on November 30. He expressed his gratitude for the opportunity to return to the Met Lab.

A letter also arrived from Bob Connick. He encloses a brief summary of the last month's work in Berkeley. He then mentions that he received from the Area Engineer's Office a brass holder with a small platinum dish on it which held several thousand c/m of plutonium. He asks if I know anything about it--neither he nor Joe Hamilton do.

I sent Mr. Doyle Getter of The Milwaukee Journal, at his request, a copy of my speech for tomorrow, along with a photograph of me, some biographical information, and a copy of the University of Chicago press release. I mention that I still have relatives residing in Michigan.

British paratroopers restored order in Tel Aviv by opening fire on a huge crowd of Jewish civilians killing and injuring 57. The last time British soldiers fired on Palestine citizens was in 1939.

Friday, November 16, 1945

I participated in the Fiftieth Anniversary Technical Conference of the Chicago Section of the American Chemical Society which was held in the chemistry auditorium of Northwestern University. See Figures 15 and 16. I was the first speaker on the program at 10:30 a.m. I presented my talk entitled, "The Chemical and Radioactive Properties of the Heavy Elements." In the talk I announced the discovery of elements 95 and 96, my concept that the heaviest elements fit into the Periodic Table as an "actinide series," and a number of other interesting results of our research at the Metallurgical Laboratory during the war, such as the development of the plutonium separation process and the discovery of plutonium in nature. The abstract for my talk is as follows:

> The discussion will be concerned with a review of the chemical properties of the heavy elements (atomic number greater than 88) and especially with the newly discovered transuranium elements. It will be shown that the present state of knowledge of the chemical properties of these elements is so far advanced that it is now possible to make quite definite statements concerning their atomic structure. The discovery of a number of isotopes of the transuranium elements will be described and some of the radioactive properties of these isotopes will be given. A brief general description will be given of the chemical separation processes which are being used for the purification and isolation of plutonium (element 94) in connection with the huge plants which were developed for the manufacture of this synthetic element.

Hamilton's log for bombardments 8b (U^{233} plus deuterons), 12b (Pa^{231} and deuterons), and 12b (Np^{237} plus deuterons) arrived today. Hamilton said he will send the interceptor target holders as soon as they are completed.

"Techniques for the Preparation of Thin Films of Radioactive Material" by D. L. Hufford and B. F. Scott was issued today.

Today's paper, as the newspaper does every day now, carries a story of the bitter fighting in Java between Indonesian and British troops.

Saturday, November 17, 1945

The huge Krupp munitions empire was seized by the British today; these plants are in the British occupation zone.

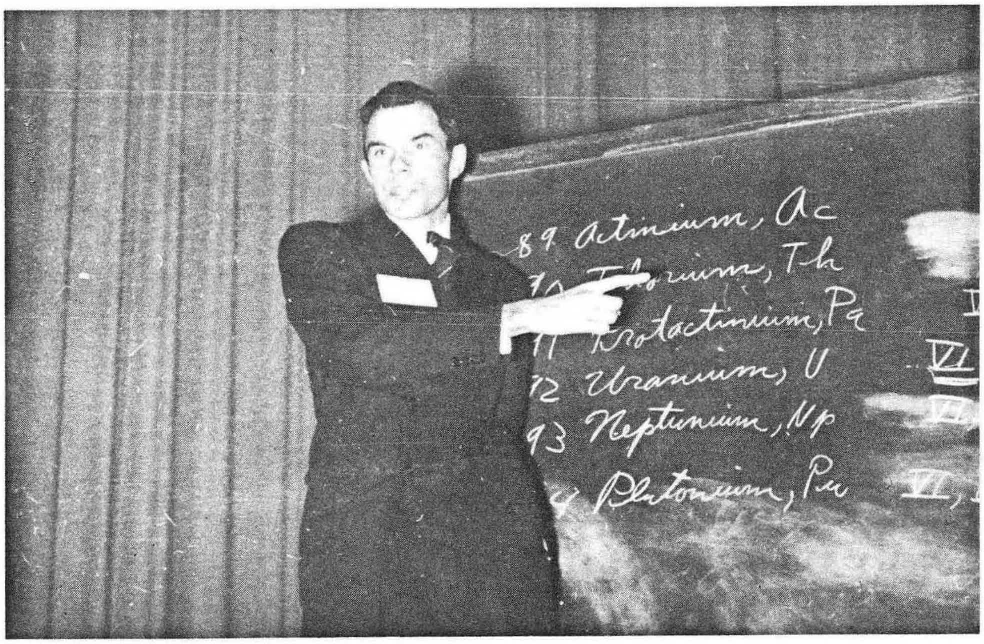

Figure 15. Glenn Seaborg at blackboard, Northwestern University, November 16, 1945.

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Figure 16. Pierce Selwood and Glenn Seaborg at Northwestern University, November 16, 1945.

Sunday, November 18, 1945

I played 18 holes of golf at the Evergreen Golf Club with Darrell Osborne and Stan Thompson. Our scores were DO-171, ST-101, and GS-111.

There are reports of two days of fighting in northern Iran. Members of the Separatist Democratic Party are the insurgents.

Monday, November 19, 1945

I attended the 9:00 a.m. meeting of the Laboratory Council in Room 209, Eckhart Hall. Others present were Branch, Cole, Dana, Daniels, Dempster, Foote, Furney, Fussler, Hilberry, Hughes, Jacobson, Johnson, Lapp, Metcalf, Moulton, Colonel Nichols, Nickson, Ohlinger, Willard, Young, Zachariasen, and Zinn. Daniels announced that on November 26 the Laboratory will decrease its standard work week from 48 to 44 hours. There will be no change in the pay for monthly-rated employees. A 5 percent increase in base pay and time and a half for overtime beyond 40 hours will be given to the hourly-rated personnel. He said that land may be purchased at Argonne for the relocation of the Met Lab, but the Lab will stay in its present site for at least a few years. Daniels also described a number of plans for piles in the future, such as the one at Clinton, a fast neutron pile, and a high temperature pile. He also mentioned a fast neutron pile for Los Alamos.

Colonel Nichols described plans for university research institutes and indicated that Argonne would be run by a board of governors composed of midwest people and that the same would be true of the other government labs. He believes that the research carried on at universities supported by the Manhattan District would be nonsecret but there would, of course, be some secrecy covering the work at the government labs.

Daniels emphasized that we should be in a position to encourage people to stay on at the Met Lab (that is, Argonne Lab) past next July 1 if that is their wish.

Daniels also described the program of future Lab Information Meetings. He said that there will be an Information Meeting covering the health area on December 10, one in physics on December 11, and one on chemistry on December 12. He said that we should submit a list by the end of this week of people whom we would like to attend so that their names can be put on the program.

Willard announced that the following programs will be rotated between physics, health, and chemistry. There will be seminars every other week, beginning Monday, November 26. I will arrange the first program.

It was announced that there is a good possibility that we will get copies of German papers covering work that the Germans did in the nuclear field during the war.

It was also agreed that those scientists concerned with the social implications of nuclear energy should do their speech making or political work entirely on their own time and own expense.

Metcalf announced that he and Johnson are leaving the patent group and that Lieutenant Chisolm will be in charge of this group with Lieutenant Hawkins as his assistant. General Groves has authorized that this group will soon be placed under civilian leadership.

Hamilton called me from Berkeley to discuss the pulse analyzer apparatus being built for Ghiorso in the Berkeley laboratory electronics shop. We also discussed some of the details of shipping the bombarded samples from Berkeley to Chicago. It will not be possible to use the Army Transport Command for future shipments because there will be no stop in Chicago. We therefore decided that shipments will be made on the "City of San Francisco" or by commercial airline.

Hamilton told me that Cattell of Science magazine has wired E. O. Lawrence, asking for a short write-up on elements 95 and 96, apparently as a result of my announcement during the ACS Symposium at Northwestern University last Friday. We will prepare a short paper for this purpose.

I received a reply dated November 15 from Bruno Pontecorvo, of the National Research Council in Montreal. Pontecorvo says he is very interested in joining the future staff at Berkeley but that his plans for the future are not yet clear. He suggests we discuss the matter further during my forthcoming visit to Canada. In a postscript he mentions he has sent copies of our correspondence to Segrè.

Elliott L. Abers, Chairman of the Committee on Lectures, Purdue University, wrote to ask me to speak at the local chapter of Phi Lambda Upsilon. Abers recalls that I once showed him, a UCLA graduate, around the Department of Chemistry at Berkeley.

I sent Peggy McLaughlin in Chicago information on plutonium for her science project.

This morning's newspaper carries an item stating that Colonel Charles A. Lindbergh asserted that development of the atomic bomb has made imperative a world organization for control of destructive forces.

Tuesday, November 20, 1945

I attended an 8:28 a.m. meeting of Groups 1 and 3 of Section C-I in the New Chemistry conference room. Also present were Asprey, Britain, Cunningham, Florin, Ghiorso, Hagemann, Hindman, Hopkins, Hyde, Jaffey, James, Katzin, Kohman, Magnusson, Manning, O'Connor, Osborne, Peterson, Robinson, Sedlet, Simpson, Stewart, R. Thompson, S. Thompson, Van Winkle, and Westrum. I reported that ten grams of thorium were bombarded with neutrons in the Hanford pile for 16 days and then flown to Chicago. The purpose was to extract the uranium as soon as possible after the pile exposure and determine the ratio of U^{233} to U^{234} from which the capture cross section of Pa^{233} can be calculated. Hagemann said the thorium metal was dissolved in HNO_3 and the solution salted with $Ca(NO_3)_2$ from which the uranium was extracted with ether. The yield was 310 micrograms if it is assumed to be pure U^{233}. Samples of this material will be made to run pulse analysis, fission, and mass spectrometry measurements.

James reported on the plans for the Np^{237} plus helium ions target scheduled to arrive this week. The first and immediate thing required is to remove the 95 by LaF_3 precipitation from a solution in which the neptunium and plutonium have been oxidized, as it is expected that the shortest-lived products will be in this 95 fraction. A small part of this fraction will be analyzed by pulse amplitude and absorption curve techniques, a larger portion will be decontaminated from rare earth elements using fluosilicate cycles, and another portion will be retained to be milked for plutonium at intervals. The neptunium and plutonium fractions will be decontaminated by ether extraction after eliminating the fluoride, and then the neptunium and plutonium will be separated by extraction of the neptunium into benzene with TTA. The neptunium fraction will be carefully examined as it should contain Np^{236} without any Np^{238}.

Manning presented his methods and calculations for determining the absolute cross sections of some of the charged particle reactions in an attempt to determine the source of the 40-day x-rays found in the Np^{237} plus deuteron target.

Asprey and Cunningham reported that the effort to milk 20 µg of 95^{241} from plutonium for shipment to Berkeley is proceeding well. About 1 mg will be needed to establish the hyperfine structure for determining nuclear spin. However, Cunningham is working on a method that may require less material.

Van Winkle reported on the protactinium plus deuteron target work. The growth of Th^{227} is being followed and should level off in another three or so days. He will look for RdAc from U^{231} in subsequent milkings. Roy Thompson reported that there is definite evidence for the presence of Pa^{229}. From the expected yield of Pa^{229} and the observed amount of Ac^{225} present, it is estimated that the alpha-particle branching of Pa^{229} is less than 1 percent.

I announced that we expect to get a 150 g button of 350 gt plutonium and I would like Simpson to fractionate off the 95 if it is possible. I then reviewed our plans for future neutron bombardments at Hanford and said that the following materials will be sent to Hanford: (1) Farmer's plutonium sample no. 4, which arrived today, (2) about 10 mg of Np^{237}, (3) 5 mg of U^{233} that is to be extracted from our decaying Pa^{233}, (4) 0.1-1 gm radium for the production of actinium, and (5) a quantity of U^{238}.

I said that Daniels is reasonably confident that the Met Lab will continue after July 1, but the work that will be done and its relationship to the Nuclear Institute at the University of Chicago are not clear. I informed the group that Manning, Perlman, and I "will all be away for a while" (at Berkeley) in early December. Cunningham and Stan Thompson will handle the administrative duties for the section at that time.

Thompson reported on some experiments done to see if there is a divalent state of element 95. A sulfate solution of 95 and 96 tracers was shaken with sodium amalgam in presence of carriers and gave evidence that 95 may have been reduced to the +2 state.

George Weil, our liaison representative at Montreal, called me at 9:10 a.m. to say that the Canadian atomic energy project scientists have requested that Perlman and I visit Canada during the first week in December. I told him that I could not make it before the third week in December and suggested the week of December 17 as a possibility.

Weil called back at 9:30 a.m. and said that the week of December 17 will be acceptable. I said that we will try to arrange our schedule in order to be in Ottawa on December 18.

Jerry Howland, in a letter dated November 19, says that he has heard through friends that my comments on the Quiz Kids program were well received. He points out that I received a two-column headline in the November 17 issue of the New York Times on the first page of the second section. He says many scientists at Monsanto are very concerned over the formation of plans for the future of nuclear research, and he hopes I will not turn down any opportunity to appear on the radio or in the news reels as publicity is needed in order to get government support. He goes on to say that recently 37 Monsanto scientists issued a statement to the press and in the form of a letter to Congressmen and Cabinet members. The views of these scientists are in agreement with the general views of the Oak Ridge, Chicago, and Los Alamos associations of atomic scientists about the lifting of secrecy and pushing for the development of a strong international organization "to handle all international problems."

Howland says that on November 14 the Dayton Association of Atomic Scientists was organized and a delegate was elected who attended the Federation meeting in Washington.

Howland also reports that Lum is being removed from his position in Central Research Unit 1 and will become an assistant to Thomas.

Ames and Kohman have devised a tentative procedure for radium analysis in the Mallinckrodt Process when solutions contain an excess of barium and negligible amounts of H_2SO_4. This tentative procedure, described in MUC-GTS-2070, is to be used as soon as possible after the process filtration and should routinely take only about 25 minutes to perform. The report will be sent to Mallinckrodt to supplement the rapid approximate radium analysis procedure previously described in MUC-GTS-2063.

President Truman proposed compulsory health insurance in a nationwide federally subsidized medical care system.

<u>Wednesday, November 21, 1945</u>

Hamilton phoned to inform me that the Np^{237} plus helium ions target has received a total exposure of about 115 microampere-hours and is scheduled to arrive in Chicago today. I asked that he give equivalent total helium ion bombardment exposure to the Pa^{231} and U^{235} targets to be sent to us next.

We discussed the content of our short writeup for publication in <u>Science</u> magazine, my forthcoming trip to Berkeley, and my reservations on the "City of San Francisco."

I received a telegram from the Honorable Helen Gahagan Douglas, from California, House member of the 79th Congress, First Session, stating that she expects to introduce the following resolution in Congress this Friday, November 23:

> Whereas in the atomic bomb there has been unleashed the most destructive force known to mankind, in the production of which the United States can have no enduring monopoly and against which there is no specific military defense; and
>
> Whereas any effort to pursue a foreign policy based on our temporary superiority in atomic weapons will force other nations as well as ourselves into an atomic armaments race, thereby creating a world of fear and hatred in which nations great and small face sudden destruction; and
>
> Whereas the fundamental aim of United States foreign policy is to have world peace--an aim which cannot be achieved by the United States alone or by any other country by itself, but only through maintaining and strengthening the cooperation and unity of the peace-loving peoples and nations of the world; and

Whereas the United Nations organization cannot work unless the United States, Great Britain, and the Soviet Union work cooperatively on all major issues: therefore be it resolved

That prior to any attempt to specify international machinery for dealing with atomic energy, the President of the United States immediately invite to a conference the governments of Great Britain and the Soviet Union, in order to discuss the common danger created by atomic weapons, and to plan for the joint approach by these three nations to the other members of the United Nations organization for establishing a world-wide system of international cooperation and control of atomic energy to the end of preventing a competitive armaments race and safeguarding peace.

Douglas asked for my endorsement.

In answer to Everson's letter of November 15, I sent him the following tabulation of personnel information on the individuals being considered for transfer to the Berkeley Radiation Laboratory.

Name	Point of Origin	Education		Present Classification and Salary		Estimated Berkeley Equivalent
Blaedel, W.	Chicago	BA MA PhD	1938 1939 1941	Associate Chemist	\$355[1]	P4 368-396
Cunningham, B.	Berkeley	BS PhD	1935 1940	Chemist	416[2]	P5 396-442
Ghiorso, A.	Berkeley	BS	1937	Chemist	385	P5 396-442
Goeckermann, R.	Madison, Wisconsin	BS	1944	Junior Chemist	230	P2 256-307
James, R.	Berkeley	BS	1942	Associate Chemist	300	P3 307-368
Knox, W.	Berkeley	BS	1942	Associate Chemist	300[3]	P3 307-368
Magnusson, L.	Huntsville, Alabama	BS	1941	Associate Chemist	300	P3 307-368
Miller, D.	Madison, Wisconsin	BS MS	1941 1943	Associate Chemist	325[4]	P3 307-368
O'Connor, P.	Madison, Wisconsin	BA MS	1942 1944	Junior Chemist	268[5]	P2 256-307

continued...

Name	Point of Origin	Education	Present Classification and Salary	Estimated Berkeley Equivalent
Seaborg, G.	Berkeley	BA 1934 PhD 1937	Principal Chemist $650	
Thompson, R.	Austin, Texas	BA 1940 MA 1942 PhD 1944	Associate Chemist 315[6]	P4 368-396
Thompson, S.	Berkeley	BA 1934	Senior Chemist 425[7]	P6 442-547
Westrum, E.	Berkeley	BA 1940 PhD 1944	Associate Chemist 305[8]	P4 368-396

[1]Chemist $385 (1/1/46)
[2]Senior Chemist $450 (1/1/46)
[3](Clinton)
[4](Clinton)
[5]Associate Chemist $285 (1/1/46)
[6]Chemist $350 (1/1/46)
[7]Senior Chemist $450 (1/1/46)
[8]Chemist $350 (1/1/46)

I suggest that Everson and I discuss the transfer dates for these individuals during my trip to Berkeley next month, but I say that, at present, it does not seem probable that any of these persons would transfer before January 1, 1946. However, it is planned that all those going will have made the move before March of 1946.

Kohman sent a letter to the attention of G. L. Martin at Mallinckrodt summarizing the analytical results of the radium concentration obtained in the experiments Ames and Kohman performed in St. Louis on November 5-7. He also includes copies of the alternate procedure, MUC-GTS-2070, for radium analysis with uranium extraction process solutions containing excess barium and negligible amounts of H_2SO_4.

Kohman mentions problem areas that Mallinckrodt may wish to study in the future. He suggests that undesired alpha activities might be removed from radium solutions by using a solvent extraction procedure involving hexone and 6 - 10 N HCl.

I wrote Hamilton to inform him that two lightweight shields with 1½" lead walls are being sent to him by courier today. I say that we believe these shields may be suitable for handling interceptor targets that are not excessively radioactive.

Manning and Perlman prepared and sent to Hogness a summary of the work of Section C-I for the period October 15 to November 15, 1945. It contains the following information:

I. Heavy Isotope Work

A. Transmutation of Radium to Actinium

One mg of Ra^{226} was bombarded with neutrons in the Clinton pile for a period of about two weeks. It was anticipated that Ra^{226} would capture neutrons to produce a new isotope of radium, Ra^{227}, which should be a short-lived beta-particle emitter, resulting in the formation of 13.5 year Ac^{227}. Actinium-227 was, in fact, observed after the bombardment, in an amount corresponding to a pile neutron capture cross section of the order of 10 to 12 barns for Ra^{226}. The actinium was identified after separation from the radium by means of IR-1 adsorption columns.

B. Deuteron Bombardment of Np^{237}

A 14 mg sample of neptunium was bombarded for 483 microampere-hours with 22 Mev deuterons in the Berkeley 60-inch cyclotron. Evidence was found for the following reactions:

1. $Np^{237}(d,p)Np^{238} \xrightarrow[2.0 \text{ days}]{\beta} Pu^{238}$

2. $Np^{237}(d,n)Pu^{238}$

3. $Np^{237}(d,3n)Pu^{236}$

In addition to the above reactions there was evidence for another reaction; an x-ray activity decaying with a half-life of about 40 days was found in the plutonium fraction. This same activity had been found earlier in the plutonium fraction from helium ion bombardments of U^{235} and natural uranium. The most probable assignment for the 40-day activity is to Pu^{237} or Pu^{235}, decaying by orbital electron capture to Np^{237} or Np^{235}, respectively. The yield of the x-ray emitting isotope, appears to be more nearly that which would be expected from a d,4n reaction on the original Np^{237}, yielding Pu^{235}, than from a d,2n reaction which would yield Pu^{237}. If the 40-day activity is assigned to Pu^{235}, it appears necessary to assume that Pu^{237}, which must have been formed in considerable quantities by a d,2n reaction during the bombardment, either is very short-lived (less than 6 hours half-life) or that it is long-lived (at least several years half-life for orbital electron capture). A pulse analysis of the alpha particles in the plutonium fraction from this bombardment showed peaks at ranges corresponding to Pu^{238} and Pu^{236}. Alpha particles from Pu^{235} and Pu^{237} were either not present in detectable amounts or had ranges very close to those of Pu^{236} or Pu^{238} and hence were concealed in these peaks.

C. Deuteron Bombardment of Pa^{231}, Production of a New Uranium Isotope (U^{231})

A sample consisting of 2 mg of protactinium was bombarded for 452 microampere-hours with 22 Mev deuterons in the Berkeley cyclotron. Evidence was found for the following reactions:

1. $Pa^{231}(d,p)Pa^{232} \xrightarrow[\text{1.6 days}]{\beta} U^{232}$
2. $Pa^{231}(d,n)U^{232}$
3. $Pa^{231}(d,2n)U^{231}$
4. $Pa^{231}(d,3n)U^{230}$
5. $Pa^{231}(d,p2n)Pa^{230}$

The new isotope, U^{231}, formed by the d,2n reaction, decays by orbital electron capture with a 4½ to 5 day half-life as evidenced by the presence of a 4½ to 5 day x-ray activity in the uranium fraction from this bombardment. The estimated yield of U^{231} was approximately that to be expected from a d,2n reaction. The assignment of the 4½ to 5 day activity to U^{231} was based in part on the observed yield and in part on the tentative identification as Th^{227} of an alpha activity which was periodically removed from the uranium fraction. The Th^{227} would be formed as a result of a branching decay of U^{231} by alpha emission. The amount of activity extracted periodically from the uranium fraction decreased with time at a rate roughly corresponding to the decay of the uranium x-ray activity. The amount of Th^{227} recovered corresponded to a branching-ratio of the order of 0.01 percent for the U^{231} alpha decay.

Uranium-230 was formed in high yield by a d,3n reaction on Pa^{231}, as evidenced by the fact that nearly all of the alpha activity in the uranium fraction was due to U^{230} and its daughters.

Protactinium-230 was identified in the protactinium fraction by the observed growth of the alpha activities characteristic of the U^{230} series.

No direct evidence was found for any activity which could be assigned to U^{229}, which was presumably formed as a result of a d,4n reaction on Pa^{231}. A protactinium fraction was removed from the purified uranium fraction about three days after the end of bombardment and two days after separation of the uranium, but no evidence was found for the 31-hour Pa^{229} alpha activity. Protactinium-229 would be formed by orbital electron capture decay of U^{229}. This indicates that U^{229} probably has a half-life shorter than twelve hours. There was evidence for the presence of Pa^{229} in the original protactinium fraction after the bombardment. This might have been formed either as a result of rapid decay of U^{229} or directly by the reaction $Pa^{231}(d,p3n)Pa^{229}$. The Pa^{229} was identified by the detection of daughter members of the 4n + 1 series.

II. Chemistry of Elements 95-96

A. Decontamination of 95-96

Because of the close similarity in chemical properties of the rare earths and elements 95 and 96, the major difficulties in decontamination of the latter are encountered in the separation of the radioactive rare earth elements. The most useful method obtained so far consists of precipitation reactions from fluosilicate solutions. The solution is made 3 M in H_2SiF_6, 5 mg Ce(III)/ml is added and the solution is allowed to stand for one hour. Cerium precipitates carrying with it about 90 percent of the rare earth activity and 5 to 20 percent of the 95-96. The 95 and 96 in the supernatant solution are then removed by making the solution 3 to 6 M in HF and adding more cerium carrier. A series of these cycles is effective in completely removing rare earths. Before the rare earth separation zirconium is removed by ceric iodate precipitation and the 95-96 are separated from barium by hydroxide precipitation.

Elements 95 and 96 present in plutonium that had been subjected to prolonged neutron bombardment at Hanford were decontaminated from fission product activity by a factor of about 10^8. The remaining activity amounted to 1 beta disintegration per 100 alpha disintegrations of elements 95-96. The 95-96 yield in this case was fairly low. Another sample of 95-96 was decontaminated by a factor 10^5 with 50 percent yield.

B. Attempted Oxidation of Elements 95-96

Rare earths such as terbium, cerium, and praseodymium can be oxidized above their trivalent state by fusion with sodium nitrate. Trivalent rare earths may then be leached out with an acetate buffer at about pH 5. In three experiments, mixtures of elements 95-96 (95^{241}-96^{242}) along with macroscopic quantities of cerium, praseodymium, and terbium, respectively, were fused with sodium nitrate, lanthanum being present in all cases. The ratio of 95-96 activities in the initial material was 1.13. After fusion and leaching, the ratio of these elements in the terbium oxide was 2.0. In the other two samples a lesser enrichment of 95 took place. These experiments indicate that element 95 can be oxidized to a higher than trivalent state and that 96 is oxidized to a lesser extent or not at all.

C. Attempted Reduction of Elements 95-96 by Sodium Amalgam

The rare earths europium and samarium are reduced in acetic acid solution by sodium amalgam and enter the amalgam. Upon washing the amalgam with 2 M HCl the rare earths are removed. An experiment of this type was tried with 95-96 (95^{241}-96^{242})

activities present and some enrichment of 95 was observed in the mercury. However, the activity did not come out of the mercury as readily as samarium which was also present in the starting solution.

When the sodium amalgam reduction is made in H_2SO_4 solution, the rare earths susceptible to reduction do not enter the amalgam but are precipitated as insoluble divalent sulfates. With a mixture of samarous and europous sulfates, the samarium can be removed by washing the precipitate with cold dilute HNO_3. When a mixture of 95 and 96 (95^{241}-96^{242}) was incorporated into an experiment of this type, a part of the activity precipitated with the sulfates and almost all of this was removed in the nitric acid wash. The activity so recovered showed an enrichment of element 95 with respect to 96.

These reduction experiments may be taken as evidence that element 95 possesses a lower than trivalent oxidation state and that element 96 cannot be reduced or only does so with greater difficulty.

D. Separation of 95-96 by Fractionation of the Oxides

Lanthanum oxide containing 95 and 96 oxides was heated in a fractionating crucible described in previous reports. The oxides were heated in a vacuum in which oxygen partial pressure was estimated to be of the order of 10^{-5} mm and the volatilized oxides were caught on plates at graded temperatures. The hottest plate at which active material was found was at 2,000°C and the coldest at 750°C. The maximum deposit was found on the 1,900°C plate. A considerable enrichment of the 95 was found progressively on the colder plates. Whereas the starting material contained about 11 percent by activity of 95, the 1,900°C plate showed somewhat of a depletion of 95. The 1,850°, 1,800° and 1,750° plates showed progressive enrichment of element 95 of 17 percent, 49 percent, and 78 percent, respectively.

This experiment definitely proves that the activities ascribed to 95^{241} and 96^{242} do belong to different elements. The separations obtained by the methods described in the previous sections of this report were not as clear cut as in this experiment.

E. Chloride and Oxide of Element 95

Eight micrograms of 95^{241} were precipitated as the hydroxide, dried, and treated with CCl_4 at 750°C. A light yellow sublimate was obtained and its x-ray pattern was examined by Zachariasen. The pattern was poor but could nevertheless probably be attributed to the trichloride, the structure being typical of the trivalent rare earth, plutonium, and neptunium chlorides. A revised, but perhaps still inaccurate, value for the melting point was obtained. The previous value of <700°C has been redetermined as 730°C.

A sample of 95^{241} nitrate was ignited in a capillary tube and the x-ray pattern found was that of a cubic fluorite lattice consistent with a formula of 95-dioxide. The dioxide was treated with hydrogen at 930°C and a new unidentified phase appeared. This new phase was in turn treated with CS_2 and H_2S at 1,000°C and still another unidentified phase appeared. The conversion was not complete in the latter case.

III. Chemistry of Neptunium

A. Tetrafluoride of Neptunium

Upon treating NpO_2 with HF containing 5-10 percent H_2 at 500-550°C, NpF_4 is formed. It has previously been found that under the same conditions PuO_2 is converted to the trifluoride. It was also shown previously that NpO_2 can be converted to a trifluoride by HF containing a higher proportion of hydrogen, e.g., a 50-50 HF-H_2 mixture.

B. Separation of Plutonium and Neptunium by TTA Extractions

It has been found by the California group that TTA is a very effective complexing agent for Pu(IV). Under conditions of proper activity, plutonium(IV) may be quantitatively extracted as a TTA complex from aqueous solution into benzene. Plutonium(III), on the other hand, does not form an extractable TTA complex.

It has now been found that neptunium in the tetravalent state also forms an extractable complex with TTA. This behavior forms the basis for a convenient separation of plutonium and neptunium in which neptunium(IV) is extracted into benzene by TTA while plutonium is maintained in the aqueous layer in the non-extractable III state.

The following procedure is used: A mixture of plutonium and neptunium in 5 M HCl containing 0.1 M KI and 0.1 M hydrazine hydrochloride is heated at 100°C. It is then diluted tenfold and again raised to 100°C. This treatment has been found necessary to reduce the neptunium to the quadrivalent and the plutonium to the trivalent states. In the presence of TTA virtually all of the neptunium can be extracted into the benzene, removing only about 0.3 percent of the plutonium. The mechanisms of the reactions are being determined.

IV. Present Status of the Redox Solvent Extraction Process

The feasibility of the Redox Process has now been quite conclusively demonstrated by the results of 28 runs through the first cycle and of 3 recently completed runs through both the first and second cycles. Employing the small pilot plant columns, the actual results obtained show that the process, even in its present stage, can give at least a 98 percent yield of plutonium which has been decontaminated by a factor of 10^6 to 10^7 with respect to uranium (equivalent to 99.9 percent purity for plutonium at 250 gt level) and decontaminated by a factor of 10^7 with respect to both beta and gamma radiation. The plutonium is obtained in a 0.5 N HNO_3 solution, the volume of which is 1/50 of the volume of starting feed solution so that the plutonium from one ton of uranium would be finally contained in 10 gallons of solution. Evaporation tests show that there is little or no contamination of this final solution by extraneous introduction of foreign materials during processing in the pilot plant, which is constructed of pyrex and 18-8 stainless steel.

The process recovers approximately 99.8 percent of the uranium, in the form of a 30 percent UNH solution in dilute nitric acid. The uranium is decontaminated by a factor of approximately 10^4 or better with respect to both betas and gammas. The fission products are substantially quantitatively obtained in approximately 1,500 gallons (per ton of uranium processed) of aqueous solution which is about 4 M in NH_4NO_3 and 0.2 M in $Na_2Cr_2O_7$.

V. Uranium Ore Control Analysis

For routine radium control in the Mallinckrodt uranium extraction plant, rapid approximate assay methods for radium in process filtrates are needed. Two types of solutions are encountered: (1) excess barium, (2) excess H_2SO_4. Methods of separating radium and counting its alpha particles have been developed for each type of solution. For (1), barium carrier is added and precipitated with a concentrated HCl-ether mixture to carry the radium. For (2), the radium is first precipitated with $PbSO_4$ to separate it from the H_2SO_4, and then precipitated with $BaCl_2$. These procedures separate radium almost quantitatively from uranium, thorium (Io), protactinium, actinium, and polonium. Tests at the Mallinckrodt pilot plant gave satisfactory results. The early high results on process solutions several days old have been found to be due to regrowth of AcX(Ra^{223}) with its short-lived descendants.

Pyrex plates have proved to be excellent mounts for barium-radium samples for alpha counting and should find extensive application in other radioactivity work.

Today's paper carries a main story on President Truman's changes in the top command of the Army and Navy. General Eisenhower will become chief of staff of the Army, replacing General Marshall, who is retiring. Admiral Nimitz will become chief of naval operations in a few weeks.

Thursday, November 22, 1945

I replied, by telegram, to Helen Gahagan Douglas about the resolution she proposes to make in Congress on the need for international cooperation and control of atomic energy. I said that I am in agreement with the principles stated in her telegram, but I do not feel qualified to comment on the advisability of introducing such a resolution in Congress.

We spent a quiet Thanksgiving Day at home but went to the Flamingo for Thanksgiving dinner; this was a mistake as Helen then suffered all night from an extreme case of "Clinton fever." (This term has appeared as a result of the frequency with which people get diarrhea with acute cramps when they visit Clinton Laboratories and are forced to eat in the cafeteria there. This has not been a problem for me for in the early days when the situation was very bad, I stayed with Vance and Mary Cooper and ate Mary's excellent meals.) Fortunately, Helen's pregnancy was not affected.

The story of the assembling of the Hiroshima bomb on board the Enola Gay appears in this morning's paper.

Friday, November 23, 1945

Helen Gahagan Douglas introduced in Congress the resolution which I received on Wednesday.

Following the resolution Mrs. Douglas went on to say,

> The scientists keep reminding us that wars are man-made; that we can save ourselves if we have the will to do so but it will take our collective wills; they are trying to tell us by the continuous marshalling of the facts, that scientists alone cannot save the world. We can insure our safety only through the mutual consent of all the peoples of the earth.

She submitted for the record the "Scientists' Statement" from which she drew up her resolution. Mrs. Douglas pointed out that this statement has been endorsed by such leading scientists as Shapley, Bridgman, Urey, Franck, Dempster, Smyth, Einstein, Pauling, Wigner, and me. An extensive list of scientists who endorsed the resolution on atomic energy for world peace was submitted and included the members of the various associations of the Federation of Atomic Scientists, the National Working Committee on Atomic Energy headed by Dr. Shapley, and a large list of scientists signing as individuals.

I received a letter dated November 11 from Harrison Brown saying it is not certain whether he will join the group at Berkeley although it still looked quite attractive. He presently is leaning toward joining the Chicago Nuclear Institute but will make up his mind in the next two to four weeks.

I wrote to Lavender asking for copies for a number of documents pertaining to Patent Cases 52 and 61. I also ask for the return of the original assignments (1942 draft) of already executed Case 52. I say that the inventors feel that they must have these documents in order to be "sufficiently informed to proceed intelligently in future action involving these cases and hence to be in the best position to protect the interests of themselves and the Government."

I had another conference with Donald McPherson of the John Wiley Publishing Company about the possibility of my writing a book on nuclear chemistry for Wiley. McPherson said Wiley has a strong interest in publishing the plutonium volumes of the Metallurgical Project Record (MPR).

Daniels wrote to Captain Solberg of the Navy's Bureau of Ships to say he is pleased the Navy is giving attention to our Met Lab proposal for power piles (for ship propulsion use). He explains some of the details of recent pile research and points out that BeO has an unusually high heat conductivity which makes it favorable for high temperature pile use.

Weissbourd answered a letter from Miss Olive Buttsback of Chicago for me. Weissbourd said,

> It is the conviction of nearly all scientists that unless we achieve international control of the atomic bomb, there will be no place where we can go to live in peace and security, and the Armageddon of which you speak will surely arrive.

He suggests she write her Congressman to express her views on international control.

Don Hull wrote from Oak Ridge, requesting a copy of my Northwestern University talk. He also asks how to obtain a dozen copies of the 1944 "Table of Isotopes." Hull sent his best regards to Helen and me.

I wrote to Walter V. Burg, University of Toledo, to give him the brief quotation from my talk on November 16 about the discovery of elements 95 and 96. I explain that clearance has not been obtained to say much. I also promise to send him a copy of the text when I can have more copies made.

Rioting has been occurring for a couple of days in Calcutta in demonstrations marking Indian national army day.

Saturday, November 24, 1945

I received a letter dated November 23 from Ted La Chapelle saying that he read with interest a copy of my talk at the ACS Symposium at Northwestern University. He was curious why his name had not been mentioned in my account of the men who had worked out the chemistry of neptunium, especially as he had spent a year and a half on the problem. He asked if his name is being deleted from the Met Lab volumes.

Scull of Time magazine called me to say he has a report from Paris that the Russians have developed an infracosmic generator that can prevent an atomic bomb from exploding (or possibly predetonate it). The report says that this device can operate at a distance of 100-125 kilometers and can explode all bombs within a radium of 20 kilometers.

I wrote to Kennedy to inform him that I finally requested from Lavender the copies of documents on our patent cases. I enclose copies of my letter of yesterday to Lavender for him and Wahl. I include a copy of my talk at Northwestern University. I say the main emphasis in the talk was on the heavy elements and the separations process. I mention that it received more publicity than I had expected.

I also mailed Segrè a copy of my letter to Lavender and I thanked him for the sample material he arranged to send to us. I tell Segrè I am planning to spend the first week of December at Berkeley and the third week in Canada, at which time I will try to encourage Bruno Pontecorvo to join the group at Berkeley. I think he is the best man to build the type of machine many of us think we should have.

I then congratulated Segrè on the birth of a new daughter, his third child.

In a letter to Latimer I enclose a copy of my talk at Northwestern University and ask for his comments. I then write,

> We are doing a sufficient amount of work with elements 95 and 96 now so that it would be extremely convenient if we might have names for them, e.g., to use in connection with the writing of formulae. We have a couple of thoughts along this line and would like to learn your reaction to them. For element 95 we favor the name "americum," symbol Am, the name being based on its chemical similarity to europium, on the basis of the "actinide hypothesis," and also to a lesser extent, on the fact that it was a product of a great American research enterprise. For element 96 we favor "berkelium," symbol Bk, on a basis which is obvious. It seems worthwhile to name at least one of the several new elements first produced in Berkeley after that region and as you know the first production of element 96 can be ascribed to the cyclotron at Berkeley. We have some feeling against the "californium" in view of the

previous unfortunate histories in connection with the other elements which were named after states. If you have any suggestions for a more euphonious name than berkelium which still places its origin, I would appreciate hearing them.

I informed Ernest Lawrence, by letter, of the feeler I made to Bruno Pontecorvo about joining the group at the Berkeley Radiation Laboratory. I say that I am willing to talk with him when I am in Canada if he (Lawrence) thinks it advisable. I also mention that Perlman, Morgan, Robinson, and Werner will start work there about the first of December. I will spend the first week of December in Berkeley with them. The other men will probably arrive over a period of a few months, although some remodeling may have to be done in order to make the laboratories safe for handling intense alpha activity.

Luther B. Arnold, who is now with Arthur D. Little, Inc., wrote to me at home. He said that he heard, with amazement, reports of the announcement of elements 95 and 96 on the Quiz Kids Program. He requests information on the elements for use in their monthly Industrial Bulletin. Today I sent him a copy of the Northwestern University talk, saying that this is the total of the information that has been cleared. I mention that James and Morgan were associated with me in the work on 95, and James and Ghiorso on 96. I also emphasize the point that any deductions he makes from the manuscript must be his own. I do say that I would like to look at his write-up.

In other correspondence today I sent Charles K. Hunt biographical material, a photograph, and an abstract of my talk to be given to the Detroit Section of the ACS on January 17. I say that I will bring Mrs. Seaborg with me and would appreciate a hotel reservation for the night of the meeting.

Manning sent Daniels a request for a total of about 1.05 grams of pure radium as the oxide or chloride. Of this amount, about 50 mg would be irradiated in the Clinton pile for about one month to produce a small amount of Ac^{227} to study its nuclear properties and daughter products. Of particular interest will be the determination of the possible fissionability with thermal neutrons of Th^{227}, the 19-day daughter of Ac^{227}.

The remaining 1 g of radium would be irradiated in a Hanford pile for several months in order to produce 1-2 mg of Ac^{227} to be used in basic chemistry studies. The importance of these studies lies in the fact that actinium seems to be the prototype of many transuranic elements (actinide series), and only limited information has been obtained on actinium chemistry other than that from tracer studies. Nearly all of the radium requested would be recoverable and can be returned later.

Manning also requests that Daniels arrange to have about six Hanford slugs with high plutonium concentrations (around 800 gt) set aside for our future processing in order to provide a source of higher isotopes of plutonium and transplutonium elements. Methods for working up these slugs can be developed during the next few months while the slugs are cooling.

This morning's paper carries a story that American troops are wrecking Japan's five cyclotrons and dumping them in the sea.

Sunday, November 25, 1945

Darrell Osborne and I played 18 holes of golf at the Evergreen Golf Club. Our scores were DO-159 and GS-106.

In the evening Helen and I went to the Ricardo Studio Restaurant (437 N. Rush) where I spoke to a group of artists on the implication of nuclear weapons. The meeting was sponsored by the Independent Citizens Committee (ICC).

Under today's dateline the paper says that 54 army officers have been decorated for their part in making the atomic bomb possible.

Monday, November 26, 1945

Lorraine Eisen started working as a secretary in our section today at $0.79 per hour.

Beach and Kent of the California Research Corporation visited Section C-I today and conferred with Perlman, Stan Thompson, and me. We discussed our plans to move to the Radiation Laboratory at the University of California and talked about the possibilities of cooperation with the California Research Laboratory. One possibility is that they might build a pile for common use by the Radiation Laboratory and California Research Laboratory. I said that I might discuss this further with Latimer and Lawrence and then possibly call R. A. Halloran, President of the California Research Corporation.

Joe Hamilton called me from Berkeley. He told me that the main line that brings electric power into the Radiation Laboratory blew out today stopping the bombardment of Pa^{231} and U^{235} with helium ions. If this leads to a long shutdown, he will send the targets immediately if he can get a plane, otherwise he will finish the bombardment by the end of the week and then send the target so that it will arrive in Chicago next Monday or Tuesday. I told him that we will send him the 95^{241} sample by the first of next week.

Hamilton also mentioned that he has made reservations for me on the December 8 streamliner to Chicago; Latimer, Earl Miller, and I will have a drawing room and he and Lawrence a bedroom.

Hamilton remarked that Van Atta has built a mass spectrograph that will be very useful in identifying artificial radioactive isotopes.

I received a telephone call from Dean Moore of Notre Dame. Notre Dame is considering adding Gerhart Friedlander to the faculty, and Dean Moore asked for my evaluation of Friedlander. I told Moore that I would strongly recommend Friedlander without any reservations whatsoever.

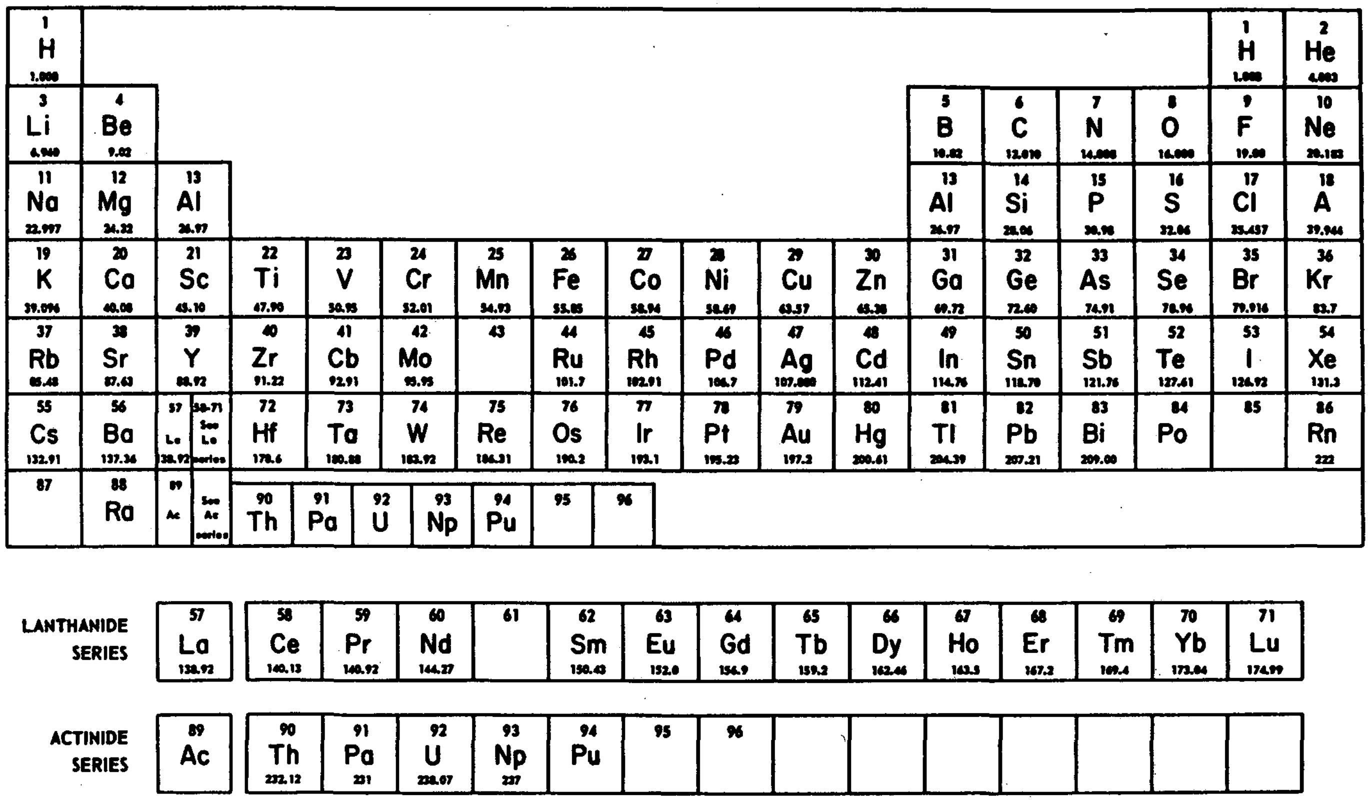
Chemical and Engineering News is planning to publish my speech at Northwestern University in the issue of December 10, 1945. Today I received a letter, dated November 23, from James M. Crowe, managing editor, who says that I will be able to make changes in the galley proof. He asks that I send the new periodic table to him within a few days. Robert F. Gould, the Midwest editor stationed here in Chicago, called later to discuss the publication and to say that he will bring a copy of the news story out to me for approval within a day or so.

I then replied to Crowe's letter. I say that Gould will send him a copy that incorporates the changes I want. I enclose two copies of the new periodic table showing the heaviest elements as an actinide series. (Figure 17.)

I called Robert Mulliken to tell him that my talk to the ACS Symposium at Northwestern University is going to be published in Chemical and Engineering News. I also told him about the forthcoming letter in Science magazine by Hamilton and me on the production of elements 95 and 96.

We discussed the status of the volumes that I am responsible for in the Metallurgical Project Record, namely volumes 14 and 17. I said I thought it would be possible to have these nearly ready in draft form by December 31.

Magnusson and Hindman wrote a memo to me discussing studies conducted on the separation of neptunium as a diketone complex from plutonium solutions. The work of Calvin, Crandall, J. Thomas, and Reid at Berkeley has shown that it is feasible to separate diketone complexes of Pu(IV) from aqueous solutions of low acid concentration by extraction with benzene. The +3 and +6 oxidation states of plutonium do not form extractable complexes to any extent with diketones. Since either Np(IV) or Pu(IV) may be removed as a diketone complex from mixtures, the element one wishes to retain in the aqueous solution must necessarily be in an oxidation state other than +4. Separating Np(IV) from a mixture of Np(IV) and Pu(III) has been shown



XBL 7711-10632

Figure 17. Periodic Table showing heavy elements as members of an actinide series, 1945.

to be feasible. In one run a plutonium decontamination factor of 300 was obtained with complete recovery of the neptunium. The amount of decontamination is as good as that obtained in average bromate cycle. The diketone complex method has the advantage in that mechanical losses are minimized and the time required is somewhat less than with the bromate method. Following removal of the neptunium, the plutonium can be recovered by carefully oxidizing the plutonium to the +4 state with bromine, chlorine, or nitrate followed by diketone complexing and extraction with benzene.

Scull of Time magazine, who telephoned me last Saturday about the story that the Russians have developed an infracosmic generator for incapacitating atomic bombs, called again today to get my reaction. I told him that Time could quote me as being "skeptical" about the reported Soviet development.

K. K. Darrow of the Bell Telephone Laboratories wrote me on November 23 (I received the letter today) enclosing a copy of a letter from Vladimir Karapetoff to H. A. Barton of the American Institute of physics. Karapetoff, who is retired from Cornell and was associated with the Board of Economic Warfare in 1942-43, published an article on electronic orbits within atoms in 1930 in the Journal of Franklin Institute (Vol. 210, p. 609), which gave data for all known elements and also extended considerably beyond uranium in the anticipation that heavier elements might one day be discovered. Karapetoff would like someone to compare his predictions with available data on neptunium and plutonium. Darrow suggests I or a colleague might be willing to review the data and write something kindly to Karapetoff since he is now blind and cannot consult the literature for himself.

I read a copy of W. W. Johnson's letter of November 24 to Gordon Leader making a formal offer to Leader for employment in Section C-I. Leader is to report for duty on November 30.

I wrote to Harrison Brown to thank him for his letter of November 20. I say I am very anxious that Brown join our Berkeley group and hope he would defer a decision about the Chicago Institute until late December after my return from Berkeley when I shall know more definitely whether a position sufficiently attractive to interest him can be arranged.

I also wrote to Sidney Fox at Iowa State College and thanked him for his letter of November 12 about methods of educating the public on the implications of atomic science. I say the Atomic Scientists organization is active along these lines and eventually will enlist cooperation from people like himself. I say that I have shown his letter to Leonard Katzin (a fellow student with Fox and me at UCLA) who is active in public education and perhaps Katzin will contact him.

I then replied to Jim Peterson's letter of November 9 in which he inquired about commercial sources of supply for radioisotope tracers. I say that we have been asked the question by a number of people recently, and that it is likely that the increasing pressure of the demand for radioisotopes on a commercial scale will soon lead to some sort of provision for their production and general distribution. I inform him that at the present time I am sending him three reprints of the "Table of Isotopes" and a copy of the series of lecture notes presented in 1942.

John A. Crawford is applying for a graduate fellowship in physics at the University of Chicago and asked me for a recommendation to be written to Walter Bartky of the University of Chicago. Today I wrote a high recommendation for him to Bartky, saying that Crawford is very able and mature. I say that he served as the "physicist" of Section C-I and that, while he was with us, he discovered on a mathematical basis the effect of backscattering of alpha particles. Further, I describe his experimental ability and say that I find that he has a pleasing personality and gets along well with others.

In other correspondence today, I mailed copies of my Northwestern University talk to Harry Wagreich of the City College, New York, and to Don Hull in Oak Ridge. To Hull I also sent a dozen copies of the "Table of Isotopes."

I also replied to Elliott L. Abers of Purdue University, mentioning that I would like to address his chapter of Phi Lambda Upsilon, but I will be unable to do so until the latter part of January.

I received and read a copy of a letter Farrington Daniels wrote to E. R. Murphee of Standard Oil Development Company. Daniels confirms an arrangement extending the loan of Stephen Lawroski until April 1 on a half-time basis. Daniels explains that he is not permitted to give the reasons for retaining Lawroski's services.

During the evening I spoke to the Met Lab Chemistry Division. I reviewed our heavy element program and our cooperative program with Hamilton and others to bombard all available pure heavy nuclei with neutrons, deuterons, and helium ions. The nuclei include Io^{230}, Th^{232}, Pa^{231}, U^{233}, U^{235}, U^{238}, Np^{237}, Pu^{239}, and a few second and higher order nuclei such as Pu^{240} and Pu^{241}. In discussing each bombardment, I wrote on the blackboard a tabulation of nuclear data obtained for the products and their daughters. I talked about the various projects being conducted within Section C-I and presented the names of the investigators.

"Truman Surprises Mother" is today's top headline, proving the war is really over! President Truman flew to visit his mother on her 93rd birthday.

Tuesday, November 27, 1945

Today Leon Morgan terminated work at the Met Lab and left for Berkeley where he will report for duty on December 3.

Groups 1 and 3 met at 8:28 a.m. in the New Chemistry conference room. Present in addition to me were Asprey, Britain, Cunningham, Florin, Ghiorso, Hagemann, Hindman, Hopkins, Hyde, Jaffey, James, Katzin, Kohman, Magnusson, Manning, O'Connor, Osborne, Peterson, Robinson, Sedlet, Simpson, Stewart, R. Thompson, S. Thompson, Van Winkle, and Westrum. I reported on the status of bombardments and informed the section that there will probably be a week's delay in the bombardments because of a blow-out of the main power line to the Berkeley cyclotron. Hamilton will send the Pa^{231} target by plane, if possible, or by the "City of San Francisco." It will arrive with a U^{235} target that was used as a target backing for the protactinium. The 95^{241} target with 100-200 microampere-hours should arrive early next week. The U^{233} bombardment will be deferred until about December 5 when a purer sample will be available. A U^{238} target has been bombarded with helium ions behind the interceptor targets and now has 600-700 microampere-hours. Hamilton would like 6-12 additional depleted uranium discs to be used as backing targets.

James talked about his difficulty in interpreting the results of the 93^{237} plus helium ions bombardment. The 93 fraction shows an extra alpha peak and many small ones but he does not believe Np^{234} is present to cause this. Ghiorso said the family of alpha particles was probably not in the U^{230} series because the energies were not right. He said the alpha particles look more like those that arise from thorium emanation; radiothoron has an energy similar to the extra alpha peak observed. James said the only problem with this interpretation is that 30 percent of this activity concentrated in the plutonium fraction and quite a bit of it is still in the neptunium fraction.

Stan Thompson described the work on the 95 fraction of the 93^{237} plus helium ions bombardment and said the beta-particle activity has about reached an equilibrium level but the x-rays and gamma-ray activities are still decaying with a half-life of about 1.5 days. The energies of the radiations have been fairly well characterized. If any positrons are present, they cannot be detected with the magnetic separator, and the 1.4 Mev gamma-ray may probably mask a small amount of annihilation radiation.

Peterson reported on the 3-day neutron bombardment in the Clinton pile of 1 mg of Ra^{227} and IR-1 column separation of Ac^{227} and its daughter, 11.7 day Ra^{223}. He plans to check the alpha branching of the actinium when the Ac^{227} has been further purified. It may be possible to obtain enough actinium from the decay of six-year old protactinium in our laboratory to enable us to check the fissionability of the Th^{227} growing from the actinium.

I then announced that after the presently scheduled bombardments have been completed, all experimental work will be dropped and everybody in the section will spend all his time writing portions of our project reports.

Gladys A. Reichard, Professor of Anthropology at Columbia University, prompted by my appearance on the Quiz Kids Program, wrote me that she spent the summer in the Southwest. While there she learned from friends and Hopi Indians that they observed the light flash from the Alamogordo atomic bomb test at least 340 miles away from the site.

William Jack of Monsanto Chemical Company in St. Louis called me to say that he has received from J. C. Warner black-and-white and color pictures of the first plutonium compound to be isolated. He would like to use one of them on the cover of the Monsanto Magazine and also release it to the newspapers. I said that this would be all right but asked him to mention that Cunningham and Werner were involved with the experiments that resulted in this first isolation of a plutonium compound.

Later in the afternoon I received a telegram from Jack saying that they have decided not to submit the plutonium picture to the newspapers but will probably use it as a cover photo for the Monsanto Magazine.

I wrote to E. O. Lawrence to report on our meeting with representatives of the California Research Corporation (the research laboratory of the Standard Oil Company of California) who are interested in the future prospects of piles for power and the preparation of special materials. I suggest that Lawrence might consider a possible cooperative program between the group and the University of California.

Hamilton phoned to report that power has been restored and the cyclotron is running again. He will continue the bombardments of Pa^{231} plus helium ions and U^{235} plus helium ions (the U^{235} as a backing target) until about next Monday and then ship it to Chicago probably by plane. Possibly it may be as late as Wednesday before the bombardment will be finished. I told him that we will send him the Am^{241} on Saturday or Monday and the U^{233} on Wednesday.

Hamilton said he has a telegraphic request from Bradbury at LASL for a sample of U^{232} containing 5×10^8 dis/min. I suggest the best way to prepare such a sample would be to bombard Pa^{231} with neutrons in a Hanford reactor (for example, a bombardment of 1 mg for one month would lead to 10^8 dis/min of U^{232}, but it would take the cyclotron at least three months to make such a sample).

I held a conference in my office with T. S. Chapman, Coleman, Katzin, Manning, Perlman, Colonel Ruhoff, and R. Thompson. We discussed the recovery of appreciable amounts of Pa^{231} and Io^{230} from the uranium ores and residues available as a result of the processing of huge amounts of uranium. We told Ruhoff that we have analyzed some of these residues and find about 0.2 ppm of Pa^{231} and 60 ppm of Io^{230} in the three "carbonate" barrels that have been sent to us. Our calculations show that we would need three tons of uranium residues to get 100 grams of protactinium. We also considered the siliceous radium residues from uranium processing and find that they contain 0.2-0.5 ppm of Pa^{231}. The content of various residues and the concentration of protactinium and ionium in these and means of recovering this material were discussed in detail. The relative merits of producing Pa^{231} through the neutron bombardment of Io^{230} was also considered.

Wednesday, November 28, 1945

Albert Tannenbaum phoned me to discuss the plans for purifying the U^{232} which is to come to us from Rochester. We also made arrangements for us to assay a sample of U^{233} for them. I asked him to contact W. M. Manning to work out the details.

Norris Bradbury called me from Santa Fe. We talked about the disposition of a 160-gram sample of 350 gt plutonium that has been produced at Hanford. We also discussed the container and the shipping conditions for the sample which will be sent to us.

I told him about my idea of producing U^{232} by the irradiation of Pa^{231} with neutrons at Hanford.

Kohman sent me a memo that completely describes their (Ames, Schoke, and Kohman) trip to St. Louis on November 5-7. Kohman says that a report on their work will be available at the conclusion of their assignment.

Today I received a letter from Watson Davis about my forthcoming appearance on his science radio program, "Adventures in Science." He encloses a very rough draft of a framework for the program. Davis suggests that perhaps I could announce the name for my "new chemical babies" on the program.

I then called him at the offices of Science Service in Washington, D.C., to discuss the arrangements for the program. I will go to the CBS radio station, WBBM, in the Wrigley Building in Chicago at 1:15 p.m. on December 15, and there I will be interviewed on the direct broadcast. Davis again asked that I name elements 95 and 96 on his program, but I told him this would not be possible. The broadcast goes to some 60 to 70 stations. Davis mentioned that he has been on the air with this program for 15 years.

We also discussed plans for publishing a somewhat revised version of my talk at the ACS Symposium on November 16 in the Science Service magazine, Chemistry.

I replied to Ted La Chapelle's letter of November 23. I explain that many names were probably left out of my talk at Northwestern University as it would have been beyond the scope of the presentation to credit all the individuals who worked with neptunium. For example, I made no mention of the tremendous contributions of Roy Thompson, Katz, Albaugh, Fields, or even Cunningham who was the first to work with pure neptunium. I tell La Chapelle that even before his letter arrived, I had taken steps to augment the written report of my talk to mention these names, including his. I mention that his contributions to the project are also properly credited in the Met Lab volumes. I also suggest that, if he is still interested in graduate school at Berkeley, that he should write to Professor Latimer.

I suggested, in a letter to Jerry Howland, that if he wishes to rejoin the Met Lab, if only temporarily until July 1, 1946, he should get in touch with Willard or Manning (I shall be out town) about possibilities here.

I thanked Adele H. Maze in Oak Park for her note about a meeting of the California Alumni Association on December 10. I explain that travel will take me out of Chicago until the afternoon of December 10, but that I will try to attend for a short time, if I can work it into my busy schedule. I say that I posted the notice in the Laboratory.

In a letter to Norman Kharasch of the Department of Chemistry, Northwestern University, I accept his invitation to address the Chicago Section of the ACS on February 15, 1946. The proposed title for the talk will be, "Some Chemical Aspects of the Plutonium Project."

W. M. Branch, Chief Administrative Officer, sent out a notice that security violations have increased in many divisions in spite of the fact that the number of employees has decreased. Branch stresses that classified documents must not be left unsecured.

Other correspondence today includes a letter to Bernard Brody, in which I tell him that I have filled out the recommendation form for medical school. I also ask whether he would like to return to the Met Lab, perhaps temporarily. I say there may be some difficulties since he is still of draft age but, if he is interested, he should write to Willard or Hogness.

I also wrote a note to Arthur G. Levy who wrote me last week after listening to the Quiz Kids Program. Levy suggested some unusual names for element 95 and 96 (plus a few others). I say, "...the names you suggest introduce principles for the naming of the elements that have never before been employed. The suggestions certainly show a great deal of originality and imagination..."

I sent a letter to Kay Tracy, Office of the Chancellor, Washington University in St. Louis, returning to her an enclosed draft document by Pregel which attempts to summarize potentially interesting scientific problems that might be investigated looking at the elements in the radioactive residues resulting from the extraction of radium and uranium. Some of the problems he suggests are the possibility of finding the element plutonium in nature, studying the fission of Pa^{231}, Io^{230}, the production of Pa^{235}, Pa^{233}, Pa^{229}, Th^{229}, and Ac^{229}. Pregel speculates that all these isotopes will most likely undergo fission with thermal neutrons and that "the combination may be even more powerful than the uranium bomb." I point out that we have already given a great deal of thought to most of these items and have actually made experimental measurements in connection with many of the isotopes.

Patrick Hurley has resigned as U.S. Ambassador to China, and George Marshall has been appointed special envoy to China, according to today's paper.

<u>Thursday, November 29, 1945</u>

Iz Perlman, who first came to the Met Lab with me in April, 1942, terminated work at the Met Lab to return to the University of California at Berkeley. Herman Robinson also terminated in order to be entered on the Berkeley payroll on December 3.

Friedell at Oak Ridge called to discuss with me the trouble that Eastman Kodak has been having with some straw board that contains sufficient radiation to spoil some of Kodak's film. Apparently the radioactivity emits only beta particles but no gamma-rays; it seems to come from the water that they use in the process. We considered the possibility that this might be caused by fallout from the Alamogordo test of July 16, which might have been deposited in the water that they use. We agreed that the radioactivity might be identified by submitting samples of the contaminated area in the straw board; this could be further investigated by means of chemical separations and counting of the radiations. I suggested that he contact Charles Coryell to follow up on this.

John R. Kinsey, Associate Editor, <u>Popular Science</u> magazine, called about an article he is writing for his magazine on atomic energy. I suggested that he come to my office so that I could go over the proposed content of the article with him in more detail. Later today he brought the article out for me to see.

After reading the copy of the article Luther Arnold asked me to review for publication in the Industrial Bulletin of the Arthur D. Little Company, I called to tell him that I find the article acceptable as written. We also discussed a number of tracers that we will soon be making available to him.

I also called Watson Davis to discuss, in more detail, the text for my interview on the "Adventures in Science" radio program on December 15. Davis said I should be at the studio in the Wrigley Building at 12:30 p.m. Davis again asked me to propose names for elements 95 and 96 during the interview, and I again told him that this is impossible. I informed Davis I will send him a revised text from Berkeley during my trip.

I wrote to Vladimir Karapetoff, retired Cornell professor, about his 1930 predictions on the electron structures for possible transuranic elements. I say that I read his article which had previously escaped me. I note that his proposed placing of the 5f electrons has been found actually to occur in the heavy element region and deviates only with regard to the actual beginning of this transition series. I say that I am sending him a copy of a talk I recently made so that he can see how well his predictions have been borne out.

Other correspondence included a letter to Charles K. Hunt to give him the title, "The Chemical Processes in Plutonium Production," of the talk I will deliver to the Detroit Section of the ACS. I sent James T. Grady of the American Chemical Society News Service a copy of the paper I intend to present before the Illinois-Iowa Section on December 13. Finally, I sent Ruth C. Behr, a high school student who is having difficulty convincing her teachers that there are more than 92 elements, a little information about neptunium, plutonium, and elements 95 and 96. I say that I hope it will be some help to her in educating her teachers.

Kohman sent a request to Furney for several samples of ores, gangue, barium cake and residue, aqueous raffinate, raffinate precipitate, and clarified barium filtrate. He needs about 1 kg of each solid or one liter of each liquid sample and, if possible, they should all be from the same specified type of ore.

I left Chicago at 6:00 p.m. on the "City of San Francisco" for Berkeley (lower berth in car N68). I am carrying three targets for bombardment in the Berkeley cyclotron: two natural uranium metal discs about 500 g each, and a depleted metal disc of 434 g.

This morning's paper says it is becoming increasingly clear that non-Indonese cannot live peacefully without permission of the Indonesians as fighting continues there.

Friday, November 30, 1945

Enroute to Berkeley.

In Chicago Gordon Leader is scheduled to be rehired.

The article, "The Production of Elements 95 and 96," by Hamilton and me was published in the November 30, 1945 issue of Science magazine.

Harold Urey appeared before a Senate atomic energy committee and recommended outlawing use of the atomic bomb under the United Nations. He thinks that we should offer to dismantle ours and "dump" the fissionable material in the Mississippi River rather than get into an armaments race.

Saturday, December 1, 1945

I arrived in Berkeley via the "City of San Francisco." Latimer and I spent the morning talking at the Chemistry Department. I had lunch at the Faculty Club; and then Calvin, Latimer, and I went to Memorial Stadium where we saw California beat St. Mary's Preflights, 6-0.

I am staying at Melvin Calvin's home this evening. Elin Calvin was born three days ago.

Dr. Irving Langmuir of General Electric told a Senate committee today that he favors outlawing the atomic bomb and believes Russia will produce its first bomb in about three years.

Sunday, December 2, 1945

In Berkeley. Calvin and I played golf at Tilden Park. I shot a 99 for 18 holes and Calvin shot 69 for his nine holes.

Jeanette and I had lunch at Tiny's in Oakland.

I am again staying at Calvin's home tonight.

Monday, December 3, 1945

In Berkeley. I spent the day with Latimer and various individuals of the Radiation Laboratory. Perlman, Werner, and Robinson were added to the Radiation Laboratory payroll today. Werner's salary is $340 per month, and Robinson's salary is $375 per month until January 1 at which time it will go up to $400.

Before retiring at Calvin's home, I wrote to Helen.

Tuesday, December 4, 1945

In Berkeley. I continued discussions with Radiation Laboratory individuals in my office in Room 102 of Building 4 about our new nuclear chemistry group being established. Leon Morgan began work at a salary of $280 per month with an increase to $300 on January 1.

I had dinner with Jeanette at Tiny's in Oakland. I spent part of the evening with the Casons and then returned to Calvin's home where I am sleeping.

I learned that the Chicago Tribune carried an item today quoting Thorfin Hogness as saying that the "golden age of atomic energy is many years in the future." This is a quotation from a speech he gave at a meeting of the City Club of Chicago yesterday.

Wednesday, December 5, 1945

In Berkeley. I received and read copies of George Everson's offers to Al Ghiorso of $385 per month and to Stan Thompson and Burris Cunningham of $450 per month.

Lawrence, Latimer, and I met with Follis, President of Standard Oil Company of California, and other executives of the company to consider a cooperative program in which they might build a pile.

I had dinner at Prescott's home. I am still staying with Melvin Calvin.

Today's paper says that the Senate voted last night to place the United States in the new League of Nations.

Thursday, December 6, 1945

In Berkeley. I gave a talk to the people on the hill.

Lawrence sent me an interdepartmental note inviting me to become a regular member of the Monday evening research conference which meets in Room 222 of Le Conte Hall from 7:30 to 9:30 p.m.

I learned in a small item in the Chicago Tribune this morning Oppenheimer is quoted as saying that the atomic bomb raises a serious problem as to the value of capital ships in the Navy as the cost of bombs is so small compared with the cost of battleships. Oppenheimer testified before a special Senate committee on atomic energy.

I wrote Helen a note and again spent the night at Melvin Calvin's.

Friday, December 7, 1945

Our new secretary, Mary E. Millard, began working today. Her salary is $191 per month with a 41-hour work week.

I had lunch with Jeanette. Later I attended a Radiation Laboratory cocktail party at the Claremont Hotel; some of us then had dinner at Trader Vic's. I am still residing at Calvin's home.

MacArthur's headquarters announced today that all mineral resources that might be used for an atomic bomb will be taken from Japan, even if they have to be shipped out of the country, and there will be a ban on mining such materials.

Saturday, December 8, 1945

Leonard Dreher and I ate lunch together. Later I left for Chicago on the "City of San Francisco." Latimer, Earl Miller, and I are sharing a drawing room. Hamilton and Lawrence are also on the train.

This morning the San Francisco Chronicle carried a story entitled "Man and the Atom" in which Paul C. Smith, editor of that paper, says that we need a world organization and a conscience in dealing with problems of the atomic age.

Sunday, December 9, 1945

Enroute to Chicago.

Monday, December 10, 1945

I arrived in Chicago aboard the "City of San Francisco" at 12:15 p.m.

This morning's headline reads "Gen. Patton Hurt in Crash." He suffered spinal injuries when the sedan in which he was riding was hit by a half-ton army truck yesterday.

The following is a summary of the events that took place in Chicago while I was in Berkeley.

Saturday, December 1

Hogness issued the manpower distribution within the Met Lab Chemistry Division (for academic employees) as of December 1, 1945.

Section C-I - Seaborg, Section Chief

Group 1 - Heavy Isotopes (Seaborg)	22
Group 2 - Control Analysis (Kohman)	6
Group 3 - Services (Stewart)	3
Group 4 - Solvent extraction (Lawroski)	17
Administration - Seaborg and Manning	2
Total C-I	50

Section C-II - Hogness, Acting Section Chief	
Group 1 - Pile Research (Daniels-Willard)	11
Group 2 - Radiation (Allen)	4
Group 3 - Fission Products (Rubinson)	5
Group 4 - Analytical (Templeton)	8
Administration - Hogness and Willard	2
Total C-II	30
Grand Total	80

The total of 80 includes 26 SED men. A formal picture of the Met Lab SED group in front of the Armory was recently taken. See Figure 18.

Other recent snapshots of our men are shown in Figures 19-21.

A letter arived from John R. Kinsey, Associate Editor of Popular Mechanics. Kinsey thanked me for reviewing his article and said he would call me December 11 to arrange for one of their photographers to take my picture.

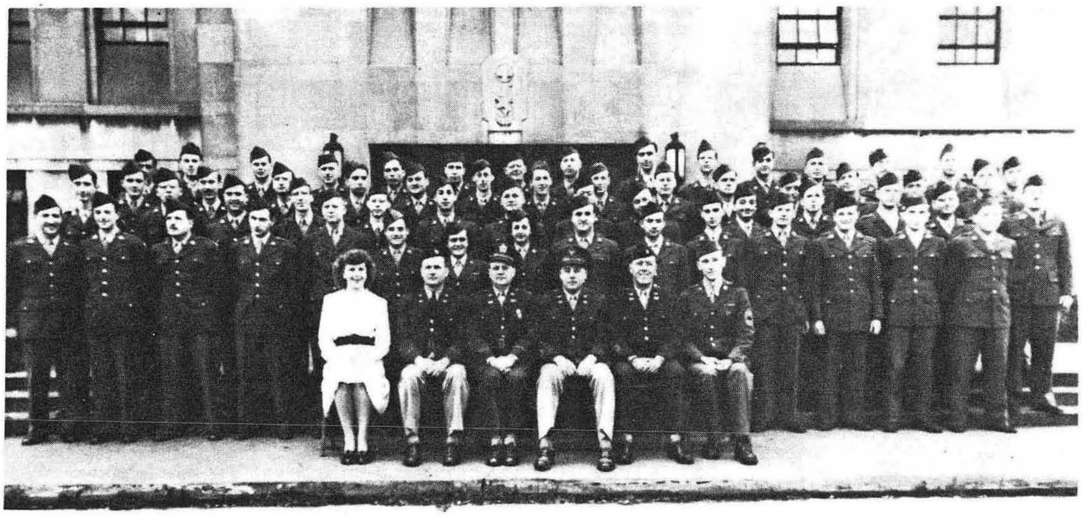
My office received a letter of November 25 addressed to me from Captain P. J. Sykes, Jr., a former undergraduate student of the University of British Columbia at Vancouver who is now stationed on Guam. Sykes requested general information on methods of production and atomic properties of the two new elements which "Stars and Stripes" has reported that I recently discovered as "a result of bombardment of uranium." He had some misconceptions of the atomic number and method of production for plutonium, and he admitted his only present sources of information are Time and Life and pre-war textbooks.

Sunday, December 2

The Advisory Committee on Argonne Operation, consisting of Daniels, Tate, Gustavson, Loomis, Colonel Fields, Major Bloch, Colonel Nichols, Eshbach, Compton, Spedding, Zinn, and Hilberry, met today. Nichols stated in his opening remarks that the new laboratory at Argonne will be a "guinea pig" and recommendations are needed for its operation in the near future.

Compton outlined problems concerning how Argonne Laboratory should be operated, the scope of the future Argonne program, the siting problems relative to the selection of a location for the laboratory, and the relationship of Argonne's future program to programs of University laboratories.

Zinn presented an outline of the present and possible future programs for Argonne.



XBB 801-458

Figure 18. SED Group at Met Lab, December 1945. (First row, left to right) Miss Emma Schlak, Maj. John H. McKinley, Capt. Thomas S. Chapman, Capt. Paul Baranowsky, Lt. John H. Mahoney, M/Sgt. Russell E. Bidlack. (Second row, left to right) T/3 Harris H. Levee, T/4 Milton Ader, T/3 Lee V. Lawhead, T/4 Eugene A. Hausman, Pfc. Ernest H. Wakefield, T/4 Renato J. Ferretti, M/Sgt. Dollie E. Henry, Sgt. Marion Gottschall, T/4 Sheldon Himmelstein, T/3 Solomon H. Turkel, T/5 Herbert Kanner, T/3 George W. Cressman, T/4 Alonzo C. Rand, Jr., T/4 Theodore H. Garner, T/Sgt. Donald C. Stewart. (Third row, left to right) Pfc. Albert J. Gaydick, T/5 Bernard B. Weissbourd, Pvt. David Revinson, Pfc. John L. Kuranz, Pfc. Alec E. Kelley, T/3 James A. Schoke, T/5 Martin H. Studier, T/4 Norman Pearlman, T/4 Donald P. Ames, T/4 Leonard G. Nierman, T/3 Marvin L. Goldberger, T/4 Phillip Fineman, T/3 Irving Wender, T/3 Herbert H. Anderson. (Fourth row, left to right) Pfc. Wallace C. Koehler, T/4 Robert K. Swank, M/Sgt. James T. Harris, T/5 Abraham Broido, T/4 Stanley W. Crain, T/4 Beverley W. Lewis, T/4 Stewart A. Fox, T/5 Thomas E. Mitchell, M/Sgt. Paul A. Dana, T/3 David R. Hume, T/Sgt. Max B. Rodin, T/3 Clarence L. Wesenberg, T/4 Charles Eggler, T/5 Richard A. Potter. (Fifth row, left to right) T/3 Walter P. Huebner, T/4 Horace H. Hopkins, Jr., T/4 William C. Redman, T/3 John H. Schraidt, Pfc Roy G. Post, T/4 Weldon C. Graham, T/3 J. W. Britain, T/3 Manuel H. Holtzman, Pfc. Robert S. Cohen, T/3 Larned Asprey, T/3 Robert T. Best, T/3 Vladimir J. Houska, T/Sgt. Walter M. Stewart, T/3 Robert J. Smith, Pfc. Sholem Postel.

Figure 19. D. P. Ames and Phillip Fineman at Brookfield Zoo, Autumn 1945.

XBB 802-1884

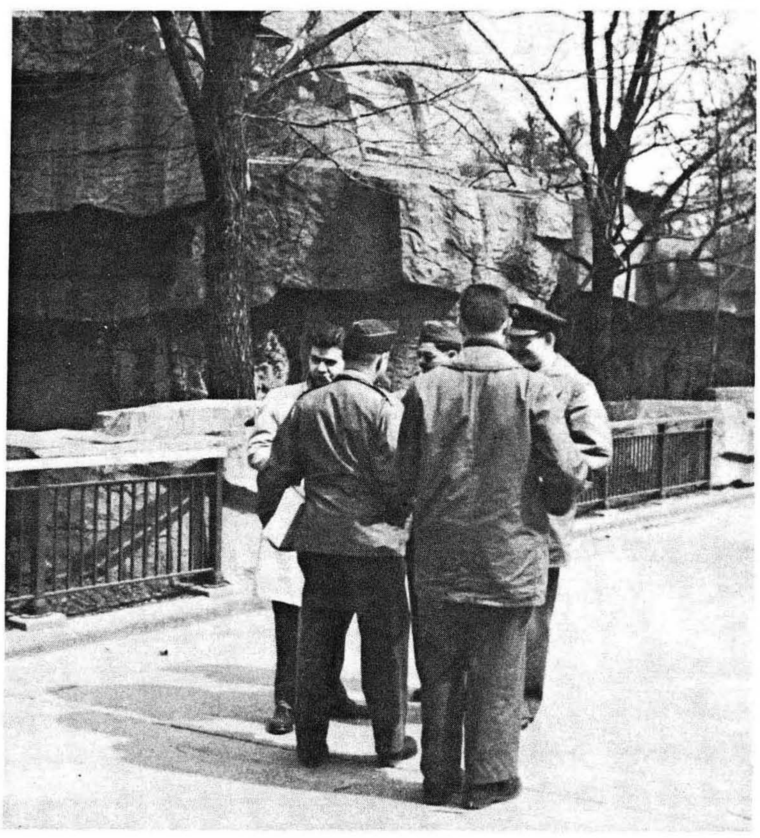


Figure 20. R. A. James, H. H. Anderson, J. W. Britain (facing), D. P. Ames and Phillip Fineman (backs), Brookfield Zoo, Autumn 1945.

XBB 802-1885

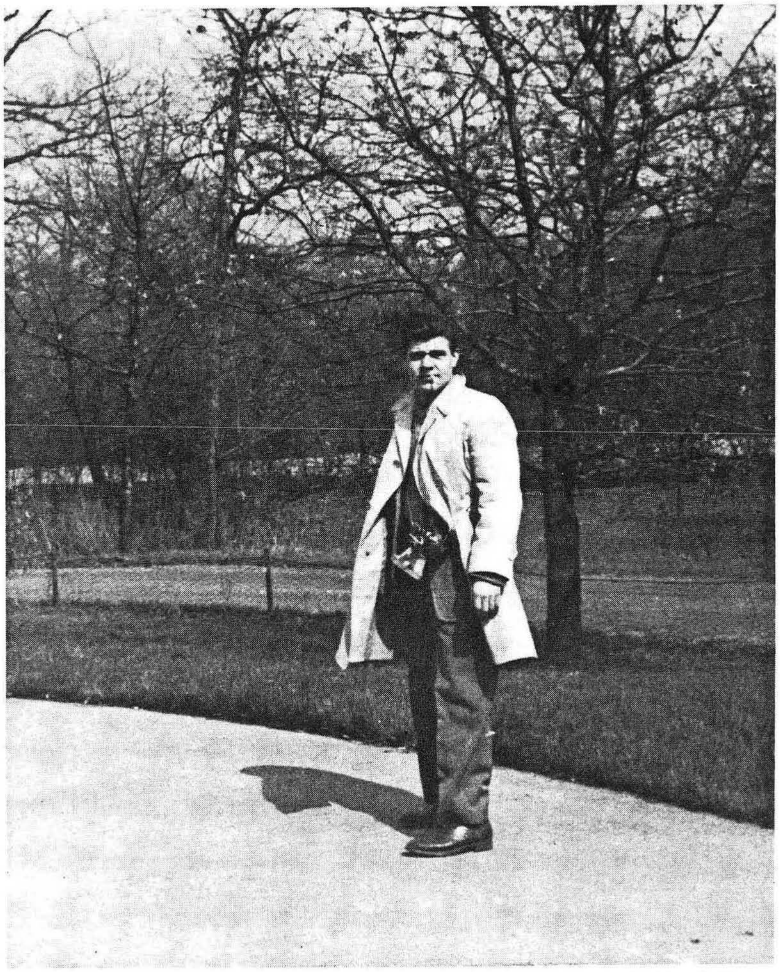


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Figure 21. Ralph James, Brookfield Zoo, Autumn 1945.

Compton offered to the Committee his ideas for a seven-man "Board of Argonne Laboratory" and a permanent council with members from various universities and institutions. He said Argonne would swallow up all of the present Met Lab organization. The requirement for strong "regional laboratories" working closely with university laboratories was discussed, and justification for such developed.

Monday, December 3

At 9:00 a.m. the Laboratory Council met in Room 209, Eckhart Hall. The following persons attended: Branch, Cole, Daniels, Dempster, Foote, Mulliken, Nickson, Ohlinger, Rabinowitch, Wilkinson, Willard, G. Young, H. Young, Zachariasen, Zinn, and Zirkle. Daniels speculated on the future of the Met Lab and said it may become part of a government regional laboratory. The U.S. Army plans to keep control until such time as the Atomic Energy Commission is formed. He urged retaining important academic personnel wherever possible.

Mulliken spoke concerning the urgency of work on the Plutonium Project Report (PPR) and read a letter from Colonel Nichols requesting that the highest priority be assigned to completing the report. Mulliken presented the PPR Editorial Board's proposed publication policy which includes the recommendations that all PPR volumes which cannot meet reasonable deadlines should be "replanned, or if necessary, liquidated."

A letter dated November 30, arrived from Lavender. He acknowledged receipt of my letter of November 23 requesting copies of Cases S-52A, S-52B, S-52C, S-52D, and S-61 as well as copies of the patent agreement between the inventors and the Government. Lavender's reply was as follows:

> The present arrangements, as you know, provide for all copies of the agreement being held by me in escrow until the agreement had been consented to by the Regents of the University of California. Upon receipt of advice from the Regents of the University of California as to the University's willingness to execute the consent to the agreement, the agreement will, in accordance with the arrangements, be transmitted to them for execution and upon return, an executed copy of the agreement will be sent to each party signatory thereto.
>
> The applications are at present under examination and it is expected will be filed shortly, however, it has been the policy of this Office not to distribute copies of applications to inventors in view of the classified nature of the subject matter. Copies will be at all times available at the respective patent organization initiating the application, in this case copies of the application are available at all times to the

inventors at Chicago. Upon release of the subject matter from classification, copies will be made available to the inventors. It is believed that you appreciate the necessity of not having classified documents made available to individual inventors. I hope that the provisions for their availability in Chicago will meet with your approval; however, if you desire, copies could be made available to the patent group at Berkeley, California, or Santa Fe, New Mexico, for inspection by the inventors at any time.

In accordance with your request, the original assignments executed by yourself, Dr. Kennedy, and Dr. Wahl on 9 November 1942 are returned herewith.

A letter was sent over my signature to Everson giving personnel and salary information on six C-I individuals who are to be transferred to the Radiation Laboratory. These individuals are Blaedel, Cunningham, O'Connor, R. Thompson, S. Thompson, and Westrum.

Dr. J. J. Nickson spoke at the weekly Monday night Met Lab seminar on the subject "Tolerance Doses."

Tuesday, December 4

Groups 1 and 3 met at 8:28 a.m. in the New Chemistry conference room. In attendance were Asprey, Britain, Cunningham, Florin, Ghiorso, Hagemann, Hindman, Hopkins, Hyde, Jaffey, James, Katzin, Kohman, Magnusson, Manning, O'Connor, Osborne, Peterson, Sedlet, Simpson, Stewart, R. Thompson, S. Thompson, Van Winkle, and Westrum. Manning said that except for a few remaining bombardments scheduled for Berkeley, first priority must be given to completing the backlog of writing assignments for the Project Record. The protactinium target will arrive tomorrow with its U^{235} backing. The 95^{241} has been sent for a ten-day bombardment even though it has a few percent of thorium in it. Following this, the U^{233} sample will be bombarded with helium ions for a week.

Ghiorso reported that some Pa^{231} will be bombarded at Argonne to make Pa^{232} which he will then test for slow neutron fissionability.

Van Winkle said he redetermined the specific activity of Pa^{231}. Previous half-life values made on small samples were 38,500 and 32,000 years as determined by this laboratory and Von Grosse, respectively. Van Winkle's best measurement is now 34,400 ± 150 years, but this is still only a preliminary value because of some uncertainty about purity. There is now a total stock of 7-8 mg of protactinium in Section C-I. TTA and the ketone separation method for protactinium extraction and purification were discussed. The ketone method is more specific for use in protactinium purification.

Florin described new apparatus for converting UF_4 to UF_6 and introducing it into a capillary suitable for x-ray analysis.

Cunningham described tentative observations on 95 chemistry. James said that a 95^{241} sample followed for eight months shows a 40-year half-life but measurements are still not accurate enough to report the half-life with certainty.

Stan Thompson said that one or two activities have been found in the 95 fraction of the working up of the Np^{237} plus helium ion target. The radiation and half-life properties are somewhat different from 40-hour lanthanum.

James reported that the x-ray activity in the neptunium fraction of the Np^{237} plus helium ion target is quite long-lived and may be Np^{235} from the reaction:

$$Np^{237}(\alpha,2n)95^{239} \xrightarrow{\alpha} Np^{235} \quad (t_{\frac{1}{2}} = 6 \text{ months})$$

If this is true, then the half-life for 95^{239} must be less than half a day.

Stan Thompson suggested that some of the electrons in the 95 fraction of the neptunium plus helium ion target may come from 95^{240} as follows:

$$Np^{237}(\alpha,n)95^{240} \xrightarrow[\text{Isomeric Transition}]{\gamma} 95^{240} \xrightarrow{\text{k cap.}} Pu^{240}$$

Peterson reported that the new value for the neutron flux at Clinton gives a neutron cross section for radium of about 20 barns.

Stewart said that his attempts to find a new method of separating lanthanum and 95 have not been successful.

Manning submitted forms MP-112 and MP-113 recommending that Westrum be promoted from an Associate Chemist to a Chemist rating.

My office sent Watson Davis a retyped script for the December 15 science radio program. This includes a few changes, which will have to be cleared through the Security Division.

Wednesday, December 5

Hindman and Magnusson prepared a proposal for a neptunium research program which includes the following studies: (1) the kinetics of bromate oxidation of Np(IV); (2) the radiation spectrum of neptunium; (3) the precise (to better than 5 percent) determination of specific activity; (4) solvent extraction of neptunium and plutonium;

(5) the potentials of Np(IV)-Np(V) couples; (6) the production of Np(IV) in nitrate solutions; (7) the formation of fluoride complex of Np(IV); (8) the sulfate complex(es) Np(IV), Np(VI); (9) the hydrolytic behavior of Np(IV), Np(V), and Np(VI); (10) the solubilities of Np(V) compounds; (11) the preparation of $NpCl_3$ and $NpCl_4$ (Fried), (12) the spectrum of neptunium; (13) the fluorides of Np-NpF_3, NpF_4, NpF_5, NpF_6 (Florin); and (14) the preparation of $NpBr_3$ (and $NpBr_4$?) (Fried).

Manning sent Hogness five requests for irradiation of samples in one of Hanford's piles. These include (1) additional neutron irradiation of U^{235} and Pu^{239} samples (CW-4); (2) irradiation of about one gram of radium (Ra^{226}) for a period of several months; (3) irradiation of 5 mg of Np^{237} for a period of several months; (4) irradiation of approximately 10 gm of pure U^{238}; (5) irradiation of 5 mg of U^{233} for a period of about a year.

In another memo Manning sent Chapman the analysis (by R. Thompson) of the protactinium sample received from Chapman's office a few weeks ago. The sample contained about 28 mg of Pa^{231} which will be retained for use in our chemistry program. Manning included a description of the analytical procedure used together with detailed results.

Thursday, December 6

A copy of MUC-GTS-2080, "Redox Solvent Extraction Process for Recovery and Decontamination of Plutonium and Uranium from Irradiated Uranium," by S. Lawroski, W. J. Blaedel, and I. J. Schaffner was received in my office. It is a 17-page description of the Redox Process that includes a comparison of this method with the Bismuth Phosphate Process.

Friday, December 7

Ghiorso, Peterson, Hagemann, and Studier spent the day at Argonne. They bombarded O'Connor's 49NH sample (purified plutonium from CW-3, Hanford-irradiated plutonium). In addition, they also plan to bombard some Pa^{231} in the thimble to make Pa^{232}. This will be then tested in the thermal column for slow neutron fissionability.

Daniels sent a teletype message to Compton (Chancellor, Washington University, St. Louis) stating he has decided to stay with the Chicago project to "try to aid in (a) successful transition into a strong regional laboratory advancing our field and cooperation with universities and institutes." He resisted the possible move of the Met Lab programs to Clinton and recommended that his reactor project be pushed through and that research on the project be continued at

Chicago. He said he was proud of the accomplishments during the past six months and pointed out that a whole new radioactive series was discovered under my direction.

* * *

A Project Information Meeting on Health is being held today.

Ghiorso and Studier are continuing their experiments at Argonne.

"The Chemical and Radioactive Properties of the Heavy Elements," appeared in today's issue of Chemical and Engineering News [23, 2190 (1945)]. I include the following paragraphs about the actinide concept.

> The elements 90 to 94 lie in corresponding positions just below the sixth period transition elements Hf to Os (atomic numbers 72 to 76) in which the 5d electron shell is being filled. The transition elements Hf to Os are similar in their chemical properties to the corresponding 4d transition elements in the fifth period (Zr to Ru, atomic numbers 40 to 44). Although the first members ($_{90}$Th, $_{91}$Pa) of the group 90 to 94 show a great resemblance in chemical properties to the first members ($_{72}$Hf, $_{73}$Ta) in the 5d transition series and to the first members ($_{40}$Zr, $_{41}$Cb) in the 4d transition series, the later members ($_{93}$Np, $_{94}$Pu) show practically no resemblance to $_{75}$Re and $_{76}$Os and to $_{43}$Ru and $_{44}$Ru. This suggests that it is the 5f electron shell which is being filled, although it is not possible to deduce from this chemical evidence alone whether uranium is the first element in the series for which this is the case. While it is beyond the scope of this discussion to give all the supporting evidence, we would like to advance the attractive hypothesis that this rare-earth-like series begins with actinium in the same sense that the "lanthanide" series begins with lanthanum. On this basis it might be termed the "actinide" series and the first 5f electron might appear in thorium. Thus the characteristic oxidation state--i.e., the oxidation state exhibited by those members containing seven 5f and fourteen 5f electrons--for this transition series is III.
>
> The oxidation state of IV demonstrated by thorium is then analogous to the IV oxidation state of cerium. From the behavior of uranium, neptunium, and plutonium it must be deduced that as many as three of the assumed 5f electrons are readily given up, so that the failure of thorium to demonstrate an oxidation state of III is accounted for. On the basis of this hypothesis, element 95 and 96 should exhibit very stable III states; in fact, element 96 should exhibit the III state almost exclusively because, with its seven 5f electrons, it should have an electron structure analogous to that of gadolinium, with its seven 4f electrons.

I read a copy of Fried's memo of December 7 to Zachariasen about the formation of neptunium tribromide and neptunium triiodide using the reaction:

$$6\ NpO_2 + 8\ AlX_3 \xrightarrow[heat]{} 6\ NpX_3 + 4\ Al_2O_3 + 3\ X_2$$

Beyes Afroyim called me to arrange for me to sit for my portrait at 9:00 a.m. tomorrow in his office.

The University of California issued a press release outlining Lawrence's plans for an almost complete reconversion of the University's nuclear research laboratories to peacetime pursuits directed toward the betterment of mankind. It is foreseen that the research program at Berkeley may make possible the production of "new and undreamed-of elements, the uses of which must be left for future determination." The release also states that

> Professor Wendell M. Latimer, Dean of the College of Chemistry, and Professor Glenn T. Seaborg, one of the discoverers of plutonium, and other colleagues of Professor Lawrence in the chemistry and physics departments, are hopeful that it may be immediately possible to produce new trans-uranic elements, four of which, 93 (neptunium), 94 (plutonium), and 95, and 96, have been produced already by the combined process of bombardment in the 60-inch cyclotron and chemical separation.

The release goes on to say,

> Professor Seaborg, who is still on leave of absence to the University of Chicago, will return early in 1946 to join Dean Latimer in the direction of the chemical phases of the atomic research, such as the search for new elements.

A reply arrived from Karapetoff to my letter of November 29. He thanked me for the copy of my talk and asked if he could quote from my letter to him.

Helen and I attended a University of California Alumni dinner organized by the Alumni Secretary, Bob Sibley. The dinner was held on a boat in the Chicago Yacht Club harbor. I gave a brief talk on nuclear energy.

Colonel Friedell spoke on the subject "A Medical Man's Experience at Hiroshima" at the Met Lab Seminar this evening.

<u>Tuesday, December 11, 1945</u>

Today's Project Council Information Meeting on Physics included papers by D. J. Hughes (Absorption Cross Sections for Fast Neutrons); A. S. Langsdorf (Further Experiments with the Oscillation Method for the Determination of Danger Coefficients); R. Scalettar (Critical Experiments); L. W. Nordheim for A. Weinberg (Experimental Plate Pile); T. H. Brill (Automatic Control of the CP-3 Pile); W. Sturm (Critical Spectrometer Measurements); A. J. Dempster (Isotope Abundance of a Special Sample of Uranium); W. C. Redman (Total Absorption Cross Section for Fast Neutrons); W. Spatz (Photoneutrons from Heavy Water); S. Bernstein for L. Borst (Spectroscopic Studies with Neutrons); R. B. Briggs (Column Performance in Metal Recovery); C. Lawson (Heat Transfer and Fluid Flow on Trombone Cooler); P. Lauletta (Effect of Molten Na/K Alloys upon Pile Materials); C. Neher (Production of Myrnalloy Metal); J. Chipman, M. Cohen, A. R. Kaufmann (Recent Progress in Metallurgy of Beryllium); R. L. Seifert (Vapor Pressure of BeO); C. A. Hutchison (Volatility of Uranium Oxide); G. W. Haldeman (Desorption of Xe). Dempster, in his paper, stated that his mass spectrographic determination of the sample of supposedly 100 percent U^{233} obtained from me contained 96.3 percent U^{233} and 3.7 percent U^{238}. These results can be used to correct the nuclear data on U^{233}.

Mark Fred made arrangements for me to attend the dinner of the Physics Club of Chicago, and give the invited paper entitled "A Discussion of Nuclear Energy." In my talk I pointed out that although current public consciousness of the Atomic Bomb Project largely centers around its military aspects, when the entire project history can be told it will be seen that the manufacture of the atom bomb involved the solution of an extensive list of extremely interesting and important scientific problems. As security permitted, I described the methods which have been developed for the production of fissionable material. I also discussed some of the potential peacetime applications of fissionable isotopes.

Under a December 10 dateline this morning's newspaper says that there will be a test by the Army and Navy of the atomic bomb using it against naval vessels.

Wednesday, December 12, 1945

I received and read a letter dated December 11, from Watson Davis, Director of Science Service. Watson enclosed the complete script of the upcoming December 15 "Adventures in Science" radio program.

I read a copy of Zachariasen's memo dated December 10 to Daniels about the crystal structure of $NpBr_3$ and NpI_3 as interpreted from samples prepared by Fried. $NpBr_3$ is hexagonal with two molecules per unit cell. The structure type is that of UCl_3. NpI_3 is orthorhombic with four molecules per unit cell and isomorphous with UI_3, PuI_3, LaI_3, $NdBr_3$, and $SmBr_3$. These properties were predicted by Zachariasen a year ago.

In another memo of December 11, Zachariasen reported on the tetragonal crystal structure of the beta form of UF_5 as interpreted from the x-ray diffraction patterns from a sample furnished by Libby.

I wrote to Miss N. A. Parkinson, Assistant to the Editor, Chemical and Engineering News, to tell her I would be happy to review William L. Lawrence's booklet, "The Story of the Atomic Bomb."

The Project Council Information Meeting on Physics and Chemistry continued today with emphasis on chemistry. The eleven papers from Clinton were presented by H. A. Levy (Uranyl Salicylate Complex Ion); C. Nelson (Determination of Small Quantities of Uranium by Means of Thiocyanate); N. Elliott (Production of a Light Radioactive Gas); H. Gest (Chemical Consequences of β-Decay of Selenium and Lanthanum); L. Myers and G. Boyd (Co-Precipitation of Trivalent Ions of Rare Earths and Plutonium); A. Adamson (Kinetics of Ion-Exchange Adsorption of the Alkali-Metal Ions); R. Leininger (Composition of Uranium Peroxide); M. Burgy (Short-Lived γ-Rays from Fission Products); J. Floyd (Cross Section Limits of a Heavy Isotope); S. H. Murray (Calculation of Pile Lattice). J. Marinsky of Clinton described his work on Radioactive Isotopes of Neodymium and Element 61. Using the ion exchange-elution adsorption method for the separation of these rare earths, he has identified the two-three year element 61^{147} and found the 47-hour radiation previously attributed to Nd^{149} to belong to element 61^{149}. E. Gladrow from Ames talked about the Separation of Rare Earths, and H. R. Nelson from Battelle spoke on Thermal Conductivity of Beryllium Oxide.

There were eight papers from Chicago:

Lawroski discussed the Redox Process and described the history and procedures used in its development. Thirty-five runs have been made and the application of the Redox Process to plutonium now seems feasible and practical. The whole process appears to be much simpler than the Bismuth Phosphate Process now in use.

James reported on the discovery of a new isotope of plutonium, thought to be Pu^{233}, which decays by K-capture.

Stan Thompson talked about the chemical properties of elements 95 and 96. He said separation of 95 and 96 from each other and from the rare earth fission products has been accomplished by complexing with fluorosilicate. Details of the process are being worked out. Oxidation states of the new elements are being investigated.

Cunningham announced the isolation of weighable amounts of pure element 95 from a five week old 9-gram sample of plutonium. Nearly 12 μg of 95-oxide has been isolated. A half-life of 490 years has been determined (by weighing) for this isotope of 95.

M. Freedman reported on fission yields of plutonium.

R. J. Hayden presented mass spectrographic analysis results obtained with samples of element 61 and ruthenium which give mass assignments of 147 for 1.2-year element 61 and 106 for ruthenium.

Ghiorso reported the fission properties of a number of heavy isotopes: Np^{237}, 95^{241}, 91^{233}, 91^{231}, 94^{240}, 88^{226}, and 88^{228} are found to be probably not fissionable while Th^{229}, Th^{227}, Ra^{223}, and possibly 96^{242} are measured as probably fissionable.

S. Gordon discussed recent experiments on radiation chemistry and the evidence of the Wigner effect in beryllia.

Helen and I had dinner at Ricardo's Restaurant with a group of news commentators and other individuals under the sponsorship of the Independent Citizens Committee for the Arts, Sciences, and Professors (CCASP). Following dinner I gave a short talk about the implications of nuclear weapons on the future of the world.

The B-29 named the Dreamboat established a new transcontinental record of five hours, 31 minutes, and 44 seconds flying from Burbank to New York yesterday.

Thursday, December 13, 1945

Weaver of the Chicago radio station WBBM called to verify the arrangements for my appearance on the Watson Davis "Adventures in Science" radio program on Saturday. I agreed to have an early lunch with him Saturday at the Wrigley Building where the WBBM studios are located. We will have a short rehearsal at 12:15 p.m. immediately followed by the interview. Weaver told me that the program will be broadcast on stations WISN in Milwaukee and WBD in Peoria but not on WBBM in Chicago. It will also be broadcast by 60-70 other CBS stations throughout the country.

Daniels and Branch issued a notice that December 24 and 25 and January 1 will be laboratory holidays.

Larry Magnusson and I travelled by train to Rock Island, Illinois, in order to present a talk, "A Discussion of Some Principles of Atomic Energy," to the Illinois-Iowa Section of the American Chemical Society at Augustana College. Arrangements were made for my talk by Professor J. P. Magnusson, Larry's father. Snow storms caused the train to be very late, and Larry and I arrived barely in time for the talk which was given in Room 102, Wallberg Hall. It is the first heavy snow of the season. I showed a first picture of a plutonium compound.

This morning's paper says that President Truman will ask Congress for legislation fixing price ceilings on old and new houses.

Friday, December 14, 1945

Larry Magnusson and I returned to Chicago from Rock Island by train.

Ted La Chapelle wrote, in a letter dated December 10, from Monsanto Central Research Laboratory to thank me for the revised copy of my talk at Northwestern University. La Chapelle says he is writing to Latimer about attending graduate school in Berkeley next fall.

In an official notice R. S. Mulliken announced that Herman H. Fussler has resigned and communications formerly addressed to him should now be addressed to Dr. Hoylande D. Young. Classified documents are no longer available in Room 217, Eckhart Hall, but are now located in Room 251, Ryerson Laboratory.

I wrote to Kennedy to inform him that, as stated in Lavender's letter of November 30, the original patent assignments of November 9, 1942, have been returned to me. I say that, with respect to the other things we asked for, we apparently so far have been unsuccessful. I ask for recommendations as to what might be the next best move, if anything.

Saturday, December 15, 1945

I called Harrison Davies at Clinton Laboratories to see if he is interested in accepting an academic position with our group at the University of California. Davies said he already has been offered an assistant professorship at the University of Chicago at a salary of $5,500 and he would have to be offered about $4,500 and an assistant professorship to consider the position. I told him that the program at the Radiation Laboratory would offer him much freedom in his research.

I replied to a letter from D. J. Salley of American Cyanamid Company that arrived while I was in Berkeley. Salley asked for possible candidates with experience in radioactivity for a position as a physical chemist. I suggest Howland, Hindman, Hagemann, and Katzin as possibilities.

In a letter to Dean George Whipple of the School of Medicine of the University of Rochester I give a brief statement about Bernard B. Brody who has applied for admission. I mention that I have already filled out a questionnaire for Brody, and I believe that he will be an excellent prospect for the medical profession.

I also filled out a rating sheet for D. S. Breslow for the Wyandotte Chemicals Corporation.

Helen and I had lunch at the Wrigley Building with John Dunn, the Chicago announcer of the Watson Davis "Adventures in Science" radio program. At 1:15 p.m., following a short rehearsal, I was introduced to the CBS radio audience as the guest scientist on the program. Following comments by Watson Davis by wire from Washington, I was interviewed on the direct broadcast from Chicago.

Announcer: Now, Mr. Davis, who is our "Adventures in Science" guest today?

Davis: Our guest today is one of the chemists who worked on the atomic bomb. That is distinction in itself, but even more important our guest is the discoverer of the two newest and heaviest chemical elements that are known to science. He is Dr. Glenn T. Seaborg, Professor of Chemistry of the University of California, who during the war has been working on the atomic bomb elements at the Metallurgical Laboratory of the University of Chicago. In this broadcast Dr. Seaborg will tell how the two heaviest elements, number 95 and 96, were discovered. Now to hear Dr. Seaborg, "Adventures in Science" takes you to Chicago.

Switch to WBBM, Chicago, at 2:19:45 p.m. EST

Announcer at WBBM: This is Chicago, John Dunn, speaking. Dr. Seaborg, we would like to know about the two new chemical elements that you discovered.

Seaborg: Before I tell you about the two new elements that I recently reported to the Chicago Section of the American Chemical Society, I think you ought to know that the atomic bomb research has actually added four new and heavier elements to the 92 elements of the periodic table. One of these, plutonium, number 94, is the stuff out of which the atomic bomb can be made.

Announcer: What's number 93, Dr. Seaborg? I know that number 92 is uranium.

Seaborg: Number 93 is called neptunium, after the planet in the solar system that comes next beyond Uranus. Of course, plutonium is named after the next planet, Pluto, the farthest out in the solar system, which was discovered only about a decade ago. Neptunium or element 93 is a stepping stone in the manufacture of plutonium from uranium of the most plentiful sort--that is the uranium isotope number 238.

Announcer: Then these two new elements, neptunium and plutonium, are extremely important in the whole question of atomic energy and the atomic bomb and the effect it is having upon our civilization.

Seaborg: They certainly are. And one interesting fact about plutonium is that when the immense plants constructed for its manufacture were planned and engineered, we who were doing the chemical work had actually never made enough of either neptunium or plutonium to be seen with the unaided eye.

Announcer: Yet these immense factories costing hundreds of millions of dollars were built to manufacture something that no one had really seen before.

Seaborg: That's right. And fortunately everything went along very well and it was possible, as the Japanese and the world know, to make plutonium by the pound. Plutonium is, as you know, one of the elements that when bombarded with slow moving neutrons or neutral fundamental particles of matter split with immense release of nuclear energy.

Announcer: So an element that never existed in nature, but was made to order so recently that it doesn't even appear in the textbooks, is very important in connection with the atomic bomb.

Seaborg: As a matter of fact, we found that while plutonium was first discovered in the laboratory, it does exist in minute quantities in a natural state. Dr. M. L. Perlman collaborated in this work in which it was shown that plutonium is being constantly manufactured in uranium ore in very small quantities by very much the same processes that are used to manufacture it in the laboratory. This is the first instance in which it has been necessary to make an element in the laboratory in order to prove its existence in nature.

Announcer: Now, Dr. Seaborg, how did you make 95 and 96?

Seaborg: We started out with uranium of the most common sort, weight 238. Both this 238 isotope of uranium and plutonium-239 were bombarded with high energy hearts of helium atoms called alpha particles. The bombardment was done in the cyclotron of Professor Ernest O. Lawrence at Berkeley, California. The new elements 95 and 96 were produced as a result. In order to accomplish this the cyclotron was rebuilt by Dr. Joseph G. Hamilton, Thomas M. Putnam, John R. Charles, Malcolm Webb, and their group so that it could produce alpha particles with an energy of 40,000,000 electron volts, the highest energy alpha particles ever produced. Very high energies are needed for charged particles to penetrate the nucleus of heavy elements like uranium and plutonium. Dr. Hamilton and his group performed

Seaborg: (cont.) the bombardments. The elements themselves were identified chemically at the Metallurgical Laboratory at the University of Chicago.

Announcer: This must have been a very tedious and difficult process.

Seaborg: It did involve the usual requirement of proving that the chemical properties of the two new elements are different from those of all the other 94 elements. I'd like to say that associated with me in the discovery of element 95 were Mr. Ralph A. James and Mr. Leon O. Morgan. The co-discoverers of element 96 were Mr. James and Mr. Albert Ghiorso.

Announcer: Weren't you also one of the discoverers of plutonium, element 94, Dr. Seaborg?

Seaborg: Yes, plutonium was discovered late in 1940 by Dr. E. M. McMillan, Dr. A. C. Wahl, and Dr. J. W. Kennedy and myself in researches at Berkeley. Just to complete the record on the discovery of the new transuranium elements, I should say that Dr. McMillan and Dr. P. H. Abelson discovered the first of these, neptunium, at the University of California in May, 1940. Of course, not too much was said about the discovery although it was announced at that time, because shortly after its discovery the curtain of secrecy was lowered on all atomic nuclear work in the researches which led to the atomic bomb. The neptunium that they discovered was, of course, a stepping stone to plutonium and the kind of neptunium they discovered has a very short existence, only two and one-third days life. The same team that discovered plutonium discovered another variety of neptunium, this one neptunium-238 which after a couple of days decays to the variety of plutonium of weight 238 with a half-life of about 50 years. It is this plutonium which was the first form of plutonium to be found.

Announcer: You certainly have been discovering a lot of the fundamental stuff of the universe, Dr. Seaborg, and the writers of chemistry texts are going to have quite a time keeping up with you.

Seaborg: Here is something else. There has always been a question as to what kind of family these heavy elements were going to form. We think we have pretty good evidence that the elements with atomic numbers larger than 88 actually form a new series like the rare earths that the chemists know about. And thus the elements actinium, thorium, and protactinium of atomic numbers

Seaborg: (cont.) 89, 90, and 91 are the first elements in this new rare-earth-like series which corresponds very much to the series of elements that begins with lanthanum, which is farther down in the scale of atomic numbers.

Announcer: If you have that sort of analogy, wouldn't it be logical to expect, Dr. Seaborg, that you will find some elements heavier and of higher atomic number than even the 95 and 96?

Seaborg: The question of the existence of even heavier elements depends entirely on whether the hearts, or nuclei, of these elements are sufficiently stable--that is, live long enough to enable one to detect them, but I think we have enough to talk about with these four new elements, haven't we?

Announcer: I guess you're right, Dr. Seaborg. It does sound pretty complex, and we better tend to the chemical babies that we do have. By the way, I'd like to know whether you have named these two new elements that you discovered.

Seaborg: Naming one of the fundamental substances of the universe is, of course, something which should be done only after careful thought. We've been faced with considerable difficulty in these cases because we have run out of planets. Naming neptunium after the planet Neptune and plutonium after the planet Pluto was rather logical. But so far the astronomers haven't discovered any planets beyond Pluto. So we will have to go to some other method of naming.

Announcer: What is that, Dr. Seaborg?

Seaborg: This hasn't been decided yet. One possibility might be to rely on some property of these elements. We do have an idea for the naming of element 95 along these lines and may have a suggestion to offer pretty soon. And by the way, you may be interested to know that we have received lots of suggestions--some good, some not so good.

Announcer: Dr. Seaborg, perhaps some of the listeners to "Adventures in Science" will want to make suggestions as to the names of the new elements. Would you be willing to have them write in their suggestions?

Seaborg: I won't promise to follow the suggestions but it might be interesting to know what the public thinks about naming new chemical elements.

Announcer: Very well, Dr. Seaborg. If you want to suggest names

Announcer: (cont.) for new elements 95 and 96, just drop a postcard to Watson Davis, Science Service, Washington 6, D.C. And to all those who write in Mr. Davis will send free a copy of the current issue of Chemistry magazine which contains Dr. Seaborg's full technical paper and a new arrangement of the chemical periodic table. It is free for the asking. Be sure to ask for the elements 95 and 96 article and address Watson Davis, Science Service, Washington 6, D.C.

Now for next week, "Adventures in Science" will tell how amateurs can operate radio stations and our guest will be Mr. George W. Bailey, president of the American Amateur Radio Relay League.

(System at 2:29:30 p.m. EST) You have been listening to "Adventures in Science" with Watson Davis, Director of Science Service... a CBS presentation...heard every Saturday, same time, same station. This is CBS, the Columbia Broadcasting System.

* * *

Report CS-3359, "Chemistry Division Summary Report for November 1945," was issued today. The material on Section C-I is identical to that submitted by Manning and Perlman to Hogness on November 21. Section C-II activities were reviewed by Hogness and include the status of the efforts on the high temperature pile, radiochemistry, and the analytical services.

Report MUC-FD-L-118, "The Metallurgical Laboratory Report for November 1945," was also issued and contains the following information of interest:

Under Physics and Metallurgy it is reported that a sample of the oxide of element 95 consisted of the dioxide and about 20 percent of another phase.

With a view toward preparing $PuCl_4$, the reaction of aluminum chloride with PuO_2 was studied. Only $PuCl_3$ was formed.

A new method of preparing $NpCl_3$ has been developed involving the passing of a mixture of $CCl_4 + H_2$ over NpO_2 and subsequent sublimation at 700°C.

In the section on Instruments it is noted that effort has been directed toward developing field-type uranium ore prospecting instruments for both surface and drill-hole use. Detecting instruments are being studied for measuring soft beta particles. It is hoped that before long C^{14} can be made available for use by scientists throughout the world for tracer work. It was pointed out that the loss of personnel from the Instrument Section continues at an alarming rate.

The information presented in the Chemistry section of the report was that covered in Report CS-3359.

It was noted in the Health section of the report that a 150 R whole body x-ray dose, given at 12.5 R per day, has produced leukopenia, anemia, and thrombocytopenia in individuals with mild chronic rheumatoid arthritis.

A summary of the Met Lab research program gave the following distribution of effort for the Chemistry Division during November:

Chemistry Division Section	Group	Area of Effort	Percent of Division Effort
C-I	1	Nuclear and chemical properties of the heavy elements	18
C-I	2	Control Analysis of uranium ore	7
C-I	3	Recovery of product	4
C-I	4	Redox solvent extraction process	14
C-II	1	High temperature pile	14
C-II	2	Radiation chemistry	4
C-II	3	Fission-product studies	5
C-II	4	Analytical chemistry	2
Writing for the Plutonium Project Record			30

The distribution of Met Lab employees as of November 30, 1945, (including 67 SED men) is as follows:

	Academic	Non-Academic	Total
Argonne Laboratory	34 (a)	8 (b)	42
Chemistry Division	78 (c)	18	96
Health Division	71 (d)	126	197
Physics and Metallurgy	15 (e)	8	23
Information	12 (f)	16	28
Patents	6 (g)	12 (h)	18
Services and Development	31 (i)	178 (j)	209
Scientific Administration	3	2	5
Associated Personnel (Project, Site Y, etc.)	6	4	10

cont...

	Academic	Non-Academic	Total
Administration			399
Security and Safety			149
Total			1176
Total excluding 67 SED men			1109

SED men included as follows:
(a)$_{9}$, (b)$_{1}$, (c)$_{26}$, (d)$_{11}$, (e)$_{1}$,
(f)$_{1}$, (g)$_{1}$, (h)$_{2}$, (i)$_{10}$, (j)$_{5}$ = 67 total

The total expenditures for the month of October 1945 were reported at $435,182 as compared with the estimated budgets of $473,700 for November 1945, and $511,400 for December 1945.

The temperature fell to 10°F at 2:00 this morning, and the forecast is for 0°-5°F tonight.

Sunday, December 16, 1945

Perlman and I left at 8:00 p.m. for Ottawa on the New York Central. We changed trains in Toronto.

The temperature high in Chicago today has been 9°F between 2:00 and 3:00 p.m.

Monday, December 17, 1945

Perlman and I checked into the Chateau Laurier Hotel when we arrived in Ottawa.

Chicago temperatures are scheduled to have a high of 14°F today; here in Ottawa it is really cold!

Tuesday, December 18, 1945

Perlman and I visited the Ottawa Laboratory of the National Research Council during the morning. We had meetings and discussions with Goldschmidt, Mungen, Cook, Steacie, Guéron, and Mills. George Weil, the U.S. representative to the Canadian Nuclear Energy Program, also attended. We primarily discussed the proposed process change for the 23 separation process involving the removal of the MnO_2 precipitation step and a change in the extraction solvent.

Mungen described his work on the separation of U^{233} and Pa^{233} from thorium and later showed us the separation process plant equipment set up in Ottawa.

Goldschmidt talked about his work on the separation of plutonium from neutron-irradiated uranium and fission products; however, we could not divulge any information on our USA process. We saw the equipment set up to test their plutonium separation process.

Before leaving the Ottawa Laboratory, Mills showed us the blueprints and plans for the Chalk River site and the details of the separation plant.

During the afternoon we were driven to the Deep River townsite near Chalk River where we spent the evening. It is still very cold!

Wednesday, December 19, 1945

Guéron, Goldschmidt, Spence, Mungen, and Cook accompanied Perlman and me on our tour of the chemistry building and hot laboratories at Chalk River. C. W. Gilbert showed us the "Zeep" heavy water-uranium pile, now operating at 1 watt of power which has the possibility of going up to 1 kw.

In the afternoon I talked to the assembled research staff. I discussed the method of formation and some of the chemical and radioactive properties of elements 95 and 96. The audience was very interested in these results. I described the relationship of these elements to other members of the actinide series in a qualitative way. The production of U^{233} was described along with the various side reactions encountered in the process.

Tongue and Pontecorvo then showed us the pile being built under their direction for possible start-up next March. It will operate at 10,000 kw first and then may go up to 20,000 kw with an estimated flux of $2 \cdot 10^{13}$ n/sec/cm^2 (30 times Argonne pile). The pile consists of 11 tons of uranium and 17 tons of heavy water.

We were also told about the projected British Laboratory, the Harwell Laboratory situated at Didcot, a town five miles from the Laboratory and about twelve miles from Oxford. John Cockcroft will be the Laboratory Director while Bob Spence will be in charge of the chemistry program.

I talked to Pontecorvo about joining our group at the Radiation Laboratory in Berkeley. Pontecorvo said that his present salary at CRL is $6,000 per year but it is going up to $7,200 on January 1 and that he has been offered an associate professorship at the University of Michigan. He agreed that a professorship at Berkeley at $6,000 would probably be satisfactory and that he could be available to start next September. He will let me know within a month or two.

After a reception and dinner, Perlman and I went down to the railroad station at midnight to catch the night train to Montreal.

The House of Representatives passed the United Nations organization bill yesterday. The bill will now go to committee to resolve minor amendment differences between the House and the Senate.

Thursday, December 20, 1945

The Transcontinental Canadian Pacific train, which originated in Vancouver, was about four hours late. Perlman and I stood in a very crowded, overheated, tiny station. The only way to obtain relief from the overheated station with its foul air was to venture out occasionally into the sub-zero degree weather. The train finally came along about 4:00 a.m., and we boarded it for Montreal.

Late in the morning we arrived in Montreal where we toured the National Research Council Laboratory and talked with the scientists.

A. C. English and Cranshaw described the work that they have done on tracing out the 4n + 1 decay series.

During the day we learned much about the ion chambers, counters, and related electronics used on the Canadian project, much of which appears to be very original and ingenious.

Guéron told me that he will return to France in a few weeks where he will work on the French nuclear project under the direction of Joliot. Goldschmidt is not returning to France immediately but perhaps he will in about a year. Leslie Cook is slated to become the chief of the chemistry program on the Canadian project when Goldschmidt leaves. Steacie is in charge of the chemistry for the whole Canadian National Research Council and is also Deputy Project Director of the Canadian nuclear project.

Perlman and I took the Canadian Pacific train from Montreal to Toronto where we changed trains to Chicago.

The Chicago Tribune carried an item about a speech made by Arthur Compton in Chicago yesterday. He said that cooperation must be achieved and, "We have to learn to live together or we won't live."

Friday, December 21, 1945

Events in Chicago during my absence:

Monday, December 17

A letter dated December 11 from Kennedy arrived. He enclosed a draft which he proposes to send to Lavender over our signatures in reply to Lavender's refusal to provide us with advance copies of the patent agreements being considered by the University of California and copies of the patents.

Kennedy said he will move to St. Louis at the end of this month, and Art Wahl and Sam Weissman will arrive there sometime in January, to take up their positions at Washington University.

The Laboratory Council met at 9:00 a.m. in Room 209, Eckhart Hall. Present were Branch, Cole, Daniels, Dempster, Foote, Furney, Hilberry, Hogness, Jacobson, Jesse, Lapp, Manning, Moulton, Stone, Willard, Wilkinson, G. Young, H. D. Young, Zachariasen, and Zirkle. Daniels made several announcements including that work on the PPR has been assigned top priority by Colonel Nichols.

Procedures and possible extra medical leave requirements for taking urine samples were discussed. General Groves has directed complete compartmentalization between the Met Lab and Site Y to continue.

Hilberry reported that plans for a Regional Laboratory are progressing. The assets of the Met Lab may be transferred to the Regional Laboratory and a scientific board may supervise the work. A four-week vacation period and upward salary adjustments are being considered, according to Daniels and Hogness.

Tuesday, December 18

There was an 8:28 a.m. meeting of Groups 1 and 3 of Section C-I in the New Chemistry conference room. Those present were Asprey, Britain, Cunningham, Florin, Ghiorso, Hagemann, Hopkins, Hyde, James, Katzin, Kohman, Magnusson, Manning, O'Connor, Osborne, Peterson, Sedlet, Simpson, Stewart, R. Thompson, S. Thompson, Van Winkle, and Westrum. Manning presented the current schedule for bombardments. The 95^{241} bombardment will be completed this week, and the sample should arrive on December 21. The U^{233} sample should begin its bombardment on December 20 and may not arrive in Chicago until after January 2, 1946. A 1 mg sample of protactinium will be sent to Clinton for neutron bombardment to provide U^{232} for measurement.

Katzin reported that recent experiments at Argonne to determine the fission cross section of Pa^{232} gave anomalous results and suggest contamination by U^{235}. New sample plates will be prepared.

Peterson said that Th^{227} and Ra^{223} were tested and both appear to undergo slow neutron fission with a cross section of 470 barns or greater. Thorium-229 was found to fission with a fission cross section of 50 barns.

Ghiorso gave his calculations (with Studier's data) which indicate a half-life of 5,700 years for Th^{229}.

James reported that very low limits could be set on the amounts of Pa^{230} and U^{230} produced in the U^{235} plus helium ions bombardment. Therefore, any Pa^{230}, as observed by Studier and Hyde, in the U^{233} plus deuteron target must be due to d,αn reactions rather than alpha decay from Np^{234}.

Wednesday, December 19

Hindman wrote a description for me of the method he and Britain selected and used to prepare ten Np^{237} samples for Hughes. The neptunium was electroplated over a circular area one and one-half inches in diameter on two-inch diameter burnished platinum discs. Various currents, voltage, and electrolyte concentrations were tried. From the results it appeared that the best conditions for plating is with 0.1 millimole of ammonium oxalate, enough NH_4OH to neutralize the solution, and a current of 0.5 amp. Under these conditions they found the yield to be almost quantitative.

Stewart received a call from Rodin about Stewart's request on December 13, that five cans of thorium carbonate (1, 2, 3, 4, and 5) be removed from the pile. Stewart had said that we had assumed that the cans had already been removed, but he recently learned the cans were still in the pile. Rodin reassured Stewart that the cans had been removed in October and will be shipped to us as soon as possible.

Roy Thompson wrote to Everson at Berkeley to clarify points about moving expenses connected with his planned transfer to the University of California.

A thank-you letter arrived for me from Watson Davis for my radio performance on his program. He would like a "pre-release" statement about the naming of elements 95 and 96.

Thursday, December 20

A meeting of the Solvent Extraction Group of Section C-I was held today. Present were Ader, Billheimer, Blaedel, Callison, Friedman, Goeckermann, Hausman, Hyman, Kelley, Lawroski, Leader, Manning, Schaffner, Schraidt, and Sheft. (We recently received two additional SED men for this group--H. G. Callison and A. S. Friedman.) Ader reported in detail on the results of solvent extractions runs IIC3P2F and IIC5P4F. Beta and gamma decontamination factors for both runs (after two cycles) were about 2×10^7.

Goeckermann said work on the hexone-hydrazine recovery problem was completed. More than two-thirds of the hexone, essentially free of hydrazine, can be recovered using steam distillation in a fractionating column. The remaining one-third can be steam distilled carrying the hydrazine with it if the aqueous layer in the distillation is made 1 N in NaOH. Overall, 98 percent of the hexone and more than 90 percent of the hydrazine can be recovered for possible re-use. No fission product activity is distilled over.

Blaedel described the results of two second-cycle runs made on 300-day irradiated and 10 and 20-day cooled Clinton slugs. The data showed the decontamination and plutonium recovery obtainable by the Redox Process using very short cooling times.

Schaffner reported that there will be no runs carried out this week or next, partly because of the year-end holidays and also because there is much clean-up, decontamination, and general maintenance work to be done.

Paul O'Connor terminated work at the Met Lab. He will join the group at Berkeley January 2.

A letter, dated December 16, arrived from Miss N. A. Parkinson to Chemical and Engineering News thanking me for agreeing to review "The Story of the Atomic Bomb" by William L. Lawrence. She said the booklet is being sent to me under separate cover. The review should not exceed 200-300 words.

A personnel rating sheet and recommendation was submitted by Manning to promote Cunningham from his present title as Chemist to that of Senior Chemist. A $34 per month salary increase was also recommended which would bring his salary up to $450 per month.

The following telegram arrived for me from Robert G. Sproul:

> 1945 Award for Chemical Engineering Achievement to be made to University of California and other universities and companies at banquet in New York February 26th. Will you represent us and receive award. Details will be sent if you can accept. Christmas Greetings. Please reply collect.

* * *

Perlman and I arrived in Chicago at 8:00 a.m.

Dempster called to tell me that he has run our sample, which consists of a mixture of thorium and ionium isotopes, in a mass spectrograph and has determined the isotopic composition through use of a photographic detection method. The results showed that the sample consists of 26 percent ionium and 74 percent thorium. He will verify the calibration of his photographic method later.

I also read a memo from Dempster dated December 17 which arrived while I was away. Using two new Lindemann electrometers, Dempster and Rall remeasured the relative abundances of 28 and 23 in the sample used by Zinn last spring. The new results give a value of 96.35 percent for 23 and a value of 3.65 percent for 28.

Foster York also called. He wanted to discuss my recollection of the conception of the hot wire method for making plutonium metal. Ted Magel believes he is the inventor of this method and has cited a conversation with me in order to establish a conception date. I recalled for Foster my recollection of the conversation. One problem is to resolve a conflict with Prescott of Berkeley as to who was first to conceive this method of metal production.

I wrote Segrè at Site Y to inform him that I have signed and forwarded to Lavender and Bush the letter which Kennedy sent me. I also mention I talked with Pontecorvo in Canada this week and that he is quite interested in transferring to our group at Berkeley.

Temperatures are still low in Chicago with a high of 16° at 3:00 p.m. today.

Saturday, December 22, 1945

Jaffey has just returned from Washington, D.C., where he met with various groups. He requested that Perlman and I prepare a report regarding the feasibility of international inspection, so far as chemical plants are concerned, for use by a Senate Committee. Others will prepare reports on the physical and other aspects of international inspection. The report is due at the end of January 1946.

I immediately wrote to Perlman, who left yesterday to return to Berkeley. I tell him of my conversation with Jaffey and also inform him that Daniels and Furney have given me permission to ship a large list of materials to Berkeley for our use there. I say I have asked Al Florin if he is interested in working on the Van Atta job.

Stewart sent a memo to Whitaker at Oak Ridge saying that we are sending a sample of 1.1 mg protactinium as oxide to him for irradiation in a position of high flux for one month. The sample is labeled "GTS 125."

In a memo to J. R. Gibson, Manning asks for approval of a revised work schedule for Lorraine Eisen. Manning also recommends a normal salary increase for her.

Manning prepared and sent to Hogness today the "Summary of Work of Section C-I for Period November 15 to December 15, 1945" (MUC-GTS-2109). The report gives the following:

I. Heavy Isotopes Work

A. Thermal Neutron Fission of Th^{227} (Radioactinium)

Thorium-227 (radioactinium), a 10-day alpha-emitter, was separated from an impure preparation of protactinium which has stood for several years. A sample of Th^{227}, weighing approximately 0.001 microgram, was tested for fission with thermal neutrons in the thermal column of the heavy water moderated pile at Argonne. The material was observed to be fissionable with a cross section of 450 to 500 b.

A small amount of Ra^{223} (actinium X), the 11-day daughter of Th^{227}, was also isolated and tested for thermal neutron fission at Argonne. This isotope appeared to be fissionable with a large cross section (approximately 1,000 b) but because of the small size of the sample (less than 10^{-4} microgram), the results cannot be considered conclusive.

B. Further Measurements on Thermal Neutron Fission of Th^{229}, Half-Life of Th^{229}

A sample of Th^{229} (produced by alpha decay of U^{233}) which has previously been tested for thermal neutron fission was again purified and retested in the thermal column of the Argonne pile. A cross section of 52 ± 10 barns was observed for fission of Th^{229} by thermal neutrons. This value is in agreement with the approximate values reported earlier.

A redetermination of the half-life of Th^{229} gives a value of approximately 5,700 years (± 10 percent). This value was used in calculating the fission cross section of 52 barns.

C. Helium Ion Bombardment of Np^{257}

A 15 mg sample of neptunium was bombarded with 44 Mev helium ions in the Berkeley 60-inch cyclotron for 115 microampere-hours. Several isotopes of element 95 were presumably formed in appreciable quantities as a result of α,n, α,2n, α,3n, α,4n, and perhaps α,5n reactions. However, only a single activity probably attributable to an isotope of element 95 was actually detected in the 95-96 fraction from this bombardment. There was activity present which decayed with approximately 1-day half-life, characterized by energetic electrons (~1 Mev), gamma-rays (~1.4 Mev), and a very few x-rays. However, because of the difficulty of separating element 95 from rare earths, the possibility that all or part of this activity is due to fission products cannot be ruled out.

There was no detectable alpha activity due to isotopes of 95 or 96 in the original 95-96 fraction, nor was any alpha activity observed to grow in this fraction. This observation appears to eliminate the possibility of 95^{240} being a beta-emitter of less than 60-day half-life, since otherwise the 4.95 cm alpha activity of 96^{240} should have been observed. The lack of alpha activity in the 95-96 fraction also appears to rule out the possibility of 95^{240} being an alpha emitter with a half-life between one day and ten years.

A neptunium fraction separated from the sample within two days after the end of bombardment contained a considerable quantity of long-lived (greater than three months) x-ray activity. It is probable that this is due to Np^{235}, which could have been formed by a rapid alpha decay (less than one day half-life) of 95^{239}. This isotope must have been produced in considerable quantities as a result of an α,2n reaction on the Np^{237}.

A plutonium fraction separated after the bombardment contained Pu^{236}, as evidenced by a 4.3 cm alpha activity, and relatively large amounts of Pu^{239}, most of which had been present as contamination in the original Np^{237} before bombardment. Plutonium-238 was not detected, although the large amount of Pu^{239} activity made the sensitivity for Pu^{238} detection rather low. The Pu^{236} could have been formed by any one of, or a combination of, the following three reactions:

1. $Np^{237}(\alpha,\alpha n)Np^{236} \xrightarrow[\text{18 hour}]{\beta} Pu^{236}$

2. $Np^{237}(\alpha,p4n)Pu^{236}$

3. $Np^{237}(\alpha,5n)95^{236} \xrightarrow[\text{short}]{\text{orb. } e^- \text{ cap.}} Pu^{236}$

No evidence was found for x-ray activity in the plutonium fraction. However, neither Pu^{235} nor Pu^{237}, one of which is presumably a 40-day x-ray emitter, would have been formed in sufficiently high yield to be detected.

D. Helium Ion Bombardment of U^{235}

A 300-mg sample of uranium (87 percent U^{235}, 13 percent U^{238}) was bombarded for 540 microampere-hours with 44 Mev helium ions in the Berkeley cyclotron. The sample was examined soon enough after the end of bombardment (1.5 days) to permit the first direct observation of the 18-hour beta-emitting Np^{236}, formed in this case as a result of the reaction $U^{235}(\alpha,p2n)Np^{236}$. The decay of the beta activity and the growth of 4.3 cm alpha activity (due to Pu^{236}) in the neptunium fraction agreed in giving a half-life of 18 hours for Np^{236}. The plutonium fraction from this bombardment contained Pu^{238} [formed by $U^{235}(\alpha,n)Pu^{238}$ and $U^{238}(\alpha,4n)Pu^{238}$] and Pu^{236} [formed principally by $U^{235}(\alpha,3n)Pu^{236}$ as well as beta decay of Np^{236}]. As expected, 40-day x-ray activity, due to either Pu^{235} or Pu^{237}, was also observed in the plutonium fraction. Because the expected yields for α,2n and α,4n reactions on U^{235} are of the same order of magnitude, it is not possible from yield considerations to make a definite assignment of the 40-day activity.

In a report on an earlier helium ion bombardment of U^{235} (MUC-GTS-1970), it was stated that there was some evidence for a plutonium activity with a half-life of the order of ten days to two weeks. This has not been verified in the present bombardment where observation of decay on the plutonium fraction began much sooner after the end of bombardment; no evidence has been found for a ten-day to two-week activity.

E. Helium Ion Bombardment of Pa^{231}

A 4 mg sample of Pa^{231} was bombarded in the Berkeley cyclotron with 44 Mev helium ions for about 200 microampere-hours. The neptunium fraction separated after bombardment contained a four to five day component, characterized by hard gamma-radiation and x-rays. This is presumably due either to Np^{233} [from $Pa^{231}(\alpha,2n)Np^{233}$] or Np^{234} [from $Pa^{231}(\alpha,n)Np^{234}$]; a similar activity was previously observed in a deuteron bombardment of U^{235}.

The uranium fraction showed a large amount of alpha activity characteristic of the U^{230} series, therefore, it was not possible to detect shorter range alpha particles from other uranium isotopes which were presumably formed in the bombardment. The U^{230} would be formed directly as a result of an α,p4n reaction on Pa^{231}, and indirectly by decay of Pa^{230} and Np^{230}.

The 4½-5 day x-ray activity previously assigned to U^{231} was also observed in the uranium fraction from this bombardment. Uranium-231 was presumably formed by the reaction $Pa^{231}(\alpha,p3n)U^{231}$ and perhaps also by decay of Np^{231}.

II. Redox Solvent Extraction Process

Several additional runs were made which, except for about a 0.7 percent increase in plutonium yield, substantially verified the results reported last month. At the present time it can be stated that it has been proved experimentally that the process is capable of giving the following performance: (1) a 99 percent overall yield of plutonium with a decontamination factor of 10^7 with respect to uranium, to beta fission product activity, and to gamma fission product activity; (2) a 99.8 percent overall yield of uranium with a decontamination factor of better than 10^4 with respect to both beta and gamma fission product activities.

Absorption measurements and specific fission product analyses indicate that after the 10^4 decontamination level obtained on plutonium at the end of the first cycle, the predominant gamma activity is zirconium; and that after the 10^7 decontamination level obtained at the end of the process, the predominant gamma activity is ruthenium and the predominant beta activities are cerium, ruthenium, and plutonium.

A two-cycle run was made on 300-day irradiated Clinton slugs which had been cooled only ten days and in which therefore 2.3-day Np^{239} was present at fairly high levels. It was found that neptunium followed the uranium almost quantitatively into the column IC uranium solution, with a yield of over 99 percent. The beta and gamma decontamination factors for the uranium were therefore poor, being of the order of 10 and 200, respectively. Decay curves indicated that the activity remaining with the uranium was due to neptunium. The beta and gamma factors in column IB plutonium solution were close to 10^4 for the ten-day cooled slugs, and even here the decay indicated that a good portion (greater than 50 percent) of the activity was due to the traces of neptunium, which followed plutonium in the first cycle. In the second cycle run made with this solution, the final plutonium solution showed poor beta and gamma decontamination, the values being 4 and 20, respectively and thus indicated that neptunium follows well with the plutonium in the second cycle.

Batch work indicates that neptunium cannot be made to go with plutonium in the first cycle by any simple modification of the process, and also that it may be recovered from the column IC uranium solution by a direct LaF_3 precipitation.

This morning's paper carries the news of General Patton's death yesterday. Death was due to injuries sustained in an auto accident December 9.

Sunday, December 23, 1945

"Moscow Parley Fails to Share Atom Secret" is the title of an article in the paper with the dateline of December 23. The foreign ministers of the United States, Great Britain, and the Soviet Union are meeting in Moscow. Atomic questions are one of the major items on the agenda.

Monday, December 24, 1945

Helen and I went to the Osbornes to see the children in the late afternoon. In the evening we went out for a snack; walking was difficult as it was icy and slippery.

Tuesday, December 25, 1945

We had a wonderful Christmas dinner at the Thompsons' apartment; the Cunninghams were also guests.

According to this morning's paper the United States, Great Britain, and the Soviet Union have agreed on procedures for the peace treaties for Italy, Romania, Bulgaria, Hungary, and Finland.

Wednesday, December 26, 1945

I wrote to David Lipkin at Site Y to give some suggestions for the preparation of plutonium targets to be provided by Los Alamos for bombardments.

In a letter dated December 20, Everson reminded me that Eugene Huffman is in need of two or three good inorganic chemists for analytical work on the hill. He encloses some Personnel Security Questionnaires for use in case I find someone for this work.

Daniels sent Hood Worthington a detailed letter about the possible use of the new Redox Solvent Extraction Process for recovering waste uranium now stored at Hanford. Daniels emphasizes that the advantages of the Redox Solvent Extraction are so great that Hanford should consider early replacement of the Bismuth Phosphate Process with the Redox Process. Although recovery of uranium waste may be complicated by the present $BiPO_4$ and other chemicals now in the solutions, sufficient preliminary treatment should take care of the problem.

Daniels says that Lawroski is the key man in the solvent-extraction development and at present is available only on a half-time loan basis from the Standard Oil Company of New Jersey. After March 1 we may not be able to keep him. However, Lawroski is very interested in carrying this process through to a full-scale installation at Hanford; Daniels suggests that du Pont may wish to consider a cooperative arrangement with Standard Oil to obtain Lawroski's services at Hanford.

Under a December 26 dateline the paper says that the foreign ministers conference has ended. Many topics were discussed including conversations about the atomic bomb. Rumors are reported that the discussions dealt only with creation of intergovernmental machinery for dealing with control of atomic weapons and not with questions relating to the science or manufacture of the bomb.

Thursday, December 27, 1945

T. S. Chapman called me about the interest Los Alamos has in obtaining a sample of U^{233}. Chapman told me about an earlier telephone conversation he had with Captain Cox of Los Alamos at which time Cox said Colonel Nichols wanted to know how much U^{233} might be available for some special experiments at Los Alamos. Chapman said he has informed Cox about plans to produce and isolate several hundreds of grams of U^{233} at Hanford.

Chapman and I discussed the planned Hanford production program for U^{233} and made estimates as to what would be required to produce 200 to 300 grams of the isotope. Chapman is also interested in sources of C^{14} and tritium.

I talked with Spof English by phone about several people at Clinton who might be interested in joining our group at Berkeley. English will talk with such people as Byerly and Nelson in the analytical division and Ray Edwards, Howard Gest, Orville Hill, and Nate Ballou. English mentioned that Joe Halperin currently is interested in accepting a teaching assistantship at the University of Chicago and John Hunt wants to say in the East.

I also called Iz Perlman to tell him that Templeton and Howland are soon ready to move to Berkeley. Perlman will speak with Everson about the necessary arrangements. Iz mentioned that Burgus apparently is going to Berkeley.

In a letter to Watson Davis, I tell him that I enjoyed being on his program. I say that, since the program was not broadcast in Chicago, a number of people have asked me if a transcription is available. I also offer to send him advance information on the naming of elements 95 and 96. I say that I am sending him a copy of a talk earlier this month containing a chart of isotopes which the Security Office cleared. This contains the mass numbers of isotopes of elements 95 and 96.

Westrum sent Perlman, with a copy to me, the requirements and suggestions for a design of a micro-metallurgy laboratory at Berkeley.

"Fire in Mine; Feared 30 Dead" reads this morning's top headline. There was an explosion in a mine in Kentucky believed to have resulted from coal dust or gas.

<u>Friday, December 28, 1945</u>

Don Cooksey called to inform me that Eugene Huffman will be attending a meeting of the analytical chemists of the Manhattan Project in Washington, D.C., on Tuesday, January 8. He will leave Berkeley by train on January 5. Huffman will then go to Oak Ridge and visit the Y-12 plant to study the chemical methods used in connection with the electro-magnetic method for separating isotopes. Don asked that I arrange for Huffman to visit Clinton Laboratories also, if this seems desirable. After the trip to Oak Ridge, Huffman will visit the Met Lab on January 14 and 15.

I told Don about my recent conversations with Pontecorvo about the possibility of his joining our group. I also said that Jerry Howland has decided to accept a position with us. Don said that final action on this must be delayed until Lawrence returns to Berkeley from southern California, where he is now recuperating from pneumonia.

I wrote to Roy Heath to tell him I would like to accept his invitation to visit the Wyandotte Chemicals Corporation facilities in Detroit. I say, however, the morning of January 18 is the only time I have available for the visit.

I also replied to the letter I received from Mulaika and Bert Corben (Bert spent a year at U.C. Berkeley), who are now in Australia, in which they credited me with the discovery of four elements. I tell them that, unfortunately, I had nothing to do with the discovery of neptunium but was lucky to be involved in the work on the elements of numbers 94-96 inclusive. I conclude with some personal items.

Other correspondence today includes a letter to C. K. Hunt at Wyandotte, Michigan, to thank him for making arrangements for my trip to Detroit on January 17. I also sent James Grady information about the talk I will give to the Detroit Section on January 17. In another letter I tell J. Edwin Pasek of Prentice-Hall that I was not impressed with the manuscript on atomic power he has asked me to review. It is full of errors, I say, and part of it is a rewrite of the Smyth Report. In addition, I am not convinced that publication of the manuscript would serve any useful purpose.

T. O. Jones sent Lieutenant Raymond Chisholm a very complete history about the discovery and determination of properties of elements 95 and 96. This is an excellent summary and gives names, dates, events, and report and notebook sources.

Today's newspaper carries the text of the agreement of the foreign ministers' meeting in Moscow. Among the principal items the paper reports that

> atomic energy is to be controlled and the atomic bomb outlawed as a weapon of war through the operation of a commission consisting of the U.S., Great Britain, Russia, France, China, Canada, and five or six small nations under the supervision of the Security Council of the United Nations organization.

Saturday, December 29, 1945

Spof English called me with more information about men at Clinton who might be interested in going to Berkeley. Byerly is not interested but suggested Jensen of the analytical group. English recommended both Steahly and Cecil Nelson highly, but Steahly does not want to stay in analytical chemistry. Dan Koshland has definitely decided to go to the University of Chicago. English said that Ballou, Art Adamson, and Henri Levy are also interested in learning more about the possibilities in Berkeley. Hill has been sick, so Spof has not been able to talk with him.

Tom Morgan wrote me from Berkeley to give the status of the remodeling of rooms 109 and 110 in Building 4 in the Radiation Laboratory on the hill into adequate radiochemical facilities. Tom says that Barton and Reynolds have been exceedingly helpful. In a separate, classified, memo he gives the log of the cyclotron bombardments: The 95^{241} target which was bombarded with 44 Mev alpha particles from December 7 to December 20 received a total of 158.8 microampere-hours. The backing plate for this target, U^{238}, received a total of 336.5 microampere-hours. The bombardment of this uranium plate is continuing by itself until the U^{233} interceptor target is put on the cyclotron. The U^{233} target bombardment should begin about January 7, 1946.

Tom also mentions that Betty arrived on December 20 and they are busy getting settled.

A telephone conference call was held among Chapman, Agruss, and me in order to discuss the apparent discrepancy in the quantity of Pa^{231} found in the sample Agruss sold us. He told us about his method of analysis, and I described our method in detail. Agruss agreed that our method was much more accurate and accepted our assay of 28 mg as the basis for payment. Chapman told Agruss that the Army would like to obtain 10 millicuries of actinium from him. Agruss will check its availability.

I sent Daniels an additional memo (MUC-GTS-2115) about the use of U^{232} and other material in weapons components (see memo MUC-GTS-2038, October 23, 1945). The District Engineer's Office pointed out that the neutron background arising from the Be(γ,n) reaction is 50 times higher than the present initiator and might cause predetonation. In addition, the gas phase of thoron in the decay chain might cause problems. I suggest to Daniels that another material other than beryllium, such as boron, lithium, C^{13}, or fluorine, might be considered for use with U^{232}. I feel that the gas phase problem could be taken care of through proper gadget design.

I stress the point that the potentialities for the use of Pa^{231} as a source of U^{232} appear sufficiently great so that steps should be taken to arrange for its recovery in the uranium processing procedures. It is equally important to recover Io^{230}.

I state there is another isotope, Ac^{227}, that might be suitable for use as an initiator. I give the decay chain which appears to have no gamma-rays of appreciable abundance to cause a serious problem from γ,n reactions on beryllium. However, the series does contain a gas, actinon. To separate Ac^{227} from uranium would be difficult since it would require processing about 8,000 tons of uranium to obtain about 100 curies of Ac^{227}. However, Ac^{227} might be produced by neutron capture in Ra^{226} according to the following reaction:

$$Ra^{226}(n,\gamma)Ra^{227} \xrightarrow[\text{short}]{\beta^-} Ac^{227}.$$

One hundred grams of Ra^{226} irradiated at Hanford for six or eight months should result in the production of about 0.25 g of Ac^{227} or about 150 curies of alpha-particle activity. I point out that we are planning to make about 100 mc of Ac^{227} and are awaiting the acquisition of about 1 g of radium to irradiate at Hanford, if authorized by the Army office.

I emphasize that the preparation of about 100 g or more of Np^{237} should be a definite goal as it would be a good source of the 50-year alpha-emitting Pu^{238}. I state that Np^{237}, now being essentially thrown away at Hanford, should be regarded as a national asset and saved. The Redox Solvent Extraction Process as a substitute for the present Hanford process would offer an excellent method for retrieving the current production of Np^{237} at Hanford.

As a last comment in my memo to Daniels, I state:

> It also seems worthwhile to point out again that in future high-energy piles transplutonium isotopes might be produced in such amounts that they could be used for this and other purposes. In particular, it seems that the isotopes 96^{243} and 96^{244} might have radioactive properties such as to make them interesting, that is, will be alpha emitters with half-lives in the range of a few years or tens of years.

My correspondence today includes a letter to George Everson, in which I suggest that David Templeton and Jerome Howland would be two very good inorganic men for Eugene Huffman's group. I enclose Templeton's Personnel Security Questionnaire and say that Howland's will be forwarded soon. I emphasize that we should continue to maintain the highest standards and to proceed slowly, if necessary. I then wrote to Howland to tell him that he should wait until he hears from Everson before going to California.

I received a letter in the beginning of December from E. S. Proskauer of Interscience Publishers, Inc., reminding me of pre-war correspondence about my writing a book for them on nuclear chemistry. Proskauer asks whether I can envision writing such a book in the near future. Today I replied that I am interested, but I would be unable to begin such a venture until a year to two from now.

I answered a letter dated December 18 from Walter Yust of Encyclopedia Britannica. He asks that I prepare articles on uranium, thorium, artificial radioactivity, protactinium, plutonium, neptunium, and actinium. I agreed to do this and say that the fall of 1946 would be a practical deadline.

I sent Watson Davis, in response to his request, colored transparencies of plutonium hydroxide, one of the first pure compounds ever isolated, and neptunium dioxide, the first pure compound isolated (June 20, 1944).

In a letter to D. J. Salley, American Cyanamid Company, I inform him that I have given his application forms to Katzin, Hindman, Hagemann, and Howland. I also give him the requested address for Robert B. Duffield.

The last letter today went to Captain P. J. Sykes, Jr., on Guam, in reply to his letter of November 25. I apologize for having delayed my response. I send him a list of references of information on nuclear science and transuranium elements, and enclose a copy of one of my recent talks to answer his questions about neptunium and plutonium. I then say I am not free to answer some of his queries because of security restrictions.

"Survey of the Dry Chemistry of Neptunium," (CN-3318) by Sherman Fried was issued today. This report contains a list of the known compounds of neptunium, a description of their properties, and a short summary of the methods of preparation. It is based on material in other reports and on a paper to be submitted to Volume 14B of the PPR.

Eight miners were rescued from the mine in Kentucky which had the big explosion a couple of days ago.

<u>Sunday, December 30, 1945</u>

Secretary of State Byrnes made a radio address tonight defending the Moscow agreement as being the best possible compromise attainable. The paper says, "The Secretary took pains to dissipate anxiety that the atom bomb secret has been given to the Russians at the Moscow conference." At no time, he said, were technical or scientific matters discussed and the Soviet government asked no questions about the bomb.

<u>Monday, December 31, 1945</u>

Harrison S. Brown wrote to tell me that he has decided to accept a position with the University of Chicago. He hopes that a cooperative arrangement can be worked out between our group in Berkeley and the chemists at Chicago. Harrison then thanks me for the help I have given him in the past and said, "I have learned more nuclear physics and chemistry from you than from any other individual."

Daniels requested today, in three separate memos, that Captain T. S. Chapman arrange for the following irradiations in the Hanford pile: 5-mg sample of Np^{237} (several months), 10 gm of pure U^{238} (15-30 days), CW-3 and CW-4 samples, still in original container (to be given additional six months' irradiation).

Helen and I celebrated New Year's Eve by seeing "State Fair" at the Picadilly Theater at 51st and Blackstone Streets. We then had a toasted butterhorn at our favorite little snack restaurant on 55th Street.

This morning's paper says that the New Year's Eve celebration will be the greatest in history. One night club operator says that it will be "atomic."

JANUARY 1946

Tuesday, January 1, 1946

Today, New Year's Day, was quite cold with a trace of snow. The high temperature for the day was about 19°F with a low around 7°F.

The academic manpower distribution in the Chemistry Division as of January 1946, is as follows:

Section C-I - G. T. Seaborg, Section Chief	
Heavy Isotopes Group - Seaborg	26
Services Group - Asprey	2
Solvent Extraction - Lawroski	18
Administration - Seaborg, Manning, Stewart	3
Total for Section C-I	49
Section C-II - T. R. Hogness, Acting Section Chief	
Pile Research - Daniels (Willard)	14
Radiation - Allen	4
Fission Products - Rubinson	4
Analytical - Templeton	8
Administration - Hogness, Willard, Zarse	3
Total for Section C-II	33

This gives a total of 82 academic employees of which 27 are SED men.

Today's Chicago Tribune carries copies of its front pages of important dates during World War II, including August 7 with the banner headline "Atomic Bomb Story."

Wednesday, January 2, 1946

I read a copy of a letter dated January 1, which Daniels wrote to Mackey at Hanford requesting that, if possible, the slug containing 1 mg of Np^{237} placed in the Hanford pile on October 30 be discharged after receiving an irradiation of approximately 60 days.

In another memo Daniels asks Chapman to arrange for the irradiation of 5 mg of U^{233} at Hanford for approximately one year. The purpose of the irradiation is to evaluate neutron capture cross section for U^{233} and U^{234}.

A conference on high-temperature piles began today with five representatives from the Monsanto Company in St. Louis (E. H. Buford, C. J. Colley, H. T. Gammon, F. H. Gronemeyer, and J. B. Rutter), and four representatives from Monsanto at Clinton Laboratories (including

Lum and McCullough) present. Willard and Daniels prepared a report, "Summary of High Temperature Oxide Pile Program" (MUC-JEW-63) for use at this meeting. The Monsanto group seemed interested in building a power reactor although the Clinton group was concerned about the extra work load such an undertaking may place on their already heavy program work load commitments. They seemed favorably impressed with the idea of getting Allis-Chalmers to do much of the engineering development and construction. The plant site could be Clinton, Argonne, or other places such as the abandoned Badger Ordnance Works near Madison, Wisconsin.

The discussions will run through January 4 and a visit to Argonne will be made by the group.

This morning's paper carried an item quoting Professor Lawrence Bragg of Cambridge as stating that atomic energy would bring about a world state. Professor Bragg was awarded the Nobel Prize for Physics in 1936.

<u>Thursday, January 3, 1946</u>

I attended the morning meeting of Groups 1 and 3. Those in attendance were Ames, Anderson, Britain, Florin, Ghiorso, Hagemann, Hindman, Hyde, Jaffey, James, Katzin, Magnusson, Manning, Osborne, Peterson, Scott, Sedlet, Stewart, Studier, Templeton, R. Thompson, S. G. Thompson, Van Winkle, and Westrum. I announced that Stewart is now Assistant Section Chief, taking Perlman's place. He will also take over some of Manning's former jobs such as handling the transfers of special materials. We have received a number of letters (Hamilton, Perlman, and Segrè) asking for heavy isotopes; and Stewart will try to fill the requests. Ten grams of plutonium will be milked for 95^{241} starting January 9.

I mentioned that Libby has told me that polytrifluorochloroethylene is now available through the Area Engineer for use in chemical equipment that must resist fluorine and HF. This plastic will stand temperatures up to 300°C.

Roy Thompson presented cross section data obtained from Pa^{231} bombardments. He found an alpha-emitting component with a 1.5-day half-life which he believes to be due to U^{229}. The best value for the half-life of U^{231} is 4.1 ± 0.1 days. Protactinium-231 data and data from the bombardment of U^{238}, U^{235}, Np^{237}, Th^{232}, and U^{233} are given in Figure 22.

Van Winkle reported that about 450 mg of a pure oxalate precipitate of Th^{230} have been recovered but it has not yet been spectroscopically analyzed. It should be possible to milk radium from this material.

Osborne said he has determined the half-life of Pa^{230} to be

Isotope Bombarded	Pa^{231}	U^{238}	U^{235}	Np^{237}	Th^{232}	U^{233}
Reaction	Isotope measured, cross sections in barns					
d,n + d,p	U^{232} 8×10^{-4}				Pa^{233} 0.06	
d,2n	U^{231} 6.4×10^{-4}	Pu^{238} 0.01			U^{232} 0.027	
d,3n	U^{230} 4.2×10^{-3a}			Pu^{236} 0.02		
d,p2n	Pa^{230} 1.3×10^{-3}					
d,4n		Pu^{236} 6×10^{-4}		Pu^{235f} 1×10^{-3}		
d,p				Np^{238} 0.02		
d,n				Pu^{238} 0.004		Np^{234d} 2.5×10^{-3}
d,αn						Pa^{230} 3×10^{-8}
α,fission	Ba^{140} 0.22					
α,p2n		Np^{239} 6×10^{-3}	Np^{236} 3×10^{-3}			
α,3n			Pu^{236} 9×10^{-3}			
α,4n			Pu^{235f} 7×10^{-4}			
α,4n + α,p3n	U^{231} 8.2×10^{-5b}					
α,5n;α,p4n α,αn combined	U^{230} 1.0×10^{-4}					
α,αn	Pa^{230} 4.5×10^{-4}	U^{237} 3×10^{-3}				
α,6n + α,p5n	U^{229} 2.8×10^{-11c}					
α,n	Np^{234d} 2.4×10^{-4}					
α,2n				Np^{235e} 0.014		

[a] This value is not corrected for contribution of d,p2n reaction through decay of Pa^{230}.
[b] This value involves a Pb absorption curve. If Al is used, the value is 4.3×10^{-5} barns.
[c] Assumes U^{229} decays by alpha emission only. To be correct, the value should be divided by the alpha-branching fraction, which is unknown.
[d] If this isotope is Np^{233}, then the value is for the reaction involving loss of one more neutron.
[e] This assumes that the 95^{239} formed decayed rapidly by alpha emission to Np^{235}
[f] The 40-day x-ray emitter was measured - assumed to be Pu^{235}

Figure 22. Cross section data from Pa^{231} bombardments.

17.1 days. Van Winkle announced that U^{231} appears to have an alpha half-life of 10-15 years based on alpha-decay measurements.

Studier mentioned that U^{230} has a half-life of 21 days.

I placed a conference call to English and Stoughton at Clinton. English told me that he talked with Orville Hill about a job at Berkeley. Hill feels, however, that he cannot live on the salary of a teaching assistant and is exploring the possibility of obtaining a national research fellowship. Hill wants to wait on the outcome of this before making up his mind.

Spof told me that he plans to visit us at the Metallurgical Laboratory on Monday, January 14, and will stay until January 17 or 18.

I also called Farrington Daniels to discuss the results of Daniels' telephone conversation with Hood Worthington about the provision of materials and their irradiations in the Hanford reactors. Daniels said that they talked about the problems associated with loading Hanford reactors with thorium to product U^{233}. Worthington told him that fuel slugs will be available later which should have 800 g of plutonium per ton. We must supply shipping containers for these slugs. Daniels said that Worthington also discussed the progress on samples being irradiated for us. These include sample CW-4 and a number of milligrams of Np^{237} and of U^{233}.

I wrote a letter to Lieutenant Mahoney of the Chicago Area Office, the text of which is as follows:

> I am enclosing three copies of each of four early reports describing work done at the University of California in Berkeley previous to the inception of the Manhattan District Project. It is the wish of the authors that two copies of each of these be sent to the Editors of the scientific journals designated below, to be edited and held in readiness for publication by them pending complete release from the classified category. The third copies are for your office in case you should feel that you would like to have them.
>
> We should like either to have you send them, or obtain authorization for us to send them, to the following journal Editors. Two of them, those entitled "The Chemical Properties of Elements 94 and 93" and "Element 94 in Nature" should be sent to Dr. Arthur B. Lamb, Editor of the Journal of American Chemical Society, Harvard University, Cambridge, Massachusetts. The one entitled "Nuclear Properties of U^{233}: A New Fissionable Isotope of Uranium" should be sent to Dr. J. W. Buchta, Acting Editor of The Physical Review, University of Minnesota, Minneapolis, Minnesota, and the one entitled "Nuclear Properties of 93^{237}" to Dr. Joseph E. Mayer, Editor of The Journal of Chemical Physics, Columbia University, New York, New York.

In a more personal letter, I wrote to Saul Winstein at UCLA to tell him I plan to be in South Gate, January 26 and 27. I write that I would like to talk with him. I also comment that I have heard a rumor that Charles Coryell has taken a position at MIT and that I am interested in Bill Young's final choice for nuclear chemists at UCLA.

Helen had dinner with Dorothy Paul at McCue's Restaurant; they then attended an all-Wagner concert.

Today's paper carries a long article entitled, "Report A-Bombing of Japs Had Political, Not War Aim." The article claims the use of the bomb was the decision of the executive branch and not the military. The article also states that "the bomb" was ready in 1944.

Friday, January 4, 1946

Spedding called me from Ames about his preparation of Chapter VIII of Volume 17B of the Plutonium Project Report. Spedding said he will contact Foote and others for their contributions and will try to get the chapter to us by January 31. Spedding commented that this is registration day at Iowa State. Today 600 students have registered for freshman chemistry--the normal number is 120.

I read a copy of Daniels' memo of January 2 to Captain Chapman requesting the irradiation of a 1 gm sample of Ra^{226} in the Hanford pile for several months. The purpose of the irradiation is to permit microchemical studies of pure actinium compounds. Daniels points out that,

> The very limited information on actinium chemistry now available has been derived from tracer studies. A knowledge of actinium chemistry is important since the increasing evidence indicates that actinium is the prototype of several transuranium elements (actinide series).

A letter dated January 2, arrived from Roy Heath who says that arrangements have been made to pick me up at my Detroit hotel and take me to Wyandotte Chemicals Corporation on the morning of January 18 in order to tour part of the plant.

Elliott L. Abers, a graduate student at Purdue University, wrote to confirm January 22 as a satisfactory date for my talk on "Some Aspects of Nuclear Energy." Abers will make hotel reservations for me.

I wrote to Manfred Lindner at Hanford to tell him he will be offered a teaching assistantship at Berkeley and we are looking forward to seeing him at the University of California this spring.

Helen took care of Ruth Ann Thompson for a few hours while Alice went out. The Osbornes dropped by to see her in the afternoon.

The development of a new synthetic drug offering "definite promise" of a cure for the relapsing type of malaria is announced in today's newspaper.

Saturday, January 5, 1946

Morgan wrote me from Berkeley to send me the bombardment log for the targets of Pa^{231} and U^{235} for the period November 20 to December 5. As of December 5, the Pa^{231} sample had received a total bombardment of about 179 microampere-hours while the U^{235} target had received a total of 537.8 microampere-hours.

Jerry Howland, who is presently in Janesville, Wisconsin, wrote to me to say he is waiting for train reservations to California. Howland explains that he even has a problem getting someone to crate and move his personal belongings. He suggests that the project at Berkeley might be able to put some pressure on one of the national moving lines. Howland says that, naturally, he will carry his golf clubs with him on the train.

E. S. Proskauer of Interscience Publishers, Inc., in referring to my letter of December 29, expresses pleasure in my interest in writing a book on nuclear chemistry within the next two years. Proskauer asks if I am interested in serving as a member of an editorial or advisory board for the proposed publication "Advances in Nuclear Chemistry."

I received a letter dated January 3, from James T. Grady of the New York ACS office requesting additional information on my talk to be presented at Detroit on January 17. Grady wants to be able to provide fresh publicity. He also requests a photograph.

R. F. Gould, Midwest Editor (located in Chicago), Chemical and Engineering News, wrote to me to say that it has been noted that my article on elements 95 and 96, which Gould reminds me was first published under the title "The Chemical and Radioactive Properties of the Heavy Elements" in Chemical and Engineering News, has been "reprinted" in the December 1945 Chemistry magazine without credit to C&E News. Gould claims that C&E News was granted exclusive publication rights and he requests that should the article be submitted to still other magazines, I should not overlook this credit detail. A copy of the letter was sent to Watson Davis.

In a letter to Latimer I point out that a decision concerning the offer to Manning of a position with the Berkeley chemistry department staff must be made before the end of January since Manning has other offers. I say that an associate professorship at about $5,000 would be appropriate.

Kohman, in writing to W. Y. Chang, Princeton University, asks him to clarify the data on low-energy alpha groups of polonium appearing in Chang's letter published in The Physical Review [Phys. Rev. 66, 267 (1945)].

In two separate memos to Chapman, Stewart requests the irradiation of a small sample of 95^{241} in the Clinton pile for the period of one month and information on the history and previous processing of the 19.7 mg radium source (number 272) being used by Don Ames.

Stewart then sent Perlman the information on the status of the samples he requested of me on December 29. Stewart mentions that the equipment for shipment to Berkeley is still being packed.

Stewart also answered Hamilton's letter requesting samples of Ac^{227}, Th^{230}, Pa^{231}, 95^{241}, 96^{242}, and Pu^{238}. Stewart mentions that the interceptor targets will be sent as soon as they are completed.

Report No. MUC-FD-L-131, Metallurgical Laboratory Report for December 1945, was issued today. It contains a summary of organizational activities for the Met Lab, a summary of the work of each division, and a report on the budget.

The Health Division reports that the bismuth phosphate-lanthanum fluoride method for the analysis of plutonium in urine has proved quite reliable for detecting the presence of 0.1 micrograms of plutonium in the body. With adequate urine volumes it should be possible to establish the plutonium body burden of all exposed personnel at the Met Lab.

In the Physics and Metallurgy Division section it is noted that the sample used to determine the nuclear constants of U^{233} was found to contain 96.35 percent element 23 and 3.65 percent element 28. The amount of ionium isotope in a thorium preparation has been measured, making possible the determination of the nuclear constants of ionium.

The Services and Development Division reports that the Instrument Group and Shop Group recently presented an exhibition of instruments developed and produced at the Met Lab. Outstanding among the many instruments displayed were the vibrating Reed Electrometer, the Ryerson Electrometer, and the quartz microbalance.

Argonne Laboratory now has 35 persons on its scientific and technical staff. In addition it has 44 administrative and shops personnel and 25 guards, making a total of 104 persons.

The following information was reported for the Chemistry Division. The report for the Chemistry Division includes the material submitted for Section C-I to Hogness by Manning on December 23 and the following for Section C-II:

Progress has been made on the high-temperature-pile program as follows: (1) The heat conductivity of BeO up to 1,340°C has been determined by the Battelle Institute and found to be desirably high. (2) The volatility of U_3O_8 from sintered BeO-U_3O_8 mixtures has been found to be undesirably high at 1,500°C, but preliminary experiments indicate that the volatility of UO_2 may be sufficiently low even at this temperature. (3) Test shipments of BeO bricks for pile construction have been received from two manufacturers. (4) Analysis of gases evolved as a result of bombardment of BeO in the Argonne pile set an upper limit of <1 percent of the BeO irreversibly decomposed per year of operation at Hanford flux.

A single sample of graphite recently received after exposure in a Hanford pile stringer has shown a total stored energy content of 308 cal/g (heat of combustion determined by the Bureau of Standards), far higher than any value previously obtained.

A satisfactory procedure has been developed for the analysis of urine for uranium by extraction with ether and electrodeposition of the uranium.

Mass spectrographic results have verified the mass assignment of 106 for the one-year ruthenium isotope.

The major program areas and distribution of effort within the Chemistry Division for December was as follows:

Section	Program Area	% Effort
C-I (1)	Nuclear and Chemical Properties of the Heavy Elements	18
C-I (2)	Control Analysis of Uranium Ore	7
C-I (3)	Recovery of Product	4
C-I (4)	Redox Solvent Extraction Process	17
C-II (1)	High Temperature Piles	14
C-II (2)	Radiation Chemistry	4
C-II (3)	Fission-Product Studies	5
C-II (4)	Analytical Chemistry	2
Division Writing for Plutonium Project Report		30

The budget for the entire Met Lab and Argonne Lab programs for December 1945 was at a level of $511,400. In the Chemistry Division the estimates show a slight increase due to expansion of the high-temperature pile research program and to a continuation of the Redox Solvent Extraction research which was originally scheduled for discontinuance on October 1, but which proved so successful that continuation was imperative.

Of general interest, the report describes a recent visit to the Met Lab by General Groves who gave an hour-long address to assembled laboratory personnel and stated that in all probability the laboratories at Chicago and Argonne would continue to operate as permanent facilities.

Also issued today is a report (CN-3382) entitled "Crystal Structure Results for Neptunium Compounds" by W. H. Zachariasen. The compounds listed were prepared by Sherman Fried or T. J. La Chapelle and L. B. Magnusson and were all identified through interpretation of the x-ray diffraction patterns.

The sports pages report the news that the All-American Football Conference will begin operating next fall with eight teams.

<u>Sunday, January 6, 1946</u>

Helen and I had dinner with the Foster Yorks in their apartment in Evanston.

<u>Monday, January 7, 1945</u>

I received a letter dated January 4, from Iz Perlman in which he comments on a preliminary outline I prepared and sent to him about a study of the "Feasibility of Inspection of Chemical Processing Plants in Connection with World Plants for Manufacture of Plutonium and Uranium-233." Perlman says,

> Concerning the report to be submitted to Congress on the feasibility of controlling the chemical aspects of nuclear energy, I would add the following comments to place emphasis on what I consider the most important points. I believe that the most direct approach toward knowing what work is going on is through a check on technical personnel. If one were to have a complete list of engineers, physicists, and chemists and free access to the work of any individual, then there could be no question of not having sufficient information. This roster would necessarily include technical students. With this idea as the basis for control, corollaries of the system would include access to research in progress, access to plants that

are in operation and under construction, and even access to military establishments. One other thing would probably be necessary, and that is laboratory facilities for the inspection group so that they may make any measurements they wish on materials of their own choosing.

He then asks my plans for the actual writing of the report.

A letter dated December 28 arrived from L. L. Tully, Educators Association, Inc. (publishers of The Volume Library encyclopedia) requesting data on the four new elements, neptunium, plutonium, element 95, and element 96.

I read a copy of Morgan's reply to Westrum's recommendations for the design of a Micrometallurgy Laboratory at Berkeley. Morgan says the suggestions can be followed fairly closely.

I wrote to E. O. Lawrence about the talk I had with Pontecorvo during my visit to the Canadian atomic energy project last month. I say that Pontecorvo is presently earning around $7,200 per year on the Canadian Project and that he has other offers for associate professorships or possibly even a full professorship that he must consider in deciding whether to join our group at Berkeley. Pontecorvo will make a decision within a month. If he should accept the Berkeley position, he will probably not be able to come until after the start-up of the Canadian pile, which might be six or eight months from now.

I then say, according to present plans, I shall be in Berkeley January 28 to February 2.

In reply to a letter I received last week from A. H. Angerman of Newport, Delaware, I point out that unfortunately only part of Yost's chapters on advanced inorganic chemistry have been published --those dealing with the fifth and sixth groups of the periodic table. The publication is "Systematic Inorganic Chemistry," by D. M. Yost and H. Russell, Jr., published by Prentice-Hall, Inc., New York. (Angerman had mentioned that he used Chapters 13 and 14 while working on the Project.)

I asked Daniels, by memo, to arrange for Al Ghiorso to visit the Chalk River and Montreal Laboratories to see the electronic instruments. The last week of this month would be best for this trip.

In a letter to Elliott Abers at Purdue University, I thank him for his trouble in making arrangements in connection with my January 22 talk to be given at Purdue. I say that I will leave here on the train which departs at 2:12 p.m.

I mailed Miss Nell Parkinson of Chemical and Engineering News my review of "The Story of the Atomic Bomb" by W. L. Laurence. This

40-page booklet, available for 10¢ from the New York Times consists of a compilation of eleven newspaper articles written by Laurence as official releases for the War Department. In my review I state that although the accounts given in Laurence's booklet are short, the booklet is, as far as it goes, a more accurate description of the origin and early development of the three major projects of the Manhattan District than is the Smyth Report. The three major projects referred to are the production of U^{235} from uranium by the electromagnetic isotope separation method, the separation of U^{235} from uranium by the gaseous diffusion process, and the manufacture of the new synthetic element, plutonium, by means of large-scale chain reactions and chemical separations.

In other laboratory business correspondence, Stewart sent a request to Captain Chapman for an amendment to Manning's request of October 18 (MUC-GTS-2020), to cover the helium ion bombardment of isotope 51 on an interceptor-type target for a total of 100 to 200 microampere-hours. Stewart also states that Rubinson of Section C-II has asked us to arrange for the bombardment at Berkeley of one-gram quantities of isotopes 49 and 25 with high energy neutrons induced by helium ion bombardment of a light element. This will permit a study of the fission product distribution in these fissionable materials produced by high energy neutrons. Rubinson will arrange for Site Y to send the 25 and 49 metal samples to Berkeley.

Daniels acknowledged a request from Hogness to go on a half-time basis beginning February 1, and to resign from the Met Lab on April 1 in order for him to devote full time to his university work.

Helen had a dental appointment today.

A Laboratory Council Meeting was held at 9:00 a.m. in Room 209, Eckhart Hall.

According to this morning's paper, the United States is expected to ask for revision or clarification of the atomic bomb agreement announced by the foreign ministers of the U.S., Great Britain, and the U.S.S.R. last December. This comes on the evening of the opening of the United Nations General Assembly, which will be asked to set up a commission for the control of atomic energy.

Tuesday, January 8, 1946

I attended a meeting of Groups 1 and 3 at 8:28 a.m. in the New Chemistry conference room. Present at the meeting were Ames, Anderson, Britain, Fineman, Florin, Ghiorso, Hagemann, Hindman, Hyde, Jaffey, James, Katzin, Kohman, Magnusson, Manning, Osborne, Peterson, Scott, Seaborg, Sedlet, Stewart, Studier, Templeton, R. Thompson, S. G. Thompson, Van Winkle, and Westrum. I announced that Allison, Fermi, and Teller are now in Chicago to work at the Nuclear Institute.

There was a discussion about the publication policy of the PPR. It appears the laboratory cannot force publication in the PPR. Kohman has suggested a chapter in Volume 14 (transuranic elements) on chemical procedures used in handling bombarded materials, and I asked Roy Thompson and Ralph James to try to compile such a chapter.

I said that Hopkins will attempt to isolate compounds of praseodymium, neodymium, and terbium in their higher valence states as a follow-up of some preliminary work by S. G. Thompson. As such compounds have never been prepared before, the work should be very interesting.

Magnusson reported that some 10 micrograms of the earliest isolated neptunium was still available. I suggested that perhaps several hundred micrograms from the earliest Hanford run should be saved as a matter of historical interest. Magnusson remarked that no work has been done on the carrying of Np(V).

Ames and Kohman described work of the former Radium Analysis Group. It was concluded that the best method of counting radium would be to use the pulse analyzer to determine only the percentage of long-range alpha particles present and to use this determined percentage to correct a total count taken on an Argon-CO_2 counter.

Anderson has made a series of alkali metal compounds of the type $MPuF_5$. Zachariasen's x-ray diffraction studies show this formula to be correct. Sodium, potassium, and rubidium compounds are all greenish in color. Cesium, $CsPu_2Fe_9 \cdot 3H_2O$, is a reddish brown compound. Compounds of Pu(III) and Pu(VI) will be prepared in the future.

Westrum discussed his recent preparation of PuOCl which was formed on a 200-mg scale by passing hydrogen and HCl saturated with water vapor over plutonium oxalate.

Florin described his attempts to prepare NpF_6 by passing fluorine gas over NpF_3 at a high temperature. The sublimed pinkish compound obtained was revolatilized to one end of a capillary and produced a few dark brownish crystals which, upon analysis by Zachariasen, seem to be NpF_6. Methods for determining the vapor pressure vs temperature data on NpF_6 were suggested.

I mentioned that another neptunium recovery run at Hanford should be requested very soon and Magnusson, Hindman, S. Thompson, and R. Thompson will consider the problem of recovery and draw up a suitable flowsheet.

Osborne may temporarily stop his protactinium work in order to work on the milligram of neptunium that is being bombarded for the production of Pu^{238}.

Peterson talked about his work on actinium preparations from the recent Ra^{226} plus neutron bombardment. One "purified" sample appears to have about 200 alpha c/m of which 73 c/m are of short range

(3.43 cm) and thought to be actinium alpha particles. Peterson proposes to purify carefully a larger sample of actinium and determine the amount present by following the early growth of Th^{227}.

James reported that the half-life of Np^{236} was determined from alpha particle-growth measurements to be 22 hours and 21 hours from beta particle-growth measurements. He found that the five-month Np^{235} did not grow from short-lived alpha particle decay of 95^{239} (if present) in an old plutonium plus deuteron target. Also, a 40-day plutonium orbital-electron-capturing isotope was determined not to be Pu^{235} because five-month Np^{235} did not grow from it.

Osborne reported the half-life of Pu^{232} to be 1.4 days on the basis of three separate observations and not 1.6 days as previously reported.

Stewart announced that the Health Group wants Room 11 cleaned up and samples, left by O'Connor on his departure, checked. Room B-2 is also to be given additional cleaning for radioactive contamination.

I received a copy of a letter dated December 29, from President Sproul to Sidney D. Kirkpatrick, Editor and Secretary, Award Committee of Chemical and Metallurgical Engineering, thanking Kirkpatrick for the information that the University of California is among the institutions designated by the Manhattan District to receive the 1945 award for Chemical Engineering Achievement.

Sproul said he could not be in New York on Tuesday, February 26, to receive the parchment scroll but that I, as a faculty member at Berkeley, would be happy to participate in the subscription dinner at the Waldorf-Astoria Hotel and to receive the award as the representative of the University. (Sproul wired me about this on December 20.)

In other correspondence today I wrote Harrison Brown that I understand the difficulty he must have had in making the decision not to join our group at Berkeley. I say I hope that his being in Chicago will make it easier to keep in close contact and to do cooperative research.

Another letter went to Melvin Calvin to tell him of my plans to be in Berkeley from January 28 to February 2. I ask him to arrange a hotel reservation at the Durant Hotel. I also suggest a game of golf with him and Iz on February 2 before my train departs.

I replied to the request of J. T. Grady for information on the new material I might present in my forthcoming Detroit and Chicago talks. I explain that all new material must be cleared through our Security Office well in advance of the speaking engagement, a requirement which makes it difficult to give an address in this field. I, however, list three points of interest which I plan to emphasize in the Detroit talk: (1) I will give the first public announcement of the mass numbers for the recently detected elements 95 and 96--these are 95^{241}, and 96^{242}, and 96^{240}; (2) I will show a color picture of the first pure Np^{237} dioxide sample isolated June 21, 1944; and (3) I will show a color picture of the first pure sample of Pu^{239} hydroxide isolated over three years ago.

When I was in Los Angeles in August, I had a discussion with Professor J. B. Ramsey about the electronic structure of the heavy elements. At that time my views were not cleared by Security, so I could not discuss the matter completely. Today I sent him a copy of my Northwestern University talk so that he will be able to see we have good evidence for the beginning of a new rare earth-like series in the heavy elements.

I wrote Kennedy, now in St. Louis, to ask him to give a 45-minute paper at the American Chemical Society symposium on some phase of nuclear chemistry at Atlantic City on April 9, 1946. I say that I plan to give a paper on the actinide series of heavy elements.

I tell Kennedy that I signed the letter about patent cases S-52 and S-61 in answer to Lavender's multiple refusals. I then say I have learned the cases have now actually been filed.

Helen had an appointment today with Dr. E. Davis, her obstetrician.

The front page of this morning's paper carries an item saying that Russia has developed an atomic bomb which makes the Anglo-American bomb obsolete. This claim was made by Dr. Raphael E. G. Armative, Director of the Lomechie Center for Anthropological and Human Biology in Great Britain. He declined to state the source of his information but claimed the work was done with two captured German physicists.

Wednesday, January 9, 1946

I received a letter dated December 14, 1945, from Lavender of the Office of Scientific Research and Development in Washington, D.C., enclosing a Memorandum Opinion about Patent Cases S-781. He defines four points of conflict in our patent claims relating to the process for improving decontamination of plutonium-containing solutions by the use of scavengers supplemental to by-product precipitation. Lavender concludes in his opinion that I, Stan Thompson, and Davidson were the first to conceive the subject matter under counts 1 and 2 and that we were the first to reduce the subject matter to practice. However, in counts 3 and 4, Lavender's opinion is that we do not have a provable conception or reduction to practice; these two counts should be awarded to Dr. Faris and Dr. Olsen who appear to have been the first persons to make the particular inventions.

I wrote to R. F. Gould to say I have no present plans to submit the article he referred to in his letter of January 4, to other magazines. I also enclose a copy of my letter to J. T. Grady about my forthcoming Detroit talk.

I sent L. L. Tully the following information on heavy elements for incorporation in The Volume Library encyclopedia. He requested this on December 28.

Elements	Occurrence and Discovery, Date of Discovery, Discoverer	Properties	Chief Compounds and Uses
Neptunium, Np At. wt. 237 At. no. 93 Valence 3,4 5,6	Produced in nuclear chain reacting units. 1940, McMillan and Abelson.	A synthetic element. Radioactive, emits alpha particles, half-life 2,200,000 years.	The oxide is dark brown in color.
Plutonium, Pu At. wt. 239 At. no. 94	Produced in nuclear chain reacting units. Present to small extent in uranium ores. 1940, Seaborg, McMillan, Wahl, and Kennedy.	A synthetic element. Radioactive, emits alpha particles, half-life 24,300 years.	Plutonium is used in atomic bombs and atomic energy machines.
Element 95 At. wt. 241 At. no. 95	Produced by nuclear transmutation. 1944, Seaborg, James, and Morgan.	A synthetic element. Radioactive, emits alpha particles.	Scientific interest.
Element 96 At. wt. 242 At. no. 96	Produced by nuclear transmutation. 1944, Seaborg, James, and Ghiorso.	A synthetic element. Radioactive, emits alpha particles.	Scientific interest.

I accepted Albaugh's invitation to attend an open house at his home in Englewood, California, on the evening of Saturday, January 26.

I also say that his invitation to a golf match is irresistible, and I suggest we play nine holes in the late afternoon of January 26 before the open house.

I read a copy of a memo that Daniels sent to Colonel Nichols summarizing Met Lab problems which Daniels has discussed with Nichols. The status of the high-temperature pile is described in light of Monsanto's possible interest in building a power pile. Daniels recommends that Allis-Chalmers be given a subcontract to assist with the engineering development of a gas-tight blower to circulate the helium cooling gas.

Regarding the solvent extraction developments, Daniels says, "We are convinced that the solvent extraction is a very great improvement over the Bismuth Phosphate Process and for the sake of saving U^{235} and for the recovery of fission products every effort should be made to install this process at Hanford as soon as possible." Daniels goes on to say that Lawroski is our key solvent extraction man. Since he is on loan from Standard Oil of New Jersey on a half-time basis through March 1946, we may lose him after April 1 unless definite plans are made for a large-scale implementation of the solvent extraction process at Hanford; then Lawroski would probably be interested in staying with the Project and taking an active part.

Daniels comments on the possible discharge from service of SED personnel at the Met Lab. He says we need these men who are doing important work in our program, but presently they give us what amounts to slave labor. Daniels suggests offering these men positions as civilians at normal salaries, or releasing them for full or part-time academic work.

Daniels reports that the round-robin letter approving the general principles of a regional laboratory has not yet reached Loomis and Tate but should return to Chicago in the near future.

Daniels says that Allison and Fermi have now returned and are on the University of Chicago payroll working with the Institute for Nuclear Studies. Anderson, Teller, and others will also be returning soon. These men will be consultants to the Met Lab. The fourth floor of Eckhart Hall has been set aside largely for the use of the members of the Institute. Allison's office will probably be on the first floor outside the guarded area. Daniels then suggests that a considerable portion of the Met Lab shop facilities now on the University of Chicago campus should be moved to Argonne, thus releasing part of the shop area as an unguarded area for use by the University's Department of Physics and the Institute of Nuclear Studies.

Helen had tea at Mrs. Farrington Daniels' home.

Temperatures for the day remained close to freezing with a high of 37°F and a low of 31°F. Over three-quarters of an inch of precipitation fell during the day.

Scientists in Chicago say the reported Russion atomic bomb described in yesterday's paper seems "highly improbable," according to this morning's paper.

Thursday, January 10, 1946

I attended the morning meeting of the Solvent Extraction Group. Others present were Ader, Blaedel, Goeckermann, Hyman, Lawroski, Leader, Manning, Murray, Post, Schaffner, and Sheft. Blaedel discussed a run to determine the effect of flow rate on IBU-alpha losses. Losses increased from a normal loss of about 0.6 percent at normal flow rates to about 3.6 percent at a flow rate 175 percent greater than normal. Decontamination of IBP (column IB-product) is satisfactory but ICU (column IC-uranium) decontamination was poor, probably due to the presence of neptunium. Lawroski said 99.7 percent of the neptunium goes with the uranium and that reduction may be too slow in our system. More sulfate might help.

Hyman said it appears that a factor limiting plutonium recovery in column IB is the rate of transfer of plutonium from the hexone to the aqueous phase. Almost complete transfer was shown possible with 30 seconds of shaking in earlier batch experiments. Since no very short-time transfer experiments have ever been done with care, Hyman devised a set of experiments using hexone phases containing plutonium and a Sr^{90} tracer with four different aqueous phases. In each system the plutonium and strontium would both be completely extracted at equilibrium. It appears that a stable hydrazine-hexone compound is formed which complexes plutonium strongly (and probably uranium also) and thereby maintains a hexone-soluble state even though the aqueous phase contains only Pu(III) which is not hexone-soluble. This reduces the rate of transfer of plutonium into the aqueous phase. Hyman suggested the possibility of finding a substitute for hydrazine or adding some reagent which will tie up the hydrazine and cut down on its complexing power.

When I was in Berkeley in December, Latimer told me William Shand, Jr., of Cal Tech will join the Berkeley staff in the summer to work in molecular structure investigations using x-ray diffraction. Since we are interested in having someone work on the structures of compounds of the heavy elements, I wrote to Shand to suggest he meet with me on my next trip to Los Angeles to discuss plans and equipment. Today I received a reply saying that he would be happy to visit me at my parents' home in South Gate. I immediately wrote a verification letter to him for a conference at 1:30 p.m. on January 26. I also say that I will try to have a conference with Zachariasen before I leave here.

Lombard Squires is scheduled to meet with Daniels today to discuss the possibility of incorporating the solvent extraction procedure into the Hanford process.

Iz Perlman wrote the following letter from Berkeley:

> I received your letter requesting information on magnetic separation of heavy isotopes and talked the matter over with Burton Moyer who is now running the unit. The present facilities are designed for handling considerably larger quantities than gram amounts and are therefore not at all suitable for separating small quantities of precious materials. Gram amounts would be lost within the machine. However, their present plans are to have constructed within the next two months a magnetic separator that would be designed for just such problems as we have in mind. Unless this program is curtailed here, I am told that we could definitely count on the availability of separated heavy isotopes in about two months.
>
> The limitations on the source material are not very stringent. For purposes of reference any compound whose vapor pressure is of the order of that of UCl_4 can be readily handled. With some modification of the source, even AgCl can be run. I do not have off-hand the values for the vapor presssure of $ThCl_4$ and $PuCl_3$ but believe that those compounds could be used. You might have Westrum round up such information as exists so that I can more intelligently discuss the chemical form for the starting material.

I wrote Proskauer of Interscience Publishers, Inc., to tell him that I would need more information on his plans for the "Advance in Nuclear Chemistry" before accepting membership on an editorial or advisory board.

Gordon Swanberg, a second cousin, called me on his way through Chicago to Ishpeming. He is just out of the service.

Stewart sent Hamilton a description of a sample of 51-62 mixture (sample GTS-131) which Hamilton recently received. Stewart apologizes for the small amount of 62 in the sample; he says that an additional sample of 51 is now in the Clinton pile and more isotope 62 should be available in a month or so. Stewart reports that Rubinson from Section C-II is transferring to Site Y and will make arrangements directly from Site Y to provide the necessary metal foils for the 25 plus neutron and the 49 plus neutron experiments in which he is interested. He then asks Hamilton for the bombardment number assigned to our 51 plus helium ion run at Berkeley.

Stewart then describes his research into the availability of some photographic film Hamilton desires.

An item from Washington, D.C., in today's newspaper says that President Roosevelt wrote to Wendell Willkie two days before Pearl Harbor saying that war was coming at any moment.

Friday, January 11, 1946

I received a letter dated January 9 from Sidney D. Kirkpatrick, Editor, Chemical and Metallurgical Engineering, acknowledging President Sproul's letter stating that I will represent the University of California at the Award for Chemical Engineering Achievement at the ceremonies in New York City on February 26. He will send me a banquet ticket and invitation soon.

Orville Hill is one of the men with whom Spof English spoke about positions in Berkeley. Today I received a letter from Hill explaining his present position. He wants to return to school this fall to finish his undergraduate degree and is waiting to hear about his application for the National Research Council Fellowship. Eventually he may want to go to Berkeley for graduate work. I immediately wrote Hill to say that I will talk to people in Berkeley about the possibility of Hill's coming at a later time. I ask him to keep me informed of the changes in his position.

I made a request to Norris Bradbury at Site Y for some of the waste salt solutions, resulting from the processing of Hanford uranium batches containing plutonium as high as 400 grams per ton, be saved for our use as a source of isotope 95^{241}.

In a memo to Daniels, I ask that he request that Hanford make another special run to recover the isotope Np^{237} from the waste solution of the extraction step. I included the following suggestions for a few minor alterations that could possibly increase the yield of the Np^{237}.

> First, in the extraction waste solution we would recommend that the use of $KMnO_4$ be discontinued as the source of the Mn(III) for the oxalate reduction. Either $Mn(NO_3)_2$ should be used or the $KMnO_4$ reduced before addition to the solution. Second, we would recommend that the addition of the Bi(III) begin one-half hour after start of the oxalic-manganous reduction and continue over a period of one-half hour. Third, in the product precipitation step of the decontamination cycles we would recommend (a) that the addition of the Bi(III) be made as soon as possible after start of the ferrous reduction (preferably within five minutes after addition of the ferrous) and (b) that immediately following the addition of the bismuth solution the addition of H_3PO_4 solution be started. The latter recommendations mean that the heating time before addition of the Bi(III) shall not be more than five minutes and that the one-half hour heating at 75°C after addition of the Bi(III) solution shall be dropped. It is realized that it may not be possible to meet these conditions exactly--they are mentioned in the hope the plant people will go as far as practicable in the indicated directions.

Another recommendation which is not so directly connected with the yield of Np^{237} but which we should like to point out to the HEW in case they should want further to diminish the possibility of the formation of an extraneous precipitate in the extraction step is the use of fluosilicic acid in the place of the ammonium fluosilicate in this step.

We have learned from our reading of one of the latest Hanford Technical Letters that a fairly large number of 400 gt batches is now being processed and we would like to point out that one of these batches would be particularly desirable for this special Np^{237} run.

I read a copy of Watson Davis' letter of January 8, to R. F. Gould of the Chicago Chemical and Engineering News office commenting on Gould's letter of January 4 to me. Davis says he considers Gould quite unfair in his letter to me about publication rights and credits. Davis says I had properly informed him that my paper was to be published in Chemical and Engineering News and that he checked with the Washington Office of ACS, on my suggestion, and obtained approval for publication of the paper in Chemistry, with the only provision being that mention be made of the ACS section meeting at which I presented the paper. This was done.

Davis also said,

> It seems to us that important papers such as Dr. Seaborg's on the discovery of Elements 95 and 96 should have as wide publication as possible. It is a classic of science. Any attempt to bottle up such papers and keep them from being published widely would seem to us to be a disservice to the science of chemistry.

This evening Helen and I saw Milton Berle in "Spring in Brazil" with Darrell and Marjorie Osborne at the Great Northern Theater on Jackson Street. It was terrible!

The business page of the paper today carries an item saying that atomic energy will probably never be used to power automobiles. This appears in the report on a speech by Dr. Robert E. Wilson, Chairman of the Board of Standard Oil of Indiana.

Saturday, January 12, 1946

A letter dated January 9 arrived from Lavender's office over Roland A. Anderson's signature to advise me that OSRD patent cases numbers S-52A, S-52B, S-52C, and S-52D were filed in the U.S. Patent Office on December 27, 1945, and will be given "special handling."

The cases have been assigned the following serial numbers, as related to their "S" numbers and inventors:

S-Number	Inventor	Serial No.
S-52A	Kennedy, Seaborg, and Wahl	637,484
S-52B	G. T. Seaborg	637,485
S-52C	Kennedy and Wahl	637,486
S-52D	Arthur C. Wahl	637,487

Victor Myer of the Chicago Junior Chamber of Commerce called me to tell of the arrangements for the presentation to me of the Junior Chamber of Commerce "Outstanding Young Man of the Year of Chicago Award." The ceremony will take place on January 16 at the Terrace Casino of the Morrison Hotel, and I am to make a one-minute response when I receive the award. The ceremony will be on the air at 8:00 p.m. for 15 minutes.

A letter dated January 8 arrived from S. S. Kurtz, Jr., thanking me for my willingness to assist in the program for the 1946 Conference on Petroleum Chemistry at Gibson Island. Kurtz is Chairman for the Conference.

Ray H. Jebens, formerly a du Pont man with Jim Maloney's group and now with Kimberly-Clark at Neenah, Wisconsin, wrote a letter to me which I received today. Jebens says that he is doing low concentration studies of sodium, chlorine, magnesium, and aluminum in paper pulp and asks whether radioisotope tracers of these elements are available for use in industrial research. He also inquires if the chart of the isotopes formerly posted on my office wall is now available to people off the Project.

I wired P. Gerald Kruger suggesting that he consider Stoughton, Friedlander, and Duffield as candidates for the radiochemistry position at the University of Illinois.

I received and read a copy of a six-page memo dated January 8 from Joe Hamilton to E. O. Lawrence titled,"The Role of the 60-inch Cyclotron at Berkeley, California, in the Development and Application of Atomic Energy."

In the last paragraph of the memo Hamilton states that the period extending from February 1945 to the present time (January 1946)

> has been devoted primarily to the bombardment by deuterons and alpha particles of thorium, protactinium, uranium, neptunium, and plutonium. The purpose of this work has been

> primarily to study new types of nuclear reactions, identify new isotopes of these elements, and to produce element 96. It is not possible to present in this report a detailed account of the very exciting results that were obtained by the Chicago group under the direction of Doctor Glenn T. Seaborg, to whom the materials were sent for study. However, it may be pointed out that the number of known nuclear reactions produced by deuteron and alpha particle bombardment has been found to be almost doubled as a result of this 50 percent energy increase of the accelerated particles and over fifteen previously unknown isotopes of the five elements listed above have been discovered as a result of these bombardments. Also, as a result of the work at Berkeley, element 96 has been identified. While it was anticipated that results of considerable interest would be achieved by the use of the more energetic deuteron and alpha particles, the total of new scientific knowledge has been much greater than any of us had expected.

My favorite golfer Byron Nelson is leading in the San Francisco Open with a score of 143 after two days.

Sunday, January 13, 1946

Today has been cold with the temperature reaching a low of 7°F and a high of only 22°F.

The suggestion of Watson Davis that his listeners send their ideas for names of elements 95 and 96 has brought us a large and varied selection of proposals. Today Helen and I took time to survey the suggestions. George G. Abel III of Media, Pennsylvania, suggests proxogravum for element 95 (from Latin proximum gravissimus meaning next heaviest) and gravum for element 96 (from Latin gravissimus meaning heaviest). Carl Rees of Weston, Ontario, suggests the elements be named after the two moons of the planet Mars--Deimos meaning dread and Phobos meaning terror. Mrs. Alwin Stieglitz of White Plans, New York, offers "War and Peace." Mrs. J. B. Ward of Merchantville, New Jersey, asks if we can use signs of the Zodiac which would tie in nicely with the planets. Sidney Morayniss of Toronto says,

> As you know, planets are considered as a cool mass released by the sun or even the stars. Therefore we come to the conclusion that stars and planets are composed of the same atoms, the stars having theirs in a gaseous form. Therefore I suggest you name the elements that have been recently discovered after stars of the first magnitude, as they are large and can be seen by the naked eye.

Karl Hirschfeld of Philadelphia suggests number 96 be named universum.

James McManus of Buffalo believes element 95 should be alium from the Latin word alius meaning another. Element 96 would be novium from the Latin word for new. Robert Slacum, a high school chemistry student from Westmont, New Jersey, suggests 95 should be called rhonium, and 96 should be called sechsium. Mrs. J. Pfeiffer of Ocean City, New Jersey, offers dipperium and cometium. Albert Abelson of Brooklyn suggested names for new planets [sic]. Robert G. Harris, a high school student from South Portland, Maine, proposes that 95 be called bolidium and 96, asteroidium. V. T. Johnson of Springfield, Vermont, suggests 95 be called stellanium and 96 be called astronium. Ernest H. Kalmus of New York wrote a long letter, saying we should go back to the very roots of the discovery and name the new elements according to the way they were found, namely by splitting the atom and fission. Therefore this would mean then calling them splittium and fissium. Kalmus also suggests that perhaps they could be named after the great scientists who shared in their discovery: fermium and bohrium. J. D. Boon of the Department of Physics of Southern Methodist University offers the following: 95 - pentonium, 96 - sextonium, 97 - septonium, 98 - octonium, 99 - novanium, and 100 - centurium. Arthur Nowicki of Buffalo suggests that since we have run out of planets, we should take the names of stars--from the sun we could use sunian. Another offering could be big dipperain or big bearianen. Hugo Bass from New York City suggests the following for elements 95 on: transneptunium, siderium, stellium, astralium, and cosmium. Ralph O. Payne says that since 95 and 96 were discovered by the use of alpha particles, element 95 should be named alphonium. He suggests 96 could be called cosmonium after cosmic rays, "which will give, in the future, rise to other elements." Marjorie Salsbury of Patterson, Iowa, would also like one of the elements to be named something that suggests the sun such as solonium. Eric R. Miller of Winter Park, Florida, is another listener who would like to honor famous scientists and suggests the names of Becquerel and Rutherford as men to be honored. W. B. Barrows of Framington, Connecticut, made several contributions of mythological characters such as Vulcan, Hercules, Zeus, Venus, Apollo, Mars. He notes that we have not named anything after the planet earth - terrium. He advises that a number of the elements have been named for countries and proposes amerium. Element 96 could be named finium or ultimium. Michael Flax of Brooklyn proposes neutronium and alphanium since the elements were produced with the aid of alpha particles and neutrons. Gertrude Strother of Ware Shoals, South Carolina, offers quintium for element 95 and sexium & C for element 96. Samuel Ross of Miami Beach suggests einsteinium and rooseveltium for the newly discovered elements. Joseph Wenger of Philadelphia proposes that we go beyond our own solar system and choose names from elsewhere in the universe such as from fixed stars. Martin Schachne of Brooklyn believes the elements should be named after presidents, such as washingtonium. W. J. Brandon of Linden, New Jersey, is another listener who suggests that the elements be named after dead scientists. Since we have used up planets, B. H.

Bedell of Toronto believes we should finish off with the sun and moon and call the elements solium and lunium. Frank C. Banham, Jr., of Kinberton, Pennsylvania, suggests virgorium and ariesium after two zodiac signs. Seymour Gottlieb of Long Island City proposes that the elements be named after the elements they resemble with the prefix cyclo because they were discovered in the cyclotron. He also wonders if possibly one should be named after President Roosevelt and called roosium since he made the project possible. Warren C. Chapman of Atlantic City suggests naming element 95 seaburnium, nutronium, or nonagintium. Irwin Alemanoff of Brooklyn says,

> Since so much work on elements 93-96 has been done in the University of Californa, I think that it might be fitting to name element 95 unicalium in honor of the University. As for 96 I think we should honor the family that had so much to do with the pioneer work in radioactivity and nuclear transformations by calling it curium for the Curies.

M. Miller of the Bronx suggests for element 95 unonium in honor of UNO and paximum for element 96 from the word for peace. Wanda Winfrey, a science student from Sophia, West Virginia, says, "All I have to say about naming the elements 95 and 96 is to name them a civil name that can be pronounced without a week of study." Samuel B. Katz of Brooklyn proposes the name rooseveltium, symbol Fdr, for the man responsible for the great impetus to research in nuclear physics. Donald Steward of Kenmore, New York, suggests xtinium and ytumium for elements 95 and 96. Elihu Schimmel of Lawrence, New York, says if we cannot find better names, perhaps we can use mondium for worldliness and eternium for eternity. Otto I. Bergh of Miami suggests nebulium and solium. Sidney Jacobs of Brooklyn writes,

> I think that saturnium would be a good name for 95 and jupiterium for 96. The reason for this is that a man standing in Jupiter would be 2.5 times as heavy as a man on earth but a man standing on Saturn would only be 2.25 times as heavy as a man standing on Earth and also the fact that Jupiter and Saturn are the two largest planets in our Universe."

Julian Rosenberg of Brooklyn believes these elements should be named after constellation of stars. Anthony Saletan of New York City (Manhattan) suggests curium for element 95 and einsteinium for element 96, while Robert Elenko of Brooklyn likes the name futorium (Fu) for element 96. Walter Schwandt of Great Kills, New York, believes one of the elements should be called artifium or artifician, while Myron Stein of Brooklyn suggests draconium for element 95 after the constellation Draco and leonite for element 96 after the constellation Leo. Aram Amirian of Jersey City believes element 95 should be called mechanicium and element 96 should be sciencium. Ten-year old Donald Ingenito of the Bronx would like the new elements named after the asteroids. Sunonium and moononium are the suggestions of Miss Lillian Cousey of St. Petersburg, and John Henry Geldsmith of Upper Derby, Pennsylvania, believes the elements should be named after the planets, if there are any names left. Then use the names of moons, then stars, then constellations. Finally, Dr. J. P. Von of Winter Park, Florida, writes,

> In naming the new element which you have discovered it seems only right to me that the name of the discoverer should be perpetuated and the branch of the science in which such a discovery was made identified for posterity; thus my suggestion is: DAV-I-SO-NI-UM. Or watsonium, or referring to the science some suitable name.

Monday, January 14, 1946

I attended the 9:00 a.m. meeting of the Laboratory Council in Room 209, Eckhart Hall. Others attending were Branch, Cole, Daniels, Dempster, Foote, Furney, Hilberry, Hogness, Hughes, Jacobson, Jesse, Langsdorf, Moulton, Mulliken, Nickson, Ohlinger, Stone, Urey, Willard, G. Young, H. Young, Zachariasen, Zinn, and Zirkle. Daniels talked about the recent Army procedure no. 363 relating to the release or discharge of SED men.

Mulliken discussed the formation of a Laboratory Release Committee to handle the review and declassification of classified documents. Urey then reported on the work of the Declassification Committee appointed by the Army. This latter committee favors the formation of a National Declassification Office, which the Army may locate at Oak Ridge.

The Council recommended that the Plutonium Project Record (PPR) should be a separate publication, separate from the overall Manhattan Project Report being prepared.

English from Clinton Laboratories and Albaugh, now from California, are visiting the Met Lab today.

Manfred Lindner wrote from Hanford, in a letter dated January 8, to confirm that he has accepted the teaching assistantship at the University of California at Berkeley. He thanks me for my assistance in obtaining the position for him, and he says he hopes to visit Chicago before going to Berkeley.

I received a letter dated January 11 from Wigner inviting me to give an address in the first of a series of conferences celebrating the 200th anniversary of the founding of Princeton University. Wigner suggests a technical-scientific talk such as "Artificial Radioactive Tracer Reactions in Chemistry and Their Influence on Medicine." The talk would be presented on September 24, 1946, as part of the Bicentennial celebration.

Kohman received a reply dated January 10 from W. Y. Chang in answer to his questions of January 5. Chang clarified for Kohman alpha-particle energy data he presented in his Physical Review article.

I called George Everson and learned that Lawrence will return tomorrow and make a decision about the addition of Templeton and Howland to the nuclear chemistry staff at the Radiation Laboratory. I told Everson that Templeton can leave Chicago for Berkeley on February 1, and how to contact Howland in Janesville, Wisconsin.

I sent R. R. Baker of Sonora Radio and Television Corporation in Chicago a draft of the one-minute acceptance speech I plan to make at the Chicago Junior Chamber of Commerce award ceremony.

Stewart sent information to Segrè about samples he requested in a letter to me on November 30. Stewart points out that our stocks of many isotopes requested are non-existent or very limited. The sources will be sent as soon as they become available; 95^{241} should be available within a week; U^{230}, U^{232}, Pu^{236}, and Pu^{238} should soon be available. All other requested isotopes, however, present a considerable problem and may not be in stock for some time.

Helen went to the dentist.

Byron Nelson won the San Francisco tournament and $3,000 with a score of 283. In the final round he shot a 3-under par 68.

Tuesday, January 15, 1946

Groups 1 and 3 met at 8:28 a.m. in New Chem conference room. Ray Stoughton from Clinton Laboratories and Eugene Huffman from Berkeley, who are visiting the Met Lab, attended the meeting in addition to the following individuals from Section C-I: Ames, Anderson, Britain, Florin, Ghiorso, Hagemann, Hindman, Hyde, Jaffey, James, Katzin, Magnusson, Manning, Osborne, Peterson, Scott, Sedlet, Simpson, Stewart, Studier, Templeton, R. Thompson, S. Thompson, Van Winkle, and Westrum. Stewart reported on the shipment of materials. He said that Osborne should receive the neptunium from the Hanford pile irradiation around January 20. The Pa^{231} sample from Clinton should also arrive about the same time; and R. Thompson, Van Winkle, and Osborne will handle the sample. The purpose of the work is to obtain U^{232} for fission measurements. The U^{233} and U^{238} targets from Berkeley should be here this week. The element 95 sample in the Clinton pile will come out around the first of February. The milking of 95 from plutonium is almost complete, and it will be turned over to Cunningham. As much as 0.5 mg of 95 is expected from residues to be received from Site Y. The plutonium metal may arrive before the residues and, since it is old, it should be a good source of 95^{241}.

Simpson reported on a new type of crucible consisting entirely of tantalum with no zirconium or other binders. This may be used to fractionate ions or for vapor pressure measurements.

Roy Thompson talked about the protactinium work. A total of about 50 mg is presently available within Section C-I. Repurification will start this week. Solubility measurements have shown the solubility to be 1.8 g/liter in concentrated HNO_3 and 1.4 g/liter in concentrated HCl. A dilution of 20 percent brought the solubilities down to 1.5 g/liter in the case of HNO_3 and 0.2 g/liter for HCl. In another measurement the solubility was 0.6 g/liter in 7 N HCl.

Van Winkle reported the sample of 100 micrograms of Pa^{231} that was measured at Argonne showed a fission rate equivalent to 300 alpha c/m of Pu^{239}.

Hagemann announced that the isolation of U^{233} from thorium slugs is almost complete. Although the purity of the product is still not known, he estimates that 8.5 mg of U^{233} should be obtained per slug.

Hyde said he has started experiments to determine the neutron absorption cross section of ionium. A mixture of Io^{230} and Th^{232} was irradiated in the Clinton pile. After a period of time to permit the Pa^{233} to decay to non-detectable levels, the ratio of Pa^{231} to U^{233} will be measured. Uranium-232 is also present from the second order reaction on Pa^{231} so that a value for the capture cross section for Pa^{231} will also be obtainable. Jaffey pointed out that the half-life for U^{232} is still uncertain and will lead to uncertainties in the cross section values.

Florin reported on the x-ray diffraction results obtained by Zachariasen on a NpF_6 sample, which show identical lattice constants with UF_6. The melting point of NpF_6 has been determined to be 53°C.

The organizational chart of the Met Lab Chemistry Division as of today was issued:

<u>Chemistry Division</u>

T. R. Hogness - Division Director
Charlotte Young - Secretary to Hogness
John E. Willard - Assistant to Division Director
Betty L. Carlson - Secretary to Willard
Robert E. Zarse - Administrative Liaison Officer
Helen Becker - Secretary to Willard

<u>Section C-I</u>

Glenn T. Seaborg - Section Chief
Ruth P. Rogers - Secretary to Seaborg
Kathleen Florin - Clerk
Winston M. Manning - Associate Section Chief
Donald C. Stewart - Assistant Section Chief
Jane Horwich - Secretary
Lorraine Eisen - Secretary

Group 1 - Heavy Isotopes
Seaborg, Glenn T. - Group Leader
Ames, Donald P. [SED]
Anderson, Herbert H. [SED]
Cunningham, B. B.
Fineman, Phillip [SED]
Florin, Alan E.
Ghiorso, Albert
Hindman, Clark J.
Hopkins, Horace H. [SED]
Hyde, Earl
Jaffey, A. H.
James, Ralph
Katzin, Leonard I.
Kohman, T. P.
Magnusson, Lawrence
Osborne, Darrell W.
Peterson, Sigfred
Scott, Benjamin F.
Sedlet, Jacob
Simpson, Oliver C.
Studier, Martin
Thompson, Roy C.
Thompson, Stanley G.
Van Winkle, Quentin
Walsh, Patricia
Weissbourd, Bernard [SED]
Westrum, Edgar F.
Calhoun, Opaline - technician
Erway, Norman - technician
Thomson, Helen - technician

Group 3 - Recovery
Asprey, Larned B. - Group Leader
Britain, J. W.
Sohn, Madeline

Group 4 - Solvent Extraction
Lawroski, Stephen - Group Leader
Ader, Milton [SED]
Billheimer, J. S. [SED]
Blaedel, Walter J.
Callison, H. G. [SED]
Cavataio, Vincent S. [SED]
DeRose, F. S. [SED]
Friedman, A. S. [SED]
Goeckermann, Robert
Hagemann, French T.
Hausman, Eugene A. [SED]
Hyman, Herbert H.

Kelley, Alec [SED]
Leader, Gordon
Murray, S. A. [SED]
Post, Roy [SED]
Schaffner, Irwin J.
Schraidt, John H.
Sheft, Irving
Boykin, Pearline - technician
Giacchetti, Olga

Section C-II

T. R. Hogness - Acting Section Chief
John E. Willard - Acting Associate Section Chief
Jane Kohman - Section Secretary

Group 1 - High Temperature Piles
Willard, John E. - Group Leader
Crawford, John A.
Fromm, L. W. [SED]
Gaarder, S.
Gilbreath, James R.
Haskell, B. [SED]
Hoffman, John D. [SED]
Hunt, J. F. [SED]
Hutchison, Clyde [SED]
Kittridge, Harvey [SED]
Lake, William R. [SED]
Lision, Walter [SED]
Malm, John G.
Robertson, A.
Slusser, R. F. [SED]
Walling, M. T.

Group 2 - Radiation Chemistry
Allen, A. O. - Group Leader
Gordon, Sheffield
Hochanadel, Clarence
Neubert, T. J.
Van Dyken, A. R.
Beaman, L. - technician

Group 3 - Radiochemistry
Rubinson, William - Group Leader
Adams, Richard
Freedman, Melvin
Seiler, John
Steinberg, Ellis
May, G. J. - technician

Group 4 - Analytical
Templeton, D. H. - Group Leader
Bane, Ralph
Jensen, K. J.
Lewis, Beverley [SED]
Seifert, Ralph E.
Tomkins, F. S.
Billington, Hubert - technician
Casler, Ruth - technician

Note: Adrah Winston is to be dishwasher for the entire Chemistry Division.

Joe Hamilton called to inform me that the cyclotron suffered a breakdown during the bombardment of the U^{233} target. He offered to send the sample to Chicago immediately or wait until the cyclotron is again operating and bombard the target some more. I chose the latter option. Joe mentioned that Scott is working on a method to separate element 95 and lanthanum using a TTA process.

Next week Joe is going to try accelerating carbon ions, then will try nitrogen, oxygen, fluorine, and neon ions. He has consulted with Bowen at Cal Tech about the necessary changes in the ion sources.

I also received a phone call from Cecil D. Langford about the arrangements for the talk I am to give to the Peoria Section of the ACS on February 21. We discussed the travel reservations.

Correspondence today was quite heavy. M. H. Arveson, chairman of the Chicago Section of the ACS, wrote congratulating me on my selection as the 1945 recipient of the Distinguished Service Award of the Chicago Junior Association of Commerce.

Jerry Howland wrote that he has not heard from Berkeley but that his property has been picked up for crating and shipping.

Edgar A. Waite of Standard Oil requested information about Stan Thompson's accomplishments. He would like to use the material in one or more of the company's publications.

Perlman wrote to inform me that Latimer has written the Army about the size of the hot lab needed by our groups. Iz also requested the exact outer dimensions of the cans which held the CW samples.

I invited Charles Coryell to be one of the four speakers at the Atlantic City ACS meeting in April. I mention that I have heard that he accepted an offer from MIT.

In another letter I reply to R. H. Jebens' letter of January 9, saying that I know of no commercial source of tracers yet. I suggest he try Robley Evans or John Irvine of MIT for a cyclotron source. I also say that the chart of isotopes is not available to people off the

Project, but since he could have taken a copy with him when he left the Project I am sending him one along with a reprint of the "Table of Isotopes."

Last Friday I received a letter from C. S. Schoepfle, Chairman of the Chemistry Department of the University of Michigan, asking for information on the qualifications of E. F. Westrum, Jr. Today I replied saying that Westrum is one of the best experimentalists I have ever known. He is cooperative and pleasant with a solid background in physical and theoretical chemistry. I say that I have offered Westrum a position at the University of California, and I will be disappointed if he does not come with us.

Other notes went to Wigner--I accepted his invitation to speak at Princeton; to C. K. Hunt asking that he arrange for my uncle, Lawrence Seaborg of Menominee, to attend the dinner on the night of my talk in Detroit; and to Manfred Lindner saying that I am looking forward to seeing him in Chicago.

Stewart placed a request with L. C. Furney for 100 mg U^{235} (95 percent or better U^{235}) to be used as a cyclotron target for the preparation of Np^{236}.

Wednesday, January 16, 1946

I replied to the letter from Ted La Chapelle in Dayton that I received yesterday. He asked whether I felt it would be all right to start later than March since he does not want to quit his well-paying job just yet. The money he is making will be very helpful when he is in graduate school. I say that I believe it will be all right, but he should keep Dr. Latimer informed. I also mention that I believe he will work with the small nucleus of our group now assembling in California although Latimer may have something else in mind. Probably his work on neptunium can be applied toward his doctorate. I also say that I will sign as a sponsor for a National Research Council Fellowship.

I then filled out and mailed to the U.C. Department of Chemistry a "Report of Candidate for Fellowship" for La Chapelle.

In a letter to Howland I explain that the reason for the delay in his offer from Berkeley is Professor Lawrence's absence because of illness. I say that Lawrence is supposed to return to Berkeley this week.

I read a copy of a letter James wrote to Morgan about the desirability of having a G.M. counter with a variable and reversible magnetic field. James enclosed some rough sketches of his ideas.

Bob Elson, who interviews people departing for New York on the

"Century" train, called and arranged for me to go to the train station for an interview this afternoon.

In the evening Helen and I attended a reception and dinner of the Junior Chamber of Commerce in the Terrace Casino of the Morrison Hotel for the Ten Outstanding Young Men of 1945. Top honors for the group of Ten went to Henry Ford, II, who recently assumed the presidency of the Ford Motor Company and whom I enjoyed meeting. Others members of the group of Ten included Charles Luckman, Executive Vice President, Lever Brothers Company; Frank McCarthy, Assistant Secretary of State; James Linen, publisher, Time magazine; J. Wes Gallagher, foreign correspondent; Dr. Van R. Potter, biochemist (University of Wisconsin); Abe Portas, Under-Secretary of the Interior; George C. Dade, President, Dade Brothers; Gene Root, Douglas Aircraft Company; Robert S. Ingersoll, Ingersoll Steel Division, Borg-Warner Corporation. Henry Kearns, President of the U.S. Junior Chamber of Commerce, participated and the national radio broadcast, over the network of the American Broadcasting Company, had Harry Wismer, the sportscaster, as announcer. Fifteen minutes of the program was broadcast, from 8:00 to 8:15 p.m., and I was the first to speak, followed by Kearns, who presented the "Ten Young Men of the Year 1945," and Henry Ford, II, the "Outstanding Young Man for 1945." I was cited as the Outstanding Young Man of the Chicago Area--The Annual Special Distinction Award for 1945 by Alfred Spengeman, Chairman of the Chicago awards committee, and Chicago mayor, Edward J. Kelly. My acceptance remarks were the following:

> Thank you, Mr. Spengeman, the Chicago Junior Association of Commerce, and Mayor Kelly for conferring this honor on me. The fundamental investigations on nuclear energy for which this citation is made are, of course, not the work of one individual but rather of a group of men. Although time does not permit the mentioning of many names, I will mention the early work of Arthur C. Wahl, Joseph W. Kennedy, Edwin M. McMillan, and Emilio Segrè on plutonium, of Burris B. Cunningham and Louis B. Werner in its first isolation, of Stanley G. Thompson in conceiving the chemical extraction processes which were used in its large-scale manufacture, and of Isadore Perlman, who contributed to many phases of the work. We feel grateful that the Chicago Junior Association of Commerce has signalled out science for this special mention. The special attention given now to nuclear energy comes about largely as a result of the attendant far-reaching social and political problems which should be the concern of all people. I want to thank you again for myself and the many others whom I have not time to mention tonight.

According to today's paper President Truman has declared that the U.S. will keep any Japanese mandated islands necessary for defense under the sole trusteeship of the U.S. as long as they are needed.

Thursday, January 17, 1946

I left Chicago at 9:30 a.m. on the train "Mercury" (seat 20, car 7) to Detroit where I will stay at the Detroit Leland Hotel. Uncle Lawrence Seaborg met me in Detroit. He will attend the dinner at 6:30 p.m.

At 8:00 p.m. I gave an address "The Chemical Processes in Plutonium Production" to a joint meeting of the Detroit Section of the American Chemical Society and the Metropolitan Detroit Science Club in the Rackham Building auditorium. I began my talk, which was illustrated by lantern slides, by saying that I was limited in the material I could discuss. I then went on to describe the concept and history of atomic energy, the production of fissionable Pu^{239}, and the fundamental research necessary before the operation of the plutonium-producing plants at Clinton and Hanford. I described in some detail the work on ultramicro-chemistry done during the early days of the Met Lab necessary in order to devise a separation process. In conclusion I talked about the actinide concept. The auditorium was packed and, I received a very enthusiastic response. It was an exciting evening.

In Chicago Elliot Nachtman is scheduled to start work in the Solvent Extraction Group. Nachtman has been working for Sherwin Williams in Chicago.

Helen had a dental appointment today.

This morning's newspaper carries an article about Russia's move to fortify the Black Sea coast, moving toward Turkey and making that country nervous.

Friday, January 18, 1946

Fred Conkle, Manager of Distribution Sales of the Wyandotte Chemicals Corporation picked me up at my hotel this morning to drive me to the Wyandotte plant. There, Roy Heath took me on a tour of the plant. I had lunch with him and some of his colleagues at Joey's Stable. Heath then drove me back to my hotel (Detroit Leland) where I visited with my Aunt Esther Williams (my father's sister), cousin Jean Walsh (Esther's daughter), and her husband Bill, and my Uncle Lawrence during the afternoon.

At 4:35 p.m. Uncle Lawrence and I left for Chicago (seat 7, car 317) arriving at 9:00 p.m. Lawrence and I went to Ricardo Studio Restaurant where I spoke to a group on the sociological and political implications of nuclear weapons.

According to today's paper the question of American possession of Japanese islands has become an explosive issue. In Congress the nationalists want outright annexation of the islands whereas the internationalists want trusteeships or a similar arrangement. The latter group includes the President.

Saturday, January 19, 1946

Yesterday while I was in Michigan, a telegram arrived from Watson Davis. He asked that I participate in the fifth annual Science Talent Search to be held in Washington, D.C., on March 2 or March 4. Davis suggests a half-hour talk about the transuranium elements. Davis commented that he would have called me, but Washington is experiencing a local telephone strike.

The Predoctoral Fellowship form arrived from La Chapelle. He mentioned that he also is sending such forms to Cunningham and Daniels to get their endorsements. The deadline is February 1.

Daniels sent a memo to Chapman endorsing the special Np^{237} run I requested.

Manning answered the letter I received on January 15 from Perlman --he sent Perlman the requested drawings of the CW containers.

I replied to the letter I received from E. A. Waite of Standard Oil, saying that Thompson's contribution was great and that he was largely responsible for the separation process used in the huge separation plant at Hanford. I also say that Thompson's contribution to the separation process is greater than any other single individual; however, I say that because of security restrictions I cannot describe his work.

A letter arrived from President Sproul giving me the following information about the 1945 Award Dinner for Chemical Engineering Achievement on February 28:

> A reception for the honored guests will be held in the foyer adjoining the Grand Ball Room of the Waldorf Astoria at 7:00 p.m. Tuesday evening, February 26, 1946. You are cordially invited. The dinner will follow in the Grand Ball Room at 7:30 p.m. Dress for honored guests, who will be seated on the dais will be black ties and dinner jackets. In the case of all other dinner guests, dress will be optional--and ladies will be most welcome at the dinner and award proceedings.

Stewart wrote to Whitaker at Oak Ridge that we are sending him one milligram of radium as chloride for a two to three week irradiation.

The purpose of the irradiation is to re-determine the neutron capture cross section of Ra^{226} and to prepare more Ac^{227}. The sample number is GTS 132.

In another memo Stewart asks Furney for an additional stock solution of 50 mg of radium to be used by the entire Chemistry Division.

Report CS-3387, Chemistry Division Summary Report for December 1945, was issued today. It contains information on the work of Section C-I and C-II accomplished during December 1945. The information for Section C-I is essentially identical to that contained in Manning's memo of December 23, 1945 to Hogness.

The report on Section C-II covers work on the High Temperature Pile, which is progressing well. A decision has been made to abandon steam in favor of helium as the coolant gas in the proposed high temperature pile.

The section of Radiation Effects notes that on the basis of one heat-of-combustion determination made by the Bureau of Standards on Hanford-irradiated graphite, a total stored energy content of 308 cal/g was observed--far higher than any value previously obtained.

W. M. Manning prepared and sent to Hogness today the "Summary of Work of Section C-I for Period December 15, 1945 to January 15, 1946." The following subjects are covered:

1. Further Studies of Pa^{230}-U^{230} Decay Chain

   More accurate half-life values are now available for several of the isotopes in the Pa^{230}-U^{230} decay chain:

	Activity	Half-Life
Pa^{230}	beta, gamma	17 ± 0.5 days
U^{230}	alpha	21.0 ± 0.2 days
Th^{226}	alpha	30.9 ± 0.2 minutes
Ra^{222}	alpha	35 ± 5 seconds

   The Pa^{230} half-life was measured in two ways (1) by isolating a mixture of Pa^{230} and Pa^{233} formed from a deuteron bombardment of thorium and following the growth and decay of alpha activity (contribution of U^{233} to the alpha activity is negligible), and (2) by following directly the decay of Pa^{230} gamma-activity (through 5.1 grams of lead and through 20.3 grams of lead) in a sample of protactinium isolated after deuteron bombardment of Pa^{231}. (The Pa^{230} was formed as a result of a d,dn reaction; Pa^{232}, formed by a d,p reaction, decayed rapidly and did not interfere; no Pa^{233} was formed.) The U^{230} half-life was determined by following directly for several half-lives the alpha decay of a sample of U^{230} in equilibrium with its daughters. The Th^{226} half-life was measured in two ways--by direct decay measurement on a sample precipitated with zirconium iodate from a solution containing U^{230} and by decay of recoil activity in a chamber which had been exposed to large amounts of activity of the U^{230} series. In both cases, the decay was constant through about ten half-lives. The Ra^{222} half-life was determined by following the decay of recoil activity in a chamber which has contained Th^{226}.

2. Half-Life of Pa^{232}

Decay measurements on two samples of Pa^{232}--one prepared by $Th^{232}(d,2n)Pa^{232}$, the other by $Pa^{231}(d,p)Pa^{232}$--showed that the half-life for beta decay is shorter than the previously accepted value of 1.6 days. The revised half-life is 33 ± 1 hours (1.4 days).

3. Helium Ion Bombardment of 95^{241}

A small sample of 95^{241}, 18 micrograms, was bombarded for 160 microampere-hours with 44 Mev helium ions in the Berkeley 60-inch cyclotron. The principal purpose of this bombardment was to search for isotopes of element 97. No evidence was found for any alpha activity attributable to element 97, but because of the very small sample, the sensitivity for detection was low. From the results it can be concluded that 97^{242}, which would have been formed as a result of $95^{241}(\alpha,3n)97^{242}$ cannot have a half-life between eight hours and one month if it decays primarily by emission of long-range alphas. Such an isotope might be expected also to decay by orbital electron capture; no x-rays attributable to the decay of element 97 were detected, but the sensitivity of detection was low because of the small sample and the presence of interfering activities.

4. Preparation of Neptunium Hexafluoride

Neptunium hexafluoride has been prepared by the action of fluorine at 1 atm pressure on NpF_3. The NpF_3 (about 0.5 mg) was mounted on a hot filament and the volatile NpF_6 collected in a cold part of the apparatus. The effective temperature of the reaction is unknown except that it was at least several hundred degrees. The NpF_6 was nearly colorless, with a slight brownish tint. It melted at 53°C, and was comparable to UF_6 in volatility and stability. X-ray analysis by Dr. Zachariasen showed the structure and lattice constants of NpF_6 to be nearly identical with those of UF_6.

5. Solubility of Neptunium Compounds

The following table gives the results of solubility measurements on a number of compounds of neptunium. In the few cases where the data have already been reported, references are given to the earlier reports.

Salt	Solubility g/l	Conditions
$NpCl_3 \cdot xH_2O$ purple solution	>2 (CN 3055)	1 M HCl
$Np(OH)_4 \cdot xH_2O$ tan to gray-green	0.0029	½ hour 0.5 M $(NH_4)_2SO_4$ saturated NH_3
	0.00196	0.8 M Na_2SO_4 0.5-1 M NaOH
$Np(SO_4)_2 \cdot xH_2O$	>2	1 M H_2SO_4
bright green, large crystals, green solution; also possible double sulfates with H^+, K^+, NH_4^+	>5	1 M H_2SO_4 (containing K^+)
NH_4NpF_5 (?) bright green	0.0011	2 hr ~0.01 M NH_4^+ 1 M HF
$KNpF_5$ (?)	0.0017	16 hour 0.5 M H_2SO_4 2 M HF 0.05 M K^+
bright green	0.005	1.0 M H_2SO_4 1.0 M KF 1.0 M HF
$NpCl_4 \cdot xH_2O$ solution yellow-	>5 >10	1 M HCl 2.5 M HCl
green in 1 M HCl, yellow in 5 M HCl	>50	5 M HCl
$Np(ClO_4)_4 \cdot xH_2O$ green solution	>1	1 M $HClO_4$

continued...

Salt	Solubility g/l	Conditions
$Np(C_2O_4)_2 \cdot xH_2O$	0.224	1½ hr - 1 M HCl 0.14 M $(NH_4)_2C_2O_4$
green	0.886	1 hr - 0.87 M HCl 0.09 M $H_2C_2O_4$
$Np_3(PO_4)_4 \cdot xH_2O$(?) (preparation at 75°) white crystalline to gelatinous	0.091	24 hr - 0.003 M Fe(III) 0.1 M H_3PO_4 0.2 M $(NH_4)_2SiF_6$ 0.86 M HNO_3
Np(IV) ? peroxide white, flocculent	0.10 (CN 2431)	1 M HNO_3 0.3 M H_2O_2
$NpO_2 \cdot OH \cdot xH_2O$ (?) green to dark blue-grey crystalline	0.18 0.017 0.14	slight excess NH_3 1 M NaOH 2.2 M NaOH
$(NpO_2)_2SO_4 \cdot xH_2O$ blue-green solution	>2	1 M H_2SO_4
$NpOCl \cdot xH_2O$ blue-green solution	>70	1 M HCl
$NpOClO_4 \cdot xH_2O$ blue-green solution	>2	1 M $HClO_4$
$NpO_2NO_3 \cdot xH_2O$ blue-green solution	>2.5 >50	0.5 M HNO_3 5 M HNO_3
$NpO_2(OH)_2 \cdot xH_2O$ dark brown ppt	0.023	0.1-0.5 M NH_4OH
$NpO_2SO_4 \cdot xH_2O$ green solution, more intense than Np(IV) solutions	>5	1 M H_2SO_4
$NpO_2Cl_2 \cdot xH_2O$ green solution	>2 >5	1 M HCl 2.5 M HCl

continued...

Salt	Solubility g/l	Conditions
$NpO_2(ClO_4)_2 \cdot xH_2O$ pink solution	>2	1 M $HClO_4$
$NaNpO_2Ac_3$ pink by transmitted light green by reflected light	0.1	2 M $NaNO_3$
$NpO_2(NO_3)_2 \cdot xH_2O$ green solution	>38	2.0 M HNO_3

6. Heat of Solution of PuOCl

Pure PuOCl was prepared by passing hydrogen and HCl saturated with water over plutonium oxalate. The PuOCl analyzed 12.21 percent and 12.19 percent chlorine (theoretical 12.21 percent) and 82.24 percent, 82.28 percent, and 82.26 percent plutonium (theoretical 82.29 percent). The heat of solution of this material in 6.0 M HCl was 24.17 ± 0.01 kcal. The heat of formation for PuOCl calculated from this value is 224 kcal.

7. Double Fluorides of Plutonium and Alkali Metals

Double fluorides of plutonium with sodium, potassium, rubidium, and cesium have been prepared. The sodium, potassium, and rubidium members of this series are all greenish in color with a formula of the type $MPuF_5$ (based on x-ray analysis by Zachariasen and also chemical analysis). The potassium compound shows the minimum solubility. Cesium formed a compound with the formula $CsPu_2F_9 3H_2O$. This compound was reddish-brown and lost its water at 100° in vacuum.

8. Separation of Element 95 from Plutonium by Volatilization

A small sample of plutonium metal (216 micrograms), estimated to contain 95^{241} to the extent of 0.35 percent of the total alpha activity, was distilled in vacuum to study the efficiency of separation of 95^{241}. The sample was placed in a tantalum fractionation crucible with an effective length of 6 inches and heated by induction in a high vacuum. The material which distilled out was collected on plates mounted above the orifice at the top of the crucible. The material collected on the plates was found to contain a much higher proportion of element 95 than the starting material. The results are shown in the following table.

Plate	Temp. of Top of Crucible (C°)	Temp. of Bottom of Crucible (°C)	Time of Exposure (sec.)	alpha c/m		Percent 95^{241} on plate	Percent of Total 95^{241}
				Pu^{239}	95^{241}		
1	1250	1430	3.6×10^{3}	0	180	100	-
2	1300	~1500	2.28×10^{4}	5039	4415	46.7	8.25
3	1300	~1500	2.13×10^{4}	26258	6711	20.5	12.64

The plutonium vapor pressure calculated from the experiments agrees with data obtained earlier for plutonium oxide rather than for plutonium metal. With the very small sample of plutonium used in this experiment, the material was presumably oxidized in the apparatus. The increased plutonium volatility indicated by material collected on the third plate suggests that the plutonium was reduced to a lower, more volatile oxide during the long heating.

9. Redox Solvent Extraction Process

Special column runs have been made which indicate that the somewhat high plutonium losses (0.5-1.0 percent) in column IB are due to the slow rates of transfer. Batch experiments support this observation, for plutonium tracer is extracted from a hexone phase containing hydrazine and $UO_2(NO_3)_2$ at only about 1/10 the rate at which strontium tracer is extracted. Hydrazine and/or its reaction products with hexone appear to be responsible for this difference in rate of transfer. Remedy of this situation is difficult in the present experimental column, but it should be possible to design columns to increase the hexone holdup and thereby allow more time for the transfer to occur.

It has been found in the laboratory that 0.01-0.10 M concentrations of ammonium acetate in IBX will probably decrease the plutonium losses by the resulting increase of the distribution of plutonium in favor of the aqueous phase. Separation of plutonium and uranium will not be affected adversely.

Zirconium, cerium, and ruthenium appear to be the fission product elements carried with plutonium and uranium through the process. These three fission product species account for virtually all beta and gamma activity associated with the uranium which emerges from the process at a decontamination level of 10^4, while cerium and ruthenium account for the beta and gamma activity associated with plutonium at a decontamination level of 10^7.

Uncle Lawrence had lunch with Helen and me in our apartment. He returned to Menominee on the afternoon train. Helen and I then had dinner with the Darrell Osbornes at their home (2208 E. 99th Street on the south side of Chicago).

President Truman has named Edwin W. Pauley of California Undersecretary of the Navy. Pauley is regarded as being groomed to become the first secretary of national defense.

Sunday, January 20, 1946

I conferred with Zachariasen about various x-ray crystal spectrographs and sources of various kinds of apparatus in preparation for installation of such equipment at the Radiation Laboratory in Berkeley. We also discussed possible personnel who might be added to the Radiation Laboratory staff for such a program. Zachariasen mentioned Rundle (a Pauling man) and C. D. West (from Harvard, now in industry).

Monday, January 21, 1946

During the morning I attended a meeting of the Laboratory Council in Room 209, Eckhart Hall. Others present were Branch, Cole, Chisholm, Daniels, Dempster, Foote, Furney, Hilberry, Hogness, Hughes, Jacobson, Jesse, Lapp, Moulton, Mulliken, Nickson, Ohlinger, Stone, Willard, H. Young, Zachariasen, Zinn, and Zirkle. Among the topics discussed were:

(1) The National Declassification Committee composed of Tolman, Urey, Spedding, Colonel Ruhoff, and others has been working on the problem of declassification, but as yet, no instructions for declassification have been received at the Metallurgical Laboratory.

(2) Colonel K. D. Nichols has appointed a Committee to study the possible technical methods of achieving international control of the atomic bomb. The Committee is composed of English, Spedding, Morrison, Bacher, and representatives of industry.

(3) Small conferences will be held January 21, 1946, and January 22, 1946, at the Metallurgical Laboratory with representatives of du Pont and Hanford to discuss solvent extraction and graphite problems.

(4) Daniels announced with regret the resignation from the Laboratory of Dr. Robert S. Stone which will become effective on February 1, 1946. He expressed appreciation for the extremely effective way in which the health of all employees had been safeguarded by the efforts of Stone. Stone has agreed to serve as a Consultant.

(5) Formation of the Laboratory Release Committee authorized by a vote of the Laboratory Council at its meeting of January 14, 1946, was announced by Daniels. The Committee will consist of the following persons: Mulliken, Hogness, Zinn, Dempster, Jacobson, Lapp, H. Young, and Sergeant Stuart R. Schram. Daniels asked that Mulliken serve as Chairman of the Committee, and suggested that Sergeant Schram be asked to handle the secretarial duties connected with the Committee.

(6) The urine-collection program discussed in the meetings of December 3, 1945, and December 17, 1945, has been placed in operation. At the direction of the Health Division, evening and morning collections of urine will be obtained at home. If tests reveal the presence of as much as 4 micrograms of plutonium, the employee will be asked to stay at home for a 24- or 48-hour period for the purpose of collecting urine samples away from areas of contamination. Such absences will not be counted as vacation or sick leave. Daniels stated that the plan for four weeks vacation per year for persons working in locations where there is radiation exposure has been discussed with the Wage Policy Committee appointed by Colonel Nichols. This four-week vacation is recommended by the Bureau of Standards for all those whose regular work involves exposure to radiation. Whitaker and Daniels suggested that, in drawing up new contracts for the period beyond July 1, 1946, provisions should be considered for four weeks vacation per year for persons working in radiation areas.

A. V. Grosse called me for information on the discovery of element 94 to be included in Bradley Dewey's presidential address at the ACS meeting in Atlantic City.

Foote and I conferred about the various x-rays crystallographic apparatus suitable for installation at the Berkeley Radiation Laboratory.

I also read an official notice from Branch stating that the University of Chicago considers it a civic responsibility to serve on juries. Branch then goes on to give the Met Lab policy about compensation for such service.

A memo was given to me today by Magnusson and Hindman entitled, "Recovery of Np^{237} from UNH Waste Solution at Hanford," dated January 15. They have been attempting to determine why the yield in the run to recover Np^{237} was so low (15 percent). They consider it probable that poor reduction or instability of the Np(IV) are the most likely sources of failure in the process. The conclusion they reach is that the neptunium recovery process be revised to eliminate the long, so-called reduction treatment at 75° for 60 minutes.

I received a letter from Professor James Blaine Ramsey, my teacher of physical chemistry at UCLA. I consider him the best teacher I have ever had and a person who has had a major influence on my way of thinking on scientific matters. He writes as follows to comment on my talk at Northwestern University (November 16, 1945), a copy of which I sent him earlier this month (January 8):

January 17, 1946

Dear Glenn:

I appreciate very much the copy of Chemistry containing the reprint of your talk at Northwestern. Incidentally that is a good picture of you. I had read your article in C&E News edition of December 10 and had accepted, tentatively, your hypothesis of a rare earth-like series beginning with actinium. As I recall cerium (and the other 13 rare earth metals) are paramagnetic whereas La is diamagnetic. I assume therefore that Ac is diamagnetic, and Th et seq. are paramagnetic. I would expect $Th^{+4} \rightarrow Th^{+3}$ to be a much stronger ox. agent. than it is (probably greater than ($Ce^{+4} \rightarrow Ce^{+3}$)) on the basis of your hypothesis. However, I note that the stability of the trivalent state (in aq. soln.) of your actinide series of elements is not as great as that of the tri-state of the elements of the lanthanide series. Relative energies of similar electronic states in the two series may account for the existence of the IV, V, and VI states in the Ac series. I realize that all pertinent chemical and physical evidence which justifies your hypothesis, may be still forthcoming. I shall be looking for it. I am interested in comparing properties (color, solubilities, etc.) of compounds of Pu with those of Re, in the same valence state. Also, whether 95 and 96 are noble metals. If the latter is true, then I will wonder about your hypothesis. According to your hypothesis element 97 is the first element of a period of 32 elements and not of 18. Rydberg would then postulate his series - $2(1^2 + 2^2 + 2^2 + 3^2 + 3^2 + 4^2 + 4^2...)$.

Thanks again and best regards,

Sincerely,

Blaine

P.S. Oh yes, I failed to congratulate you on receiving the honor from the Junior Chamber of Commerce last night in Chicago. Glad it was the Junior Chamber. I will feel better when some of our strong labor unions start giving recognition in some way. Just to keep our scientists from losing objectivity.

JBR

Tuesday, January 22, 1946

Groups 1 and 3 met at 8:28 a.m. in the New Chem conference room. I attended the first part of the meeting. Other men there were Ames, Asprey, Britain, Fineman, Florin, Ghiorso, Hagemann, Hindman, Hopkins, Hyde, James, Katzin, Kohman, Manning, Osborne, Peterson, Sedlet, Simpson, Studier, S. Thompson, Van Winkle, and Westrum. I first asked each man to report on the status of his writing.

Simpson then gave results (rather unsuccessful) of the cooking of 95 out of plutonium. Florin added that if plutonium turns out to have a stable higher fluoride, it would provide an excellent method of separating element 95 from plutonium. Florin then described an experiment to volatilize protactinium fluoride away from the "carbonate residues."

Hyde gave results on cross section determinations on the Io + Th^{232} sample:

$$Io^{230}(n,\gamma)Th^{231};Th^{231} \rightarrow Pa^{231*}$$

$$Th^{232}(n,\gamma)Th^{233};Th^{233} \rightarrow Pa^{233} \rightarrow U^{233*}$$

$$Pa^{231}(n,\gamma)Pa^{232};Pa^{232} \rightarrow U^{232*}$$

*Isotopes actually measured.

	Capture Cross Sections	
	By using bombardment data and amounts actually in target	By comparing to previously determined value of σ_c for Th^{232} of 7b
Io^{230}	94.5 b	29 b
Th^{232}	23	7
Pa^{231}	259	78

The discrepancy between these two sets of data was discussed. Manning said that resonance absorption by Th^{232} might account for the differences, or that the half-life of 30 years used for U^{232} might actually be too short. Kohman suggested that the error is in the value used for the flux since the quoted value might apply to uranium slugs, and this sample was in graphite.

Van Winkle obtained 45 barns for the neutron capture cross section for Pa^{231} determined by bombardment at Argonne. Even allowing the maximum corrections for chemical yield and flux uncertainties, this value is not brought above 60 barns.

1/22/46 (cont.)

Manning suggested that $Pu^{236} \xrightarrow{\text{2.7 years}} U^{232}$ might be used as a tracer for checking of the chemical yield. He did not feel it worthwhile to try for Np^{236} and Pu^{236} in the Q metal now in the cyclotron, as it has been on so very long and there would be too many corrections for irradiation times and elapsed half-lives to make a good comparison of these α,p5n and α,6n reactions.

Hagemann will bombardment U^{233} with neutrons at Argonne as a means of analyzing for U^{238} by the amount of Np^{239} produced. One-tenth percent should produce 5,000 c/m of Np^{239} per milligram of original material. He will send out two samples determined as 100 percent and 98 percent U^{233} by specific activity, and one of 96 percent purity by mass spectrograph method, as well as a little pure U^{238} as a monitor. Neptunium-237 can be added as a tracer to check neptunium yields.

The possibility of obtaining pure Pu^{241} from the deeper layers of the next Q metal target was discussed. James stated that he has calculated that the 44 Mev helium ions are reduced in energy by Q metal as follows:

First 2 mil layer - reduced to 38 Mev

Second 2 mil layer - reduced to 32 Mev

Third 2 mil layer - reduced to 21 Mev

Fourth 2 mil layer - reduced to 12 Mev

Fifth 2 mil layer - reduced to 0.

Manning suggested that the last three layers might be further fractionated at 1 mil intervals in the attempt to get Pu^{241} (produced by α,n reaction) free from the Pu^{240} produced by α,2n reaction at slightly higher energies.

Sedlet has been working on the distribution of protactinium in the St. Louis process. The first process precipitation of $PbSO_4$ contains 0.2-0.3 ppm protactinium and is probably the most concentrated source. Radium should be present in about the same amount, and actually turns out to be there to the extent of 0.02-0.04 ppm. This precipitate contains much silicon and other insoluble residue. Van Winkle commented that the residue remaining after the radium is removed should still contain the protactinium and might be an even better source.

I left the group meeting early in order to take care of my correspondence before leaving for Lafayette, Indiana.

I replied to Grosse's telephone request of yesterday, sending him copies of my recent talks. I underlined in red those portions which might be emphasized and say that the material has the advantage of having been already cleared by Security. I then mention a few noteworthy names--Cunningham and Werner for the first chemical isolation of an isotope of pure plutonium; S. G. Thompson for the separation process; J. W. Kennedy, A. C. Wahl, C. S. Smith, C. S. Garner, I. B. Johns of Los Alamos for the purification of plutonium; C. D. Coryell and co-workers for work on fission products. I say that T. R. Hogness and W. C. Johnson of the University of Chicago contributed in an imaginative way to the chemical program.

Latimer wrote to me saying that it would be difficult to make an offer to Manning at this time since members of the department believe we should go after straight organic men before specialization in photosynthesis.

An invitation arrived from M. L. Hartung asking me to speak to the Men's Mathematics Club of Chicago and Vicinity at their dinner meeting on March 15.

The Health Survey for the week ending January 18, 1946, arrived from J. J. Nickson. Rooms with "hot spots" which need cleaning in the near future are 10, 16B, 27, 30, 31, 33, 34, 35, and 36.

Daniels requested that Chapman arrange for shipment of a 1 mg sample of Np^{237} being removed from the Hanford pile. He asks that the sample arrive here before February 8.

Helen and her cousin, Frances Chilson, accompanied me to the Woodlawn station when I caught the 2:12 p.m. train for Lafayette (Queen City Special, car 465). I was suffering from a migraine headache.

In the evening I spoke at the Purdue University Chapter of Phi Lambda Upsilon Fraternity on "Some Aspects of Nuclear Energy." The talk was similar in content to the one I gave in Detroit. I managed to get my migraine headache sufficiently under control before my talk with the help of some Bromo-Seltzer.

Helen had a dental appointment at 3:00 p.m. today.

<u>Wednesday, January 23, 1946</u>

I returned to Chicago by train from Lafayette.

Madeline Sohn, a research assistant who began working two weeks ago for Don Stewart, resigned today because of her fear of health hazards. She was promptly replaced by Silvia Warshaw, who comes from Eastman Corporation, Oak Ridge, Tennessee.

A letter arrived from J. Edwin Pasek, Manager of the Industrial Division of Prentice-Hall, Inc. He heard the broadcast from Chicago of the Junior Association of Commerce and extended his congratulations to me.

A. E. Florin prepared a memorandum for me describing the preparation and properties of neptunium hexafluoride.

Acting upon a request from A. J. Dempster, Daniels asked Chapman to arrange for an exposure of samarium and gadolinium samples at Hanford. Daniels explains that our group will prepare the thin layer of samples to be mounted in slugs. Stewart will handle this for us.

In another memo to Chapman, Daniels acknowledged the letter announcing that it may be possible to separate some of the more common isotopes by the electromagnetic method. Daniels suggests a number of isotopes in which there is great interest: U^{234}; isotopes of iron, chromium, and nickel; ionium; U^{235} in very high purity; Pu^{240}; and isotopes of cadmium.

Helen attended a pre-natal class at Lying-In Hospital. We visited the Ghiorsos during the evening.

General Mark W. Clark has been nominated for permanent rank of Major General by President Truman according to today's paper.

Thursday, January 24, 1946

Al Ghiorso is working at Argonne today and tomorrow.

After reviewing references cited by Karapetoff in his early article in the Journal of the Franklin Institute, I again wrote to him today, saying:

> It seems to me, then, that there were a number of predictions in respect to this matter during the period from 15 to 20 years past and that it would be difficult to signal out any of them as displaying particularly more perspicacity than any others. I feel that I should have been better versed in the literature on this subject but all of these publications cover a period considerably before that at which I had begun to read the literature and I have not recently had a sufficient amount of time to delve into the older literature to any extent. I am grateful to you for calling my attention to this early work which I found very interesting to read.

I wrote to Martin Kamen and Milton Burton asking both men to give an invited paper at the nuclear chemistry symposium during the ACS meeting in Atlantic City.

A number of letters of recommendations went out today; to Russell S. Drum of Battelle Memorial Institute about Jerome J. Howland, Jr.;

to D. J. Salley of American Cyanamid Company, about Duffield, Hagemann, and Howland. (To Salley, I also mention that Bernard Abraham is possibly interested in a position with him.) I wrote to Warren C. Johnson about Martin Studier who is applying for a graduate fellowship at the Institute for Nuclear Studies. A letter arrived from P. Gerald Kruger of the University of Illinois, requesting a formal letter of recommendation for Robert Duffield (Duffield has already accepted Kruger's offer, but Kruger needs a letter for use in the formal appointment papers).

John T. Tate, Editor of The Physical Review, wrote asking if the authors are now willing to publish the following papers submitted several years ago and voluntarily withheld from publication:

"Radioactive Element 94" by Seaborg, McMillan, Wahl and Kennedy

"Radioactive Element 94 from Deuterons on Uranium" by Seaborg, McMillan, Kennedy, and Wahl

"Radioactive Element 94 from Deuterons on Uranium" by Seaborg, Wahl, and Kennedy

In a memo to Daniels, Zachariasen verifies Al Florin's recently submitted sample as NpF_6.

After a conference with Hood Worthington, Daniels modified our request No. 9 for irradiation of one gram of radium to 0.1 gram. Daniels says such an irradiation is the best way to produce a source of actinium.

Helen went to the library in the afternoon; she plans to spend the evening with Wilma.

I left Chicago for Los Angeles on the "City of Los Angeles." Unfortunately, I could do no better than get a coach seat and I considered that I was lucky to get that with the help of a young man, named Butler, whom I met at the Junior Chamber of Commerce dinner last week.

According to this morning's newspaper Professor Frederic Joliot-Curie said France was preparing to develop an atomic bomb a few weeks before the German occupation of France; he claimed they would have had the bomb in one year. Now he says that they will not go into large-scale production of bombs but will endeavor to exploit nuclear energy.

Friday, January 25, 1946

Enroute to Los Angeles on the "City of Los Angeles."

In Chicago, Al Ghiorso is scheduled to spend another day at Argonne.

During the day I wrote Helen a letter. Fortunately I was able to get an upper berth for tonight; apparently Butler enlisted the friendly cooperation of the conductor to make this possible.

News of Bikini atoll test appears in newspapers across the country today.

Saturday, January 26, 1946

I arrived in Los Angeles in the morning and went to my parents' home in South Gate, where I was interviewed by a Los Angeles Times reporter and by Mrs. Maxson of the Watts Advertiser-Review. The former featured the future role of atomic energy and the latter my experience as a student at David Starr Jordan High School in Watts. At 1:30 p.m. William Shand, Jr., of Cal Tech came to see me to discuss plans and equipment necessary for an x-ray laboratory in Berkeley.

Fred Albaugh and I played nine holes of golf at Rio Hondo (GTS-48, FA-64). We then went to Fred and Edrey's home for an evening open house with Vance and Mary Cooper, Zene and Celeste Jasaitis, and Harlan and Nathalie Baumbach.

Sunday, January 27, 1946

I am at my parents' home in South Gate. Saul Winstein came by to visit me.

I took the evening train (Southern Pacific) to San Francisco.

Monday, January 28, 1946

I arrived in San Francisco and went to Berkeley. I visited with Latimer in Gilman Hall to discuss plans for my return to Berkeley this spring. I also went up to the Radiation Laboratory on the hill to visit and have discussions in Building 4 with the members of my group who have moved to Berkeley--Iz Perlman, Tom Morgan, Herman Robinson, and Louis Werner. Jerry Howland completed his employment "red tape" and went on the Berkeley payroll today at $330 per month.

I had dinner with the Calvins at their home in Berkeley (1421 Leroy Avenue). The Perlmans, who are staying with the Calvins, were also there.

I am staying at the Durant Hotel. I wrote to Helen before going to bed.

A report from Washington indicates that the Bikini atomic bomb target fleet may be buffeted by 1,000-mile winds and 100-foot waves.

Tuesday, January 29, 1946

In Berkeley.

I went to Richmond to look at the possibilities of wartime housing for us on our return to Berkeley. Housing is very tight.

I played about 13 holes of golf at Tilden Park with Jerry Howland, Tom Morgan, and Iz Perlman. (For nine holes JH-66, TM-58, IP-57, GTS-57.)

Al Ghiorso is scheduled to be in Montreal today.

Wednesday, January 30, 1946

In Berkeley. Paul Nahin of the Union Oil Company, Wilmington, California, met with me this morning to discuss the availability of various radioactive tracers. Nahin mentioned that they would like me to speak at their local ACS meeting sometime. We also talked about the possibility of Union Oil making research grants to universities such as UC and Cal Tech.

Ernest Lawrence and I met to discuss the development of the chemistry and plutonium programs. We decided that we need a complete file of Metallurgical Project reports here. Lawrence will ask Captain R. L. Miller to start a request through channels for such a transfer of past and future reports. Lawrence also said that he will inform Colonel K. D. Nichols in Oak Ridge about our need.

Thursday, January 31, 1946

In Berkeley. I continued my talks with the members of my group in Building 4.

I had dinner with Jeanette, who came in by bus from Camp Parks, and then called Helen.

FEBRUARY 1946

Friday, February 1, 1946

In Berkeley.

In Chicago, Oliver C. Simpson is scheduled to transfer to Section C-II. Norman D. Erway is being promoted from Glassblower B to Junior Chemist, and T. R. Hogness is now on half-time.

The Berkeley Daily Gazette says that "Dr. Lindsey Helmholz, noted scientist" will speak under the sponsorship of the American Veterans Committee on Atomic Energy. He is visiting the Radiation Laboratory.

Saturday, February 2, 1946

In Berkeley.

I read a copy of a memo Donald Cooksey wrote to Captain R. L. Miller requesting that a complete set of Metallurgical Project reports be transferred to the Radiation Laboratory, attention Dr. A. Guthrie, for our use.

Melvin Calvin wrote a memo to me in which he asks, for promotion purposes, my confidential evaluation of John H. Lawrence. I immediately dictated a letter to Mary Millard, our secretary here at the Radiation Laboratory, saying I have a high opinion of Lawrence and his accomplishments. I also say that he is one of the pioneers in the application of artificial radioactive tracers in medical research. In many instances he was the first to apply the radioactive technique as a new principle. Lawrence was one of the first persons to use radioactive phosphorous as a tracer in the study of the mechanism of normal and tumor metabolism. I go on to say that, in my opinion, he has the reputation of one of the leading investigators in the country in this field of research.

In another evaluation letter to Calvin, I write that I consider Dr. Joseph G. Hamilton, who is still ranked as an assistant professor, as one of the outstanding men in his field. I say that Hamilton is characterized by a great breadth and diversity of ability. His work in artificial radioactivity includes not only application of tracers to biology and medicine, but also fundamental work in the discovery and identification of new artificial radioactive isotopes. In addition, he has undertaken a problem so basically in the physics area as rebuilding of the Berkeley 60-inch cyclotron, making it capable of delivering much higher energy deuterons and helium ions. Hamilton has a great number of original ideas, stemming from his unique detailed knowledge of the biological, chemical and physical aspects of nuclear work.

I also dictated an appraisal of Burris Cunningham's abilities and qualifications for Professor Latimer to support his appointment as an assistant professor at the Radiation Laboratory. I say that I particularly want to signal out Cunningham's accomplishments on the Plutonium Project. He and his men made a thorough investigation of the chemical properties of plutonium including the preparation and identification of a large number of pure compounds, working with a total supply which amounted to no more than ten and hundreds of micrograms at any one time. This was accomplished by the use and development of extraordinary techniques in the field of ultramicro-chemistry. Cunningham was responsible for the first isolation of a pure compound of plutonium, which represents the first isolation of a pure compound of a synthetic element. I go on to say Cunningham is quiet with a pleasant personality, a real scientific scholar whom I recommend highly.

Perlman and I met at 1:30 p.m. for a final discussion about our plans for the group.

In the late afternoon I boarded the "City of San Francisco" for Chicago.

Today's Berkeley Gazette notes that Trygve Lie was installed as the first Secretary General of the United Nations today.

Sunday, February 3, 1946

Enroute to Chicago on the "City of San Francisco."

Monday, February 4, 1946

I arrived in Chicago about noon.

Helen briefly summarized for me her activities while I was gone. The day after I left, Helen went shopping in the Loop. A week ago last Saturday she had dinner with the Ghiorsos, and on Sunday had dinner with the Stan Thompsons. On Tuesday she had a dental appointment at 11:00 a.m. and at 1:00 p.m., a doctor's appointment. That evening Helen had dinner with the Royal Smiths, Edrey's parents, who live at 9514 South Damon Street. Last Wednesday she attended a pre-natal class at Lying-In Hospital. She spent the late afternoon with Wilma, had dinner with Kathleen Hughes and Ethaline Cortelyou (two of her fellow workers when she worked at the Met Lab on my "Table of Isotopes"), and then spent the evening with Wilma. On Thursday Helen had a 9:30 a.m. dental appointment, then she went downtown with Marjorie Osborne. Wilma and Kristine dropped in during the afternoon. On Friday evening Helen visited Wilma.

The following events took place in Chicago while I was away:

Friday, January 25

I received a copy of a letter Daniels wrote to Hanford, attention of L. Squires, giving a bibliography prepared by Lawroski and Blaedel on the Redox Process. The minutes of the Solvent Extraction Meetings were included. Daniels states that a complete report on the process will be published at the end of March.

A letter arrived from Lavender, acknowledging receipt of the joint letter (December 11) from Kennedy, Segrè, Wahl, and me. He encloses a photostatic copy of our agreement saying that he wants to keep the orginal in escrow. Lavender states that he has heard nothing from the University of California. Lavender further states that he cannot send us copies of the application because of security, but he can arrange for us to see copies through the Patent Group at Santa Fe or Berkeley.

A letter arrived from James M. Crowe of C & E News announcing a new section to be known as "Nucleonics and Atomic Energy Forum." Edgar J. Murphy will act as consulting editor. Crowe asks my thoughts and advice on any phase of getting reliable information for publication. Crowe then requests a copy of my Detroit ACS talk.

Edgar Waite of Standard Oil Company wrote, in a letter dated January 23, that he is going to have John L. Sullivan contact me in Berkeley about the article on Stan Thompson.

Saturday, January 26

E. P. Wigner wrote a note saying, "We are delighted at your acceptance of the invitation to the Bicentennial. Thank you for letting me have the title of your address at such an early date."

An addendum to John T. Tate's letter of January 22 arrived. He notes that he has an additional paper, "Properties of 94-239" which he is holding, and again wonders if we wish to publish the paper now. Tate also questions the authorship of this paper. Originally the authors were Seaborg, Segrè, Kennedy, and Lawrence; however, at some point two of the names were crossed out--Segrè and Kennedy.

Don Stewart sent a memo to Segrè to inform him that we are sending him 3.7×10^6 c/m of 95^{241}. The isotope was milked from 240 gt plutonium. Spectrographic analysis shows only 0.7 percent lanthanum. Sample designation is GTS No. 133.

Monday, January 28

Mary Jane Healy began working as a secretary in Section C-I.

A letter arrived from George Everson enclosing a copy of his offer to Dave Templeton. Everson says that he already has sent an offer to Jerry Howland although Howland's trunk arrived before the offer went out. Everson plans to be in Chicago about February 11.

Tuesday, January 29

Groups 1 and 3 met this morning in the New Chemistry conference room. Present were Ames, Asprey, Britain, Cunningham, Fineman, Florin, Hagemann, Hindman, Hopkins, Hyde, Jaffey, James, Katzin, Kohman, Manning, Osborne, Peterson, Sedlet, Simpson, S. Thompson, Van Winkle, Warshaw, Weissbourd, and Westrum. Manning opened the meeting by asking for a report from the radium analysis group. Fineman replied that another run had been made in the search for an emanation in the neptunium decay series, but that nothing definite has resulted as yet. Manning then asked about the sensitivity of the method and the lower limit which it will give on the alpha-particle branching ratio for the decay of neptuno-radium (88^{225}), to which Kohman replied that the limit was about the same as previously given (0.1 percent for a Em^{221} half-life of one to several hours). Manning and Kohman discussed the possibility of an error being introduced in the weighing operations as a result of heat generated by radioactive decay of the radium. It was concluded that such an error was probably negligible because of the small size of the samples being weighed (200 µg). It was also concluded that the error resulting from recoil is negligible.

Simpson, in talking about the volatilization of 95^{241} from high gt plutonium, referred to the results reported last week and said that the increase in the amount of plutonium distilling into the 95 fraction with time of heating could now be ascribed to a slow decomposition of PuO_2 to a more volatile form. A small amount of water added to the crucible was observed to decrease the amount of plutonium distilling into the 95 fraction by converting plutonium to PuO_2, and thereby increasing the percent of 95 alpha particles in the volatilized fraction from 1.8 percent to 6.8 percent. Further stringent oxidation decreased the ratio of 95 to plutonium, however, and may be the result of oxidation of 95 to a less volatile form. If PuO_2 can be kept in the dioxide form, there is good evidence that purification of 95 from plutonium will be quite satisfactory. It was possible to distill out 35 percent of the 95 in the crucible, with an increase in percentage of alpha disintegrations due to 95^{241} from 0.35 percent to 8 percent in the volatilized fraction. The volatilized fraction contained 20,000 c/m of 95^{241}.

Manning announced that the salt residues containing 95^{241} will arrive from Site Y about February 1, and that the plutonium metal is scheduled to arrive about February 15; Asprey commented that the salt residues would probably occupy a volume of 30-40 gallons. The first concentration step will have to be carried out in a barrel.

Cunningham talked about the 95^{241} studies and said that he has prepared five micrograms for bombardment at Hanford, two micrograms for measurement of fission, and a sample for Segrè. The fission measurements indicate that the recently purified material contains 0.5 percent plutonium. Spectrochemical analysis shows also 0.7 percent lanthanum. The spectral lines for element 95 that were reported previously have been confirmed.

Hopkins described the radiation properties of 95^{241} as determined on a sample of the recently purified material. The absorption curves are quite similar to those obtained by Asprey on samples which were milked from plutonium at an earlier date. Beryllium, aluminum, lead, beryllium plus aluminum (and beryllium plus lead) absorption curves were run with a G.M. counter and three components were observed corresponding to the $K\alpha_1$, $L\alpha_1$, and $L\gamma_2$, x-rays of a heavy element.

Possible Assignment	Energy kev	Frequency
$K\alpha_1$	109	1 d/m per 12 α d/m
$L\alpha_1$	14.6	1 d/m per 21 α d/m
$L\gamma_1$	22	1 d/m per 120 α d/m

Manning asked how the yield of 95^{241} in the latest milking compares with the yields obtained in earlier milkings, since there is a possibility that the 95 milked from plutonium (high gt) consists of two different isotopes whose rates of growth change with time, and Cunningham replied that the yield obtained is what is expected for 171 gt plutonium.

Osborne inquired as to the source or origin of the 95^{241} x-rays, since there appears to be no gamma-ray to undergo internal conversion in the K and L shells. James, Florin, and Katzin thought the loss of energy by alpha particles as they pass through the electron shells might be sufficient to give rise to x-rays.

James reported that he has isolated U^{232} from a sample of Pu^{236} and that the yield which he obtains for a known time of decay corresponds to a half-life of 65 years for U^{232} (assuming $T_{1/2}$ for Pu^{236} = 3.0 y). A known amount of U^{233} was added to the sample to serve as a tracer for U^{232} and the ratio of U^{232} to U^{233} was determined in the pulse analyzer. An error in the yield determination would result if U^{234} were present since it would be hidden under the U^{233} peak and would cause the yield determination to be low.

Hyde summarized the results of the helium ion bombardment of U^{233}. He said that ether extractions with bromate oxidation have been used to separate neptunium, uranium, and plutonium from fission products. Then a 0.15 M solution of TTA in benzene was used to extract Np(IV) away from U(VI) and Pu(III) after a reduction with 0.1 M NH_4I. A considerable amount of uranium was observed to extract into the TTA along with Np(IV). Several washes containing NH_4I failed to remove the uranium. The aqueous solution containing Pu(III) and U(IV) was oxidized with potassium iodate to convert Pu(III) to Pu(IV). Then TTA in benzene was used to separate Pu(IV) and U(VI). The plutonium fraction was quite free of uranium. The good decontamination from uranium obtained under oxidizing conditions contrasts sharply enough with the poor decontamination from uranium obtained under reducing conditions to suggest that U(VI) is being partially reduced to U(IV) by 0.1 M NH_4I. The plutonium fraction shows a peak upon pulse analysis which corresponds to the previously observed alpha-particle range for Pu^{236}. There is also a small peak which decays with a half-life of five to ten hours. This ten-hour activity has the same range as the ten-hour activity observed in the first bombardment of U^{233} with helium ions. The isotopes expected in the plutonium fraction are as follows: $U^{233}(\alpha,n)Pu^{236}$, $U^{233}(\alpha,2n)Pu^{235}$, and $U^{233}(\alpha,3n)Pu^{234}$. No plutonium isotope decaying by orbital electron capture with a 40-day half-life was observed. This is in agreement with James' assignment for a 40-day orbital electron capture decay which was observed in the bombardment of U^{235} with helium ions to the isotope Pu^{237} since it is not possible to obtain Pu^{237} in a helium ion bombardment of U^{233}.

Hyde noted that not more than 10 to 20 c/m of long-lived x-rays were observed in the plutonium fraction. He then continued with his description of the separations procedures used in working up the bombardment, indicating that it was necessary to re-oxidize the neptunium fraction which contained uranium contamination in order to convert uranium(IV) to the uranyl form. The addition of ammonium iodide and hydrazine was used to reduce neptunium to the +4 state. TTA extraction then was used to separate neptunium from the uranium contamination. The final step in the purification from uranium consists of a lanthanum fluoride precipitation, and essentially all of the uranium is removed from the neptunium by this procedure. There are a considerable number of x-rays observed in the 93 fraction, which are decaying with a 4.5 day half-life, and are probably due to the isotopes Np^{234} or Np^{233}.

Manning said that the ten-hour alpha decay is probably not Pu^{235} since James would have observed it in his U^{235} target. This ten-hour decay is probably Pu^{234} formed by an α,3n reaction on U^{233}. Simpson predicted that Pu^{234} should have an alpha half-life from one day to one year, and Manning remarked that the x-ray decay observed in the plutonium fraction from the U^{233} target shows an initial rapid decay which might be ascribed to a ten-hour decay. James said that U^{230} and its daughters should show up in the plutonium fraction from the U^{233} bombardment if the ten-hour alpha decay is due to Pu^{234}. Hyde replied that the plutonium fraction has been examined in the pulse analyzer for the presence of the U^{230} series. Two broad peaks were observed beyond the 94^{236} range, which might be ascribed to the U^{230} series if poor resolution was the cause for the observance of only two peaks instead of five.

Manning remarked that a one-hour helium ion bombardment of U^{233} in the Berkeley cyclotron, if worked up immediately, would serve to clinch the isotopic assignment of the ten-hour alpha decay since it would then be possible to milk both Np^{234} and U^{230} from the plutonium fraction. Kohman asked about the alpha range of the ten-hour decay, to which Hyde replied that it was beyond the range of the 94^{236} alpha particles and corresponds to a range of approximately 4.5 to 5 centimeters. Weissbourd's best estimate as to the range of this short-lived alpha emitter is 4.7 cm.

Peterson summarized Ghiorso's fission studies last week at Argonne. (Ghiorso was away on a visit to Canadian Laboratories.) He said that qualitatively all the isotopes which were not expected to fission with slow neutrons did fission with small cross sections. Protactinium-231 was tested a month or so ago for fissionability and observed to have a cross section of approximately 0.04 barn. This sample was given a purification from both plutonium and uranium by Van Winkle and the result on this purified sample was again approximately the same. Neptunium-237 showed some fission which might be the result of plutonium contamination. Uranium-232, obtained from neutron bombardment of Pa^{231}, also showed some fission. However, it was recognized that a small trace of natural uranium in the protactinium could have caused the fission rate which was observed. 95^{241} fissioned at a higher rate than an earlier sample, and the fission rate observed was probably due to plutonium contamination. Thorium-230 also showed a small amount of fission. Thorium-232 showed a smaller amount of fission than the Th^{230}. Actinium-227 was tested and showed a small amount of fission, but it was thought that merely counting it in our alpha counters might have contaminated it with plutonium sufficiently to give the fission rate observed. Thorium-227 and Ra^{223} were tested for fissionability a month or so ago. The same two samples were tested again last week, and in both cases, the fission rates observed decayed with half-lives comparable to the half-lives for the two isotopes. New samples of Ra^{223} and Th^{227} were tested for fissionability. It was not possible to have an absolute determination of their fission cross sections but they were of the order of 500 barns. Radium-226, a 50 μg sample which was supplied by Ames, showed some fission. However, the behavior was somewhat different from in the case of some of the other isotopes tested since, by surrounding the tube with cadmium which absorbs thermal neutrons, it was observed that the fission rate was reduced to the background of the counter. This suggests the presence of contamination due to one of the three fissionable isotopes--Pu^{239}, U^{238}, or U^{235}--since the fission cross sections for these isotopes decrease rapidly with increase in neutron energy in the neighborhood of the thermal region. A 95^{241}-96^{242} mixture, which was repurified by Hopkins, again showed a high fission cross section which corresponds to 1,800 barns for 96^{242}.

Stan Thompson remarked that 16 α c/m of Pu^{239} would be sufficient to give the fission rate observed for 96^{242}, and Kohman said that it is not surprising to find that isotopes such as U^{232}, Th^{230}, Ra^{226}, Pa^{231}, Ac^{227} undergo fission (with small cross sections obeying 1/V law).

Peterson then described some modifications which Ghiorso has made on the fission counter to reduce the background and increase the counting efficiency.

Hagemann talked about the recent neutron-induced fission measurements carried out on pure U^{233} and a sample of U^{233} which contains some U^{234}. He stated that previous measurements on these two samples resulted in a lower fission rate for the pure U^{233} sample than was observed for the U^{233}-U^{234} mixture. The most recent measurements made last week, however, show that the fission rate for the pure U^{233} sample is now somewhat larger than the fission rate for the U^{233}-U^{234} mixture. Katzin conjectured that this latest result would probably correspond to 0.5 percent to 1 percent of U^{234} in U^{233} and would give a cross section for neutron capture by Pa^{233} of 100 barns ± 50 percent. The actual calculations have not been made.

Manning announced that we were turned down on our request to have the CW-4 sample placed back in the pile at Hanford since the cans must be retested for leaks, and that would be too much of a health hazard. Therefore, it will be necessary to place some of our high gt material in the Hanford pile and give it a long bombardment. There is some question as to whether it should be made up in the form of metal foil or as the oxide.

Kohman reported on the results from Chang at Princeton on measurements of the alpha ranges associated with Ra^{226}. The resolution of the large peaks was not much better than what Ghiorso obtains with the differential pulse analyzer.

<u>Wednesday, January 30</u>

A hand-written letter, dated January 28, arrived from Bertrand Goldschmidt in New York. He says,

> I have been asked to leave the project and this is because of my official French ties. You know how I feel about it, I don't feel it would be fair to let my country down specially when it is in a mess. They expect me back and I don't

want to deceive them. It has been a sudden blow and I must confess that I am quite disappointed to leave just when I had been given recently a responsible position and before the process I worked on had been scaled up. It can't be helped, everybody has been very nice with me, including Groves who is closely connected with the decision. I saw him in Washington a few days ago. I am staying in New York with my mother for a few days and leaving by the beginning of next week on the U.S. liner Washington directly for France. My address shall be Laboratorie Curie, rue Pierre Curie, Paris. I am sorry not to see you and Helen before leaving but now it is finished, I do not want to hang around. I can't complain, these last years have been a great experience of which I will always remember the marvelous exciting period in Chicago, the way you taught me this new field and also your very friendly American hospitality.

I must also thank you for your nice offer to join in California. I would love to work under you, perhaps later, but it can't be done now. We must wait for better international collaboration.

I hope you will have a lot of luck in your Berkeley organization and I expect to see a lot of new discoveries of new radioelements in the papers. We were all amazed by your lecture at Chalk River. Perhaps you will come to Europe and give us a lecture in Paris and visit our lab, hope it will be very soon. I will write to you and Iz, we must keep in touch to have a link all ready when international collaboration will start.

Give all my regards to Helen; I want to be sure we will meet soon, I will try. Lots of luck to you, dear Glenn.

Bertrand Goldschmidt

Hopkins prepared and sent me a memo, "Absorption Curves of 95^{241} Radiations." It contains the material he summarized at the group meeting yesterday.

<u>Thursday, January 31</u>

Today Jack Steinberger, recently discharged from the Army, was hired as a Junior Physicist to work with H. L. Anderson on Light Isotopes.

<u>Saturday, February 2</u>

Jane Horwich, one of our secretaries, resigned today, Aaron Novick, who has been in Los Alamos, was rehired as a Chemist to work with H. L. Anderson on Light Isotopes.

Sunday, February 3

Today's paper contains an article titled "Truman Asks Rule by Civilians." President Truman asked Congress to enact "with utmost speed" legislation placing atomic energy under exclusive civilian control and guaranteeing "genuine freedom" for independent research in its development.

* * *

This morning, in a memo to Furney, Manning lists our anticipated requirements for U^{235} as 1 gm of 87 percent U^{235} (this is not needed if 95 percent material is available immediately) and 10 gms of 95 percent or better U^{235}. Manning comments that we have already requested one gram of 95 percent material which we would like immediately, but we are willing to wait for the remaining 10 grams if it is possible to get purer U^{235} by doing so.

I received and read an account of Al Ghiorso's visit to Canada. On January 29 Ghiorso was given a tour of the NRC Chemical and Physical Laboratories in Montreal by Henry Seligman and A. G. Ward; Ghiorso was particularly interested in the electronic circuits. He talked with A. C. English about the measurement of the 4n + 1 series--English gave Ghiorso the latest Po^{213} half-life value as determined by J. V. Jelley. In the afternoon G. C. Hanna and Dr. Ward discussed the electronic circuits of the new "kick-sorter." At Deep River, on the next day, Ghiorso talked with Dr. Barker and George Weil about possible future use by American scientists of their planned, high neutron flux pile. Weil gave a tour of the pile construction. Dr. C. M. Watson-Munro explained the construction of the ZEEP installation. On January 31 Leo Yaffe showed Ghiorso the Chemical Research Laboratory. They discussed instrumentation techniques, and examined the electronic monitoring for the hot laboratory. Geoffrey Wilkinson gave a lecture on fission product yield data. Ghiorso notes that, although he could not participate in the discussion, it was evident that there is a tremendous duplication of effort in the Canadian and U.S. work. Mr. Cranshaw and Ghiorso then discussed theory and practical experience with ionization chambers used for measuring alpha-particle energies.

Roy Thompson for the Federation of American Scientists, who has attended the meetings in Washington, D.C., of the McMahon Committee on Senate Bill No. S-1717, sent me a memo in which he reports that the attitude of some members of the committee has been hostile. Thompson reports that McMahon has stated that a single administrator is desirable and McMahon is strongly against military control. Thompson says that McMahon feels that General Marshall (after retirement) would make an excellent Commissioner. He says that Senator Millikin favors continuance of the Manhattan District for another year. Senator Johnson is concerned about achieving the utmost in congressional control of atomic energy and thinks the McMahon Bill gives too much control to the President and too little to the Congress. Thompson reports that Senator Hickenlooper views atomic energy as essentially a military problem. Thompson goes on to say,

> It is my impression and that of many others that to push the present bill or anything like the present bill through the committee will be a herculean task. Many go so far as to consider the McMahon Bill a dead pigeon and predict the passage of a slightly revised version of the May-Johnson Bill. Right now is certainly the time to exert every possible pressure on the committee.

I can say all of us here favor the McMahon Bill, the best hope for civilian control of atomic energy, as opposed to the May-Johnson Bill, which has many aspects of military control.

The Laboratory Council met in Room 209, Eckhart Hall, at 9:00 a.m. this morning. The following persons were present: Branch, Chisholm, Cole, Daniels, Dempster, Foote, Furney, Hughes, Jacobson, Jesse, Lapp, Manning, Mulliken, Ohlinger, G. Young, H. Young, Zachariasen, Zinn, and Zirkle. Following is a resumé of the topics discussed:

The recent statement by the Army that enlisted Army personnel (SED men) should not engage in activities of the Atomic Scientists of Chicago was discussed by Daniels. He read his letter of protest.

It was announced that Colonel Frye has been appointed as the new Area Engineer for the Metallurgical Laboratory.

Tentative plans for a Regional Laboratory to be constructed at the Argonne Laboratory are progressing. Division Directors have submitted preliminary plans for the year July 1, 1946, to July 1, 1947, for possible programs assuming a budget essentially equivalent to that for the present year is available.

Daniels announced plans for the return of Eckhart Hall to the University. The evacuation of Eckhart should be accomplished prior to April 15, 1946. The Laboratory Director and a staff will move to New Chemistry as well as the Theoretical Physicists and the Health Physics group. The Travel and Audit group, the Patent group, and the Office Services Department will be relocated in the Armory.

The Council by straw vote authorized Daniels to write to Senator Brien McMahon and explain that lack of legislation for the creation of an Atomic Energy Commission is jeopardizing the position of the Metallurgical Laboratory and is endangering the defense program of the nation; the Laboratory Council members unanimously endorse the McMahon Bill.

Mulliken stated that certain volumes of the PPR will be expanded to become overall District volumes. These include volumes on analytical chemistry and on the chemistry and metallurgy of uranium.

Tuesday, February 5, 1946

I attended the meeting of Groups 1 and 3 in the New Chemistry conference room at 8:28 a.m. Others present were Asprey, Cunningham, Florin, Ghiorso, Hindman, Hopkins, Hyde, Jaffey, James, Katzin, Manning, Osborne, Peterson, Scott, Sedlet, Simpson, Studier, S. Thompson, Van Winkle, Weissbourd, and Westrum. I asked for and received reports from everyone on the progress of the writing. I then encouraged everyone to keep plugging away.

Hyde then said that he has caught the tail of an x-ray decay curve in the plutonium fraction from the helium ion bombardment of U^{233}. The very poor bombardment history makes investigation of the short-lived activities very difficult. I mentioned that 2 mg of U^{233} fell off the target. This sample was bombarded and worked up initially in Berkeley and some short-lived alpha activity was found. At the end of the bombardment this sample was bombarded overnight with a terrific beam. There is not a very high ratio of the short-life activity to that of Pu^{236}. Probably the plutonium was not sufficiently separated from U^{233}, and Hyde added that several cycles are required for this chemical separation.

Studier said that the experiment to separate Th^{226} from a plutonium fraction could be best done in Berkeley. Ghiorso suggested shipping out mica absorbers to be used in Berkeley for range analysis. Studier mentioned that the range of the new plutonium activity is 4.7 cm. I added that it would also be possible to ship out Hyde or Studier, but it would interfere with the writing program. Manning commented that California would be nice in March if the writing were finished. I agreed but added that I do not want this to be misconstrued as putting pressure on the writing. However, short-lived samples must be worked up near the cyclotron. It is difficult to adjust airplane schedules to the cyclotron operation. Hyde commented that the couriers do not mind being trapped in Berkeley.

Osborne reported on the Pu^{238} made from neutron bombardment of Np^{237} and said that 0.92 mg of neptunium was bombarded at Hanford for 80 days, but the bombardment actually corresponded to 60 full days of bombardment. The bombardment was steady for the last ten to 15 days so the saturation quantity of Np^{238} is present in the sample. After decay of the Np^{238}, there should be 3 to 10 micrograms of Pu^{238} depending upon the neutron flux. Manning said that there has been no information received on the neutron flux of the bombardment. Osborne then said there is enough Np^{238} in the sample to make a study of its radiation. The ratio of the Pu^{238} growth to beta decay of Np^{238} will be taken to get a value of the relative half-lives. Manning suggested looking for Pu^{236}

which should be present relative to Pu^{238} to a lesser extent than in ordinary pile plutonium. Osborne said that TTA would be used to separate the neptunium fraction, then bromate cycles could be used to purify the plutonium. It will be possible to run fission experiments on both the original plutonium fraction and the plutonium growing into the neptunium fraction. Manning felt that there would be less than 0.5 percent Pu^{239} in the plutonium fraction. I commented that the Np^{237} after this experiment would be very good for fission testing since it would be very free from plutonium. Osborne said that the calculated Pu^{239} content in the sample neglects its formation by neutron capture in Pu^{238}. Osborne added that he started to work on the sample in Room 1 which is across the hall from Room 11, the hot laboratory where the plutonium metal is being dissolved. When he found that the wind was blowing from Room 11 to Room 1, he changed his plans because of the hazard of contaminating his Pu^{238} with Pu^{239}.

James said that he has nothing new on the half-life of U^{232} from the experiment in which it was milked from Pu^{236}. He has prepared a sample to look for U^{232} in a uranium fraction separated without U^{233} tracer. The ratio of U^{232} to U^{233} in the previous sample indicated a half-life for U^{232} of 65 years. However, a small amount of U^{232} coming through the chemistry from the original cyclotron-bombarded U^{235} could cause a big effect.

I commented that current negotiations with Site X will get us some 50 percent U^{234}, but only by a miracle could the plutonium metal for cyclotron targets be here by April 1. Then Ghiorso asked if the plutonium could be chipped off a bomb.

Ghiorso recommended a deuteron bombardment of Pa^{231} to obtain U^{230} for fission measurements.

Manning asked when a separation of pure ionium (Th^{230}) could be obtained. I guessed a few months, and Manning said that in bombardment of ionium with deuterons to get a U^{230} sample there should be no interfering isotopes. Ghiorso commented that in such a sample U^{231} would be present. The fissionability of U^{231} could then be followed first, and after it is decayed then that of U^{230} could be followed.

Simpson suggested that if you can find a d,5n reaction the n,γ reaction should also show up due to the large number of neutrons produced. James said that on milling of uranium targets, a constant plutonium level was reached which was ascribed to neutron-induced activity. I added that many isotopes in cyclotron targets used to be ascribed to neutron reactions, but no longer is this always the explanation. They used to say something was due to an n,2n reaction instead of a d,t reaction or some such thing. The energy dependence of yield often rules out the neutron reactions.

Ghiorso reported on his neutron-induced fission measurements. Thorium-227 previously showed a cross section of 470 barns. The same sample corrected for 44 days decay shows 460 barns. Radium-223 showed 1,200 barns before and now 1,300 after decay. I questioned why so much emphasis was placed on the old samples and not on new ones, and Peterson explained there was bad luck in preparing the new samples. Ghiorso reported that the Th^{227} has decayed to 18 percent of its previous amount, the Ra^{223} to 6 percent. Manning and I thought the results were significant. Ghiorso said that repurified 96^{242} shows a cross section of 1,800 barns; the previous measurement showed 1,200 barns. Manning thought that the 95^{241} sent to Clinton for making 96^{242} tracer by neutron bombardment should give a better sample. Ghiorso said that material from 49NH may be good. He then commented that perhaps the long-lost 96^{243} was appearing although, if so, it would have to have an extremely high cross section. Ghiorso believes the 170 barns cross section found for U^{232} is probably the result of natural uranium contamination. The sample probably contains 400 micro-micrograms of natural uranium and 1,100 micro-micrograms of U^{232}. The sample of Ra^{226} was found to contain 254 fission counts corresponding to 0.1 percent of natural uranium impurity. Ghiorso then said that Th^{229} gave a cross section of 36 barns. Silver paint has effectively reduced the background by a factor of ten. Forty fission c/m were found to be due to the platinum plates. The neutron capture cross section of Pa^{233} is probably about 200 barns. Since 1 percent U^{234} was found in the U^{233} sample, it was probably produced via the $Pa^{233}(n,\gamma)Pa^{234}$ reaction, in the special Hanford neutron bombardment of a sample of Th^{232}.

Peterson said that he had poor luck in getting samples of the actinium series separated for fission measurements because the sample worked up contained an unexpected amount of solid material, largely zirconium and titanium, but now good progress has been made toward preparing thorium and actinium fractions.

I mentioned we would now have some good Np^{237} for measuring fissionability of Np^{237} and Np^{238}. We would also have pure Pa^{231} which Van Winkle will repurify. Protactinium-231 probably fissions but if it does not, we can test Pa^{232}; a similar experiment could be done with 95^{241} and 95^{242}. Ghiorso added that Ra^{228} has now been found not to fission so there are no even-even isotopes seeming to fission except possibly 96^{242}.

Manning said that we should find some way to make Pu^{236} without Pu^{239} and James suggested low energy deuteron bombardment of U^{235}. I added that varying deuteron energies would be a very good thing to do.

I discussed the U^{238} cyclotron target, saying that more money was going into this bombardment than any of the others so it should be worked up with care. Four to five layers should be taken from it in the shop. We should probably work only on the plutonium fractions and get the best ratios of Pu^{241}, Pu^{240}, Pu^{239}, and Pu^{238}. James and Manning suggested also Pu^{237} and Pu^{236}. I said that some layers would be ideal for Pu^{241} studies. Ghiorso asked whether Pu^{241} would be available for fission measurements. James replied that it was impossible to get it free from Pu^{239} but by measuring samples with varying ratios of Pu^{239} to Pu^{241}, it would be possible to get the cross section of Pu^{241}. It is hoped it will be possible to get distribution curves of fission products in these targets. Weissbourd asked whether some samples from this target would have more Pu^{240} than Pu^{239} activity so that it would be easier to resolve the Pu^{240}-Pu^{239} alpha-particle peaks with the pulse analyzer. I replied probably not.

Ghiorso said that a 0.5 mg Th^{232} sample showed only 70 fission counts, 50 with a cadmium shield, so that it would be possible to look for fissionability of Th^{233}. Stan Thompson reported that in the several month old, completely decontaminated 95^{241}-96^{242} sample, after subtracting the radiation of 95^{241}, about the same total amount of electromagnetic radiation from 96^{242} was found. Thus there is a smaller amount of radiation per alpha disintegration of 96^{242} than for 95^{241}. The sample has been showing decay corresponding to a 4.6-month half-life for 96^{242}. The biggest error in the determination is caused by the amount of 95^{241} present. James added that, in his samples, the largest error is also in the 95^{241} content, and 5.1 months appears to be the best half-life for 96^{242}. Cunningham said that no work has been done yet on the few micrograms of recently isolated 95^{241}. The only thing that has happened to the sample is that it has been photographed. Osborne commented that maybe some of it had gotten on the Life photographer's hands. Cunningham corrected him saying that the photographer's hand contamination was due to plutonium. There was a discussion of the Life photographers currently working in the lab (Fritz Goro and his assistant Bob Campbell).

Simpson again brought up the probability of the α,7n reaction. He gave the formula giving the minimum beam energy (in Mev) necessary to get a certain reaction as E = 28.6 [28.2] + (x-4)6.5 where x is the number of particles coming out of the nucleus. Simpson went on to explain that in building up his charts of nuclear energies calculated from radioactive decay energies, he has found that the binding energy of two neutrons is about 12 Mev up to thorium. After thorium it is 13 Mev. Consequently, the α,7n reaction is possible in elements up to thorium but impossible in heavier elements.

Simpson recalled that the Curie-Joliot combination was sometimes humorously referred to as Joli-Curio. Manning suggested that an approximate half-life could be found for the emanation in the U^{230} series. Ghiorso announced that an apparatus for use to measure this is under construction. Manning then asked if it could be found using an oscillograph. Ghiorso replied that it would be easier to measure the range of the alpha particles and then get the half-life from Simpson.

I have agreed to talk to the Physical Chemical Division of the Pittsburgh Section of the ACS on March 26. Jake Warner, now at Carnegie

Institute of Technology, wrote to ask if I would be able to address a joint chemistry-physics seminar in the afternoon before the ACS talk. Today I wrote to Warner to say that I would be happy to talk to the group. I suggest a broad title, such as "The Heavy Elements," and then ask whether he has any preferences for a topic.

I then sent Professor Paul Fugassi, who is arranging the evening meeting, the title "Future Possibilities with Radioactive Tracers." I enclose a summary and a photograph and say that I shall need a blackboard and projection machine. I say I shall arrive at 9:00 a.m. Tuesday and plan to leave the same night.

Farrington Daniels forwarded to me the following ingenious suggestion of Mr. Burke Brown of Guelph, Ontario, for naming elements 95 and 96:

> Thanks to Dr. Thode of McMaster University, I had the pleasure of hearing about the discovery of elements 95 and 96 by Dr. Seaboard [sic] and that they had not been named. Do you think it's fitting to name them <u>seadium</u> and <u>boardium</u> after their discoverer? I do not know if science permits this but I believe Dr. Seaboard's name should be remembered. I hope I have not taken up too much of your time and that I have been of some help.

I replied to Dr. John T. Tate's letter of January 22 and 24, saying that the authors would like the two letters with the earlier dates published: "Radioactive Element 94 from Deuterons on Uranium" by Seaborg, McMillan, Kennedy, and Wahl and "Radioactive Element 94 from Deuterons on Uranium" by Seaborg, Wahl, and Kennedy. The third paper should not be published because it merely presents a little more data. The fourth paper, "Properties of 94-239" by Seaborg, Segrè, Kennedy and Lawrence should have a reference to the previous two papers and perhaps not be published in the same issue. I go on to say that I do not know whether there are any security problems remaining, but we feel they should be published now.

Metallurgical Laboratory Report for January, 1946 (MUC-FDL-173) was issued today. The work of Section C-I was covered in Manning's memo (MUC-GTS-2155) to Hogness dated January 19. The Health Division reports that two humans injected with approximately 95 µg of plutonium on December 27, 1945, excreted 1.8 percent and 1.5 percent of the initial dose in the first 24 hours after injection, distinctly lower than the figure of approximately 2.5 percent obtained on a patient studied earlier. Relatively few urine analyses on Project personnel indicate a body plutonium content of greater than 0.1 µg. The Physics-Metallurgy Division reports the investigation of the action of bromine on neptunium metal which yields a small amount of $NpBr_3$ in addition to the expected $NpBr_4$. A number of new crystal structure results have been obtained, including those for NpF_6. Mass spectroscopy has shown the 87-day yttrium isotope to be located at mass number 88.

The distribution of Metallurgical Laboratory employees as of January 31, 1946 is as follows:

	SED	Academic	Non-Academic	Total
Argonne Laboratory	10	37	10	47
Chemistry	32	88	19	107
Health	8	75	120	195
Physics and Metallurgy	1	16	9	25
Opacity	0	4	1	1
Services and Development	8	23	180	203
Scientific Administration	5	4	3	7
Information	1	14	16	30
Retests	1	4	14	18
Associated Sites, etc. (Manhattan District Advisor, Evergreen)	2	2	2	4
	68	267	374	637
Security and Safety				141
Administration				383
Total				1165

I wrote the formal recommendation to Professor P. Gerald Kruger that he requested for Robert Duffield. After citing Duffield's work on the Plutonium Project and saying that he has earned the reputation of being a thoroughly competent and tenacious research man and director, I go on to state that I believe he would do a good job teaching an undergraduate or graduate course in radiochemistry and is qualified to direct doctorate research. I then say that Duffield is probably about the best man in the country in his age group who is now available for a position such as Kruger wishes to fill.

Wilma and Kristine visited Helen in the afternoon. In the evening Helen and I went to see the movie, "The Great John L."

George Arliss died today at the age of 77. He has been a star on the stage and in the movies for many years and is famous for his portrayal of Disraeli.

Wednesday, February 6, 1946

I received an abstract from Milton Burton for the Nucleonics Symposium at the Atlantic City ACS meeting. He includes the following note:

> So far as I get the story, we are against the usual hay-wire situation in connection with the Nucleonics Symposium. You will note that there is not anything in the enclosed paper not in the Smyth Report or in the prior literature. As a matter of fact, you might also say that there is nothing in the enclosed abstract and let it go at that. However, when I showed it to Coe he almost took a French fit. The explanation seems to be, so far as I understand it, that whether the abstract is satisfactory or not, it is possible that neither you nor I nor anyone will be permitted to present a paper before the Symposium. That is just another one of those incidents that makes me realize the complete asininity of the present situation. What we shall have to do, of course, is to write up our talks, submit them for the Laboratory directors' clearance, hope that they meet the requirements of giving adequate recognition to workers in the Laboratories and at the same time suppressing all patentable information not adequately protected, and then have them passed by the Army. If we can leap all those hurdles, maybe we can speak with the permission of the Laboratory. Of course, I will probably no longer be on the payroll by the middle of April so that if I wish I can violate any rules I care to. However, there are reasons why I would prefer not to do anything like that so that for the moment all that I can do is to restrain my righteous anger and hope that we will not have too much trouble. I would suggest that you try to get the Symposium cleared in principle before submitting the abstract of the papers for the Program. I do not think that an abstract like this requires any clearance in detail.

Robert W. Galvin, Co-Chairman of the Distinguished Service Award Banquet, wrote and again congratulated me on being named Chicago's Outstanding Young Man of 1945." He encloses records and a picture of the occasion.

In other correspondence today, I sent Watson Davis a short summary of my forthcoming talk for the Science Talent Institute on March 2. I ask that he arrange reservations for my trip back to Chicago Saturday evening. I also replied to James M. Crowe's letter of January 22 about the proposed section in C & E News to be known as

"Nucleonics and Atomic Energy Forum." I say I have found a strong reluctance to accept the term "nucleonics" and suggest perhaps the section should be simply "Atomic Energy Forum."

I then wrote to Captain Lavender in reply to his letter of January 19, to tell him that the inventors believe that the next step required is to submit the agreement with the attached disclaimer to the University of California for signature by the authorized official on behalf of the Regents. I say that it will be sufficient at the present time for copies of the applications to be available at the Patent Group at Berkeley. However, Kennedy and Wahl, now at Washington University, must eventually have individual copies.

I received the following memo requesting support for the McMahon Bill:

> The attempt to rush through the Congress an Army sponsored atomic energy bill was defeated last year largely through the efforts of aroused scientists. Heeding the requests of the scientists, the Senate appointed a special committee for the study of all aspects of atomic energy.
>
> After listening for two months to the testimony of scientists, engineers, and military experts, the Chairman of this committee, Senator Brien McMahon, has introduced a bill, S. 1717, which deserves the full support of all citizens. The reasons for this support are set forth in the testimony presented to the Senate Committee on January 28 by Harrison Davies of Clinton Laboratories, who spoke on behalf of all the associations belonging to the Federation of Atomic Scientists.
>
> The following are the main points of his testimony:
>
> (1) The McMahon Bill facilitates the creation of international controls of atomic energy by treating specifically the production of atomic bombs and fixing the responsibility for this production in such a way that international regulation must take precedence over all previous provisions.
>
> (2) A Commission responsible to the President and to the Congress controls the production and distribution of fissionable materials; it has full powers of inspection and can require reports on all work in the field.
>
> (3) Creation of private monopolies is prevented; premature industrial developments which may create vested interests and complicate the international inspection problem are not allowed. On the other hand, industrial research and developments are encouraged and the Commission is instructed to prepare the ground for future industrial applications.

(4) Research and development in the field of atomic energy are fostered by provisions for liberal and equitable distribution of fissionable materials and by-products and granting of subsidies to independent research workers, supplemented by the creation of a division of governmental research.

(5) The policy is established of full freedom of publication of basic scientific information. The Commission is instructed to assist in the dissemination of this information. The same policy is to apply to related technical information, barring exceptions for which Presidential assent is needed. No special security regulations are provided, but reliance is put on the Espionage Act, which has provided sufficient safeguards during the war.

(6) Provisions for reporting by the Commission to the Congress are such as to ensure adequate public scrutiny of its work. Not only past results but also plans for future work are to be reported regularly, and studies of social and political implications of atomic power are to be made to serve as basis for further legislation in the field.

(7) The administrative structure proposed seems adequate. It provides for a five-man, full-time civilian policy-making commission and several administrative divisions for production, allocation, research, and military application.

(8) Appeals from the decisions of the division directors to the Commission provide real safeguards against arbitrary decisions, because of the separation of the policy making authority of the commission from the day-to-day administrative functions of the division directors.

The McMahon Bill will encounter powerful opposition. Attempt will be made to prevent its passage by endless debate and to revive the original May-Johnson Bill, now pigeonholed in the House. The only hope for McMahon Bill to become law without being first emasculated in all its liberal provisions lies in a powerful support by enlightened public opinion. At present, the public interest in atomic power legislation is almost nonexistent. Except for persistent efforts of Washington representatives of the Atomic Scientists, the Senators, and Congressmen hear no voices showing that the people at large appreciate the urgency of the problem and support the McMahon Bill.

Time has come for all members of the Atomic Scientists, for all scientists, for all enlightened citizens, to act.

K. A. Kraus' report, "Studies on Polymeric Pu(IV); Depolymerization of Polymeric Pu(IV) Solutions in Nitric Acid," (CN-3399) was issued today. The experimental work for this report was carried out in the fall of 1944 in the Basic Chemistry Group of Section C-I of the Met Lab.

Today Helen attended her pre-natal class at Lying-In Hospital.

This morning's paper reports a new purge by the Polish secret police. This dragnet may have swept 75,000 to 100,000 persons into jails and prisons, with thousands held for six to eight months without any charges being brought against them.

Thursday, February 7, 1946

This morning I attended the meeting of the Solvent Extraction Group. Others present were Blaedel, Cavataio, Friedman, Goeckermann, Hyman, Leader, Manning, Murray, Nachtman, Schaffner, and Sheft. Blaedel reported on run 34U in which 0.8 M $Al(NO_3)_3$ was used in the IAS solution and 0.9 M $Al(NO_3)_3$ was used in the IBX solution instead of 8 M NH_4NO_3. The U(VI) concentration gradient in column IB appeared just as great as in the case of NH_4NO_3; however, the uranium losses in the IAW solution were about 10 percent compared with 0.1-0.3 percent when NH_4NO_3 was used. The work fairly well eliminates the use of $Al(NO_3)_3$ in column IA but indicates certain advantages in column IB.

Run 36U21P was begun with the hexone phase continuous above the feed point in column IB. The alpha-particle losses in IBU were 4½ percent and still rising at five hours when the interface was raised to the top of the column. The alpha-particle losses in IBU dropped to 0.2-0.5 percent and remained there even though various concentrations of glacial acetic acid (0.03, 0.3, and 1.0 M) were added to the IBS solution. Blaedel continued by saying that run 37U22P12F was made with 0.9 M $Al(NO_3)_3$ in the IBX solution instead of 8 M NH_4NO_3. The alpha-particle losses were 0.7 percent, slightly higher than those with 8 M NH_4NO_3. The gamma-ray decontamination factors for the AIP, IBP, and ICU solutions were 500, 3000, and 4000. Beta-particle decontamination factors were about the same. The reason for the poor gamma-ray decontamination factors has not yet been ascertained.

Sheft noted that the main gamma-ray emitter in the 2BP solution is ruthenium. This follows plutonium through the process because of its similarity to plutonium in having a hexone extractable higher valence state and an aqueous extractable lower valence state. Ruthenium follows plutonium in column 2A because the strong oxidizing agent used to form Pu(VI) for hexone extraction also forms oxidized ruthenium. If the salting out power of the aqueous phase in the 2A column is increased, e.g., by the use of 1 M $Al(NO_3)_3$ in addition to the 8 M NH_4NO_3 already present it should be possible to operate column 2A with Pu(IV). A weaker oxidizing agent could then be used and it should be possible then to keep ruthenium in the lower valence states. H_2O_2 is such a reagent, forming Pu(IV) and lower valent ruthenium.

I asked whether the possibility of oxidizing Pu(III) to Pu(IV) with HNO_3 alone has been tested, and Sheft replied, "Yes, but without success. The alpha-particle distribution coefficient indicates that oxidation to IV is incomplete."

Hyman said that there is still much to be learned about the chemical mechanisms involved in operation of column IB. The fact that the column can be made to work practically was demonstrated long before we were able to establish even our present theories about what is going on. Work of the past two or three weeks dealing with the complex chemistry involved here seems to be leading in the direction we must follow to improve practical operation. Some basic facts which determine the behavior of plutonium in column IB are the following: Trivalent plutonium is essentially completely insoluble in hexone. In the absence of any complication, ferrous ion is capable of reducing Pu(IV) to Pu(III) in what seems to be a completely reversible fashion, i.e., $Fe^{+2} + Pu^{+4} \rightleftarrows Fe^{+3} + Pu^{+3}$. Therefore to make possible a successful column using ferrous as the reductant it is necessary to maintain a low ratio of ferric to ferrous. We have found that in hexone, plutonium is strongly held in a complex of some sort which we cannot completely describe at present. So far we know it involves plutonium and hydrazine (or some resulting product of the solution of hydrazine in hexone or other material acting like hydrazine) and requires nitric acid for its formation. At present we cannot say whether it is the acid or the nitrate that is important.

The complex is in equilibrium with ordinary Pu(IV) in the hexone so as to permit only a small amount of free Pu(IV) to exist. Therefore it is possible to maintain the proper overall Pu(IV) ratio between hexone and aqueous with a very low concentration of normal Pu(IV) in each phase.

This phenomenon is not the only complication. In ordinary solvent extraction, we assume that after an interface is established there is a certain probability of a simple ion (perhaps solvated) ending up in either the organic or the aqueous phase. For the Pu(IV) complex just described, this is not true. Whether it is due to steric factors (i.e., the plutonium cannot reach the interface as often) or to an activation energy of dissociation of the complex into an extractable form, the rate of extraction of reduced plutonium from the hexone phase is only about 0.1 the rate of extraction of a base line element such as strontium in the same experiment.

To better understand and improve operation of column IB, we can (1) improve the contact between the two phases without lengthening the hold up of the aqueous phase in the column. Milton Ader has been working on this and has succeeded in finding some wetting agents which decrease the size of hexone bubbles rising in the aqueous phase and presumably this would improve contact of the two phases. (2) We can add certain material, e.g., we have tried organic hexone-soluble acids which will so complex or combine with the hydrazine as to destroy its attraction for plutonium but not its effectiveness as a holding reductant. (3) We can look for substitutes for hydrazine, as Abe Friedman has successfully done.

Friedman reported on the following reducing agents studied in an attempt to find a more suitable reagent than hydrazine: s-diphenyl carbazide, semicarbazide hydrochloride, 1-phenyl semicarbazide, phenyl hydrazine, phenyl urea, urethane, K phthalimide.

In comparing distribution ratios of plutonium between IBP and IBS, The s-diphenyl carbazide and the semicarbazide HCl gave exceptionally good results as compared with phenyl hydrazine and the regular hydrazine. Thus:

		aq/hex	
IBS	5 m.	1 hr.	2 hrs.
hydrazine	27	32	20
s-diphenyl carbazide	660	530	350
semicarbazide HCl	630	170	100
phenyl hydrazine	31	57	64

The semicarbazide HCl and s-diphenyl carbazide were studied individually and more intensively.

In further studies on s-diphenyl carbazide, very encouraging results were obtained. The distribution ratios of plutonium between the IBX and IBFP solutions, using s-diphenyl carbazide as the reducing agent, were infinite after five minutes shaking and 11,000 after an hour. On removing the hexone phase and adding a new active IBFP solution to the same IBX solution, and again determining the distribution ratios, and repeating this whole prodecure for three or more times, the resultant distribution ratios, aq/hex, were:

(2) 5 m - 3800
1 hr - 880

(3) 5 m - 840
1 hr - 5400

(4) 5 m - 5200
1 hr - 9600

The distribution ratios obtained here for hydrazine are lower than usual and it is likely that the difference in behavior of s-diphenyl carbazide and hydrazine towards uranium is not so great as the graph indicates.

The advantages of s-diphenyl carbazide over hydrazine as reductant are: (1) its exceptionally higher distribution ratios for plutonium in favor of the aqueous phase, both in the extractor and scrubber sections of the column; (2) its somewhat higher distribution ratio of uranium in favor of the hexone in the scrubber section of the column; (3) it is no less stable than hydrazine. Its disadvantages are: (1) its comparative expense ($7.00/100 g; (2) its deep color in hexone, making control titration somewhat difficult.

Leader commented that semicarbazide would probably be cheaper, and might not need to be recovered, but Hyman said that its solubility in hexone is a serious disadvantage.

Manning asked the cost of hydrazine per ton of uranium estimated for the present Redox Process, and Hyman said $250 per ton assuming no recovery.

Goeckermann reported that it has been found possible to separate an old ICW solution into a hexone fraction containing less than 0.01 N reducing power and a yellow oil with 11.6 N reducing power as determined by titration with dichromate. It is suspected this might be the methyl isobutyl ketazine, but the oil began to decompose at 200°C without boiling. No crystals formed when the oil was cooled to -78°C, but the viscosity increased. The oil has an equivalent weight of reducing power (titration with dichromate) of 71 g. When a IBS solution made by dissolving the oil in hexone was shaken with regular IAP and IBX solutions in the usual feed plate experiment, the distribution ratios were about one-half normal values over three hours.

The effect of p-chloro-phenoxyacetic acid and acetic acid on the transfer rate of plutonium was measured in the non-equilibrium type of experiments described previously by Hyman.

It has been found possible to prepare an IBS solution by shaking hexone with hydrazine hydrate solution alone for 45 minutes. The resulting solution is 0.5 N in reducing power (dichromate titration) and contains ~0.05 N free base (methyl orange end point). When this solution was shaken with an old IBP solution and the indicated amounts of acid added to the IBS solution, the following values of the distribution ratios were obtained at 15 minutes.

	0.00 H HNO_3	0.1 N HNO_3	1.0 N HNO_3	0.3 N CH_3COOH
D.R.	310	3.7	4.7	800

The extreme sensitivity to HNO_3 concentration at low levels indicates the importance of the as yet unknown part it plays in complex formation.

Helen visited Wilma today.

The 509th composite group of the U.S. Army air forces, which dropped the bombs on Hiroshima and Nagasaki, will repeat their performance on target warships at Bikini atoll next spring and summer.

Friday, February 8, 1946

Larned B. Asprey is being discharged from the SED today and will go on civilian status at $325 per month for a few weeks.

I mailed James T. Grady the text entitled, "Some Chemical Aspects of the Plutonium Project," which I am scheduled to present to the Chicago Section on February 15. I note that the Security Office did not allow any significant new material to be added. I then say I will cover material about future applications of atomic energy in the discussion period. Additionally, I tell Grady that I will speak on "Atomic Power" at the Peoria Section of the American Chemical Society on February 21.

V. Karapetoff sent me a note, which I received today, thanking me for my letter of January 24 about the 5f shell.

Today Helen took care of Carol and Lorraine Osborne while Marjorie took Kathleen, who had been injured, to the doctor. Later Helen went to see the play, "Anna Lucasta," at the Civic Theater on Wacker Drive at Washington Street.

An item, datelined February 8, in today's paper states that President Truman will not alter the sentence of General Yamashita.

Saturday, February 9, 1946

Charles Coryell sent me an abstract, "Radiochemistry and the Fission Products," for his talk at the ACS meeting in Atlantic City. Coryell writes, "This abstracts permits a completely Smythian talk, but I am hoping very much that I can get further clearance in general categories at least." Coryell goes on to say, "I appreciate very much the invitation to speak on this symposium, and I know that the security and Project complications have already caused you a great deal of work. Please keep Burton and me posted about developments."

On January 28, I received a thank-you letter from Charles K. Hunt, Program Chairman of the Detroit ACS, for my talk there and asking that I again speak to them sometime during the 1946-47 season. I replied suggesting that possibly I could speak during the spring of 1947 when more information has been cleared.

An additional letter which arrived while I was away from the laboratory is from W. Albert Noyes, Editor of Chemical Reviews, asking that I write an article on the new elements. In my reply today I say that I am willing to write such an article, but it should not be planned for earlier than the spring of 1947 because we are occupied with writing the Plutonium Project Record, and I am certain all of the material will have to appear in the official record first.

I also replied to a letter dated February 3 from Edgar J. Murphy, who is assisting in editing articles on nucleonics for C & E News. He asked for an article on the transuranium elements, and today I agreed to prepare such an article.

I received and read a copy of a letter dated February 6 from Iz Perlman to Norman Hilberry. Iz comments on the reports of abnormally high extraction step waste losses in processing of high gt material at Hanford and suggests that a possible cause of the purported excess "plutonium loss" is actually due to the presence of appreciable amounts of the isotopes 95^{241} and 96^{242}. He goes on to say that he does not know whether HEW has considered this explanation and rejected it. Perlman believes that a search for 95 and 96 should be made since almost all of the other evidence points toward some other elements than plutonium as the source of the extra "losses."

Holland has offered Indonesia commonwealth status with a promise of independence "in our time."

Sunday, February 10, 1946

Helen and I visited the Chicago Museum of Science and Industry today.

This morning's Chicago Sun says that Premier Stalin has announced a new five-year plan for the Soviet Union with large production boosts and gains in science.

Monday, February 11, 1946

William C. Bentley, who has recently been discharged from the Navy where he worked as an electronic technician, began working in Section C-I with Art Jaffey.

I called Joe Hamilton to discuss the arrangements for the bombardment of 95^{241} with helium ions. We also talked about the status of the depleted uranium bombardment. Hamilton emphasized the need to have draftsmen help with the plans for the preparation of target assemblies and suggested that Perlman include such people in our group at the Radiation Laboratory.

In the mail today I received a note from Jake Warner, who is pleased that I am willing to address a chemistry-physics seminar at

Carnegie Tech on the afternoon of March 26. Henry Eyring wrote to inquire how the symposium on nucleonics is progressing, and Cecil T. Langford reminded me of my speaking engagement in Peoria on the evening of February 21.

While I was in California, a letter arrived from George Watt requesting suggestions for a replacement for C. S. Garner, who has accepted a position at UCLA. Watt also asked if it would be possible to obtain blueprints of the Westrum "dry box" and a copy of the Counting Manual. I replied today, saying that I doubt that the University of Texas can find a man of the caliber it wants within the $3,000 to $3,600 bracket. All of the men I know have accepted or have offers above this level. I also say that it is presently impossible to send him the blueprints or the Counting Manual although the manual is only marked "restricted." I suggest he ask Mulliken for a copy and see what happens.

I wrote to Professor A. W. Bellamy suggesting that, if UCLA plans research in the fields of life sciences using by-products of atomic energy, I can suggest one man educated both in zoology and nuclear science, who might be available for such a position--Leonard I. Katzin. Katzin, I say, like myself is a UCLA graduate and one of the best men I know with such a combined training.

John T. Tate wrote another letter which I received today and promptly answered. I say he is correct that the three papers he mentioned were not done under government contract and therefore it is appropriate to proceed with the publication of the first two. The third we would like to hold for a while. I say the safest procedure for the fourth paper ("Properties of 94-239") is to have the authors secure the official release.

I also took time to write the following note to Samuel Grafton of the New York Post:

> A petition endorsing immediate world government has been sent to me under the stationery of the Writers' Board (formerly Writers' War Board), and under the signature of Rex Stout. Your name appears on the letterhead, probably as a member of the Executive Committee of that organization.
>
> In your newspaper column you have frequently criticized the proponents of immediate world government as advocating a problem remote from the real problems of international politics. If this petition does not represent your views, I should be glad to know it, and if it does, I should be glad to know why you have changed your mind.

Lieutenant General Masaharu Homma was convicted of ordering the Bataan death march and other wartime atrocities and ordered to be shot, according to this morning's newspaper.

Tuesday, February 12, 1946

I attended the meeting of Groups 1 and 3 at 8:28 a.m. Others present were Ames, Anderson, Britain, Cunningham, Fineman, Florin, Ghiorso, Hagemann, Hindman, Hopkins, Hyde, Jaffey, James, Kohman, Manning, Osborne, Peterson, Scott, Simpson, Stewart, Studier, Templeton, R. Thompson, S. Thompson, Van Winkle, and Westrum. During the general conversation preceding the meeting Osborne remarked that there were apparently about six micrograms of Pu^{238} in his sample of bombarded Np^{237}. This was about what he had expected. There was also some discussion of the half-life of Pu^{238}. James' best value is now 115 years. Jaffey's recent guess was 100 years. The very first guess at the half-life prior to the 50-year estimate was 200 years, a value which must be weighed heavily in accordance with the Ghiorso principle (the first very rough determination is the best).

I formally opened the meeting with a discussion of the writing program, which is progressing but not spectacularly. I asked particularly about papers on fissionability of the various isotopes. None of these seemed to be in process. A paper on the n,γ cross section of Pa^{233} was assigned to be written by Hagemann, Ghiorso, and Katzin.

Cunningham reported on his recent studies of the absorption spectrum of element 95. There is a prominent sharp peak at 503 millimicrons which should prove useful in following any change in oxidation state. There seemed to be no very obvious similarities between the spectrum of element 95 and those of uranium, neptunium, and plutonium. The sample studied contained 2 percent lanthanum, by spectrographic analysis. The solution was pink in color. He then announced plans to attempt oxidation with argentic ion and then bromate ion.

It was announced that a barrel has arrived from Los Alamos containing 50 gallons of the aqueous-phase from the first ether extraction performed upon the plutonium after arrival from Hanford. Stan Thompson said that it was too early to comment on the content of 95^{241} in this solution. [Advice subsequent to the meeting places the 95 content at approximately 15 mg!!] The probable process to be employed in separating element 95 will include first hydroxide precipitation with ammonia, followed by two oxidation cycles (probably bromate) to separate the plutonium and three or four fluosilicate cycles.

I remarked that Werner at Berkeley has been working on a method of separating lanthanum and 95 employing TTA. No details are available but the method seems to show great promise.

Studier reported his best value for the half-life of what is thought to be Np^{234}--the value is 4.44 ± 0.05 days. The decay of the x-rays in the neptunium fraction from the helium ion bombardment of U^{233} seems to be tailing over into a longer half-life than 4.4 days.

It may be Np^{235}. Van Winkle remarked that he also has data on Np^{234} giving a half-life of 4.3 days. His observations are less extensive than those of Studier but are thought to constitute a very good check. Studier thought that the ratio of K x-rays to 2 Mev gamma-rays in the neptunium fraction (3 to 1 ratio) was too high and may indicate the presence of some soft gamma components. This remark provoked a rather lengthy discussion of counting efficiencies of gamma-rays of various energies. It was concluded that Studier and James have a "feeling" for these efficiencies but, as Osborne remarked, it is sometimes rather hard to assess this "feeling." If Studier and James can be prevailed upon to place their "feeling" in writing, this "feeling" will be appended to these notes. Jaffey pointed out Steinberg was also collecting and correlating such data. It was also noted that except in a few rare cases such as Pa^{231}, knowledge of the fine structure of alpha particles and the resulting gamma-rays is not too helpful in assessing counting efficiencies because of uncertainty as to the extent of internal conversion.

Peterson mentioned the thesis of D. E. Hull at Berkeley in which are plotted complicated decay and growth curves which have been calculated from the published half-lives of the various natural radioactive elements. It may serve as a valuable reference. Simpson also called attention to a recent paper by Bradt, Gugelot, Huber, and Scherrer (Helv. Phys. Acta, vol. 18, no. 5) on the ratio of orbital electron capture to positron emission which includes data on counting efficiencies.

Simpson remarked that the longer half-life now in vogue for Pu^{238} is not compatible with a zero spin change. He predicts a half-life for alpha emission for Pu^{237} of between 100 and 1,000 years. Jaffey's decay curve on Pu^{238} started with what appeared to be a 66-year half-life which increased to about 100 years half-life, but the entire decay amounted to only about 10 counts per minute. Florin then suggested a specific activity determination using the six micrograms of Pu^{238} to be obtained by Osborne. The 40-day orbital electron capture plutonium isotope now thought to be Pu^{237} has no gamma-rays harder than K x-rays. This is in agreement with Simpson's predictions. There was some discussion of the limits which might be set for the half-life of Pu^{235}. Studier thought that its half-life must be less than one day or it would have been observed in previous bombardments.

Van Winkle reported on the purification of our stock of protactinium. He stated that the Agruss material has been largely separated from zirconium and if all goes well the protactinium might be purified within a week. He has also worked up the mg of protactinium that was bombarded at Clinton and obtained about 10^7 c/m of U^{232}. The log of the bombardment has not yet been received. This bombarded protactinium should now be very pure with respect to uranium and should be sent back for some U^{232} production. Ghiorso remarked he would like to have enough of it for fissionability tests. I suggested that he separate a barium fraction to determine the fission cross section.

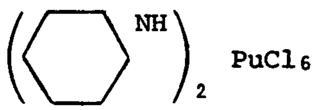
The advisability of bombarding a mg of protactinium at Hanford was discussed. Although some U^{233} would also be produced under these conditions this would not interfere in many uses of the material such as spontaneous fission measurements. Segrè has requested an amount of U^{232} not to exceed 10^9 c/m. Hamilton has requested a mg of protactinium; this will be sent soon. It was remarked that about 6×10^6 c/m of 96^{242} are being formed at Clinton by irradiation of one microgram of 95^{241}.

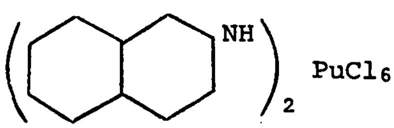
Fineman and Kohman talked about their work on the half-life of Em^{221}, product of Ra^{225} alpha branching decay. A half-life of 3 ± 1 hours has now been observed on three decay curves. This would represent 0.02 percent alpha branching of Ra^{225} and a corresponding alpha half-life of 150 years. These are very preliminary observations.

Ames discussed his recent progress in the Ra^{226} half-life determination by weighing experiments. After 33 recrystallizations, he has obtained radium containing less than 0.04 percent barium. He has 8 mg of this very pure material obtained from an original 15 mg and is now ready to start the actual specific activity determination.

Kohman reported briefly on the activities of Sedlet, who looked for protactinium in the St. Louis fraction; 50 to 100 percent of the protactinium seems to be in the first sulfate precipitate from the ore solution. This is not too encouraging since the weight of this precipitate is from 1/10 to 1/4 that of the original ore. Further processing of this precipitate includes barrelling and storage. Sedlet is looking for ionium in these fractions. Kohman said it appears that samples may be forthcoming from St. Louis around May 1.

Anderson then talked about his recent activities. Plutonium trifluoride is the only plutonium precipitate which forms in a fluoride solution in the presence of either potassium or rubidium. No double salts are formed. He reported on a number of salts of the hypothetical H_2PuCl_6:





$((CH_3)_4N)_2\ PuCl_6$

The first two have a solubility of around 5 g/l. The third is more soluble and was separated from alcohol-water mixtures; zinc and potassium do not form double salts of this character. A precipitate has been observed with cesium. The exact formula, however, has not yet been determined.

Peterson remarked that he has been continuing work on the preparation of Ra^{223}, Th^{227}, and Ac^{227} samples for Ghiorso.

Hagemann announced plans for fixing the half-life of Th^{229} more accurately using the same method as previously employed but all on one sample.

Britain reported that the recovery of plutonium from CW-3 has progressed to the stage of evaporating the solution to dryness. The beaker is hot--largely beta activity of about 200 r per hour.

Florin announced plans for initiating a study of the vapor pressure of NpF_6. Later he will attempt preparation of protactinium fluorides and volatilization of protactinium from uranium ore residues.

I called attention to the fact that a second consignment of laboratory equipment, etc., is being gathered up for shipment to Berkeley. I urged that anyone wishing to suggest items for inclusion get in touch with Cunningham immediately.

A. V. Grosse called to tell me that President Bradley Dewey of the ACS is forming a Beilstein-Gmelin Committee to plan the resumption of publication of these important volumes. Chairman of the Committee will be Hass of Purdue, and membership will include Bogert of Columbia, Fieser of Harvard, Whitmore of Pennsylvania State, Marvel of Illinois, Huntress of MIT, du Vigneaud of Cornell, and Gilman of Iowa State. Grosse said that he will serve as secretary to the Committee and they would like to have me serve on the Committee. I accepted the invitation. He also indicated that Pauling of Cal Tech will be invited to be a member. He will send me, in a few days, a copy of the material that he has prepared for Dewey for his ACS presidential speech, in which he refers to the work on element 94. He said he is looking forward to seeing me in New York on February 26 at the time of the dinner meeting of the American Institute of Chemical Engineers.

Winifred T. Koziolek began working as a technician with Hyman today.

I read a copy of a letter from Earl Hyde to Tom Morgan in Berkeley, giving the details of the procedure Hyde and Studier use to separate pure plutonium and neptunium fractions from U^{233} bombardments.

Mr. Fritz Goro of Life magazine has been taking pictures in our laboratory (Section C-I) in connection with our wartime work on plutonium here. Today I wrote to Miss Mary Alves of Life requesting a complete album of these photographs to be used for educational purposes. See Figures 23-44.

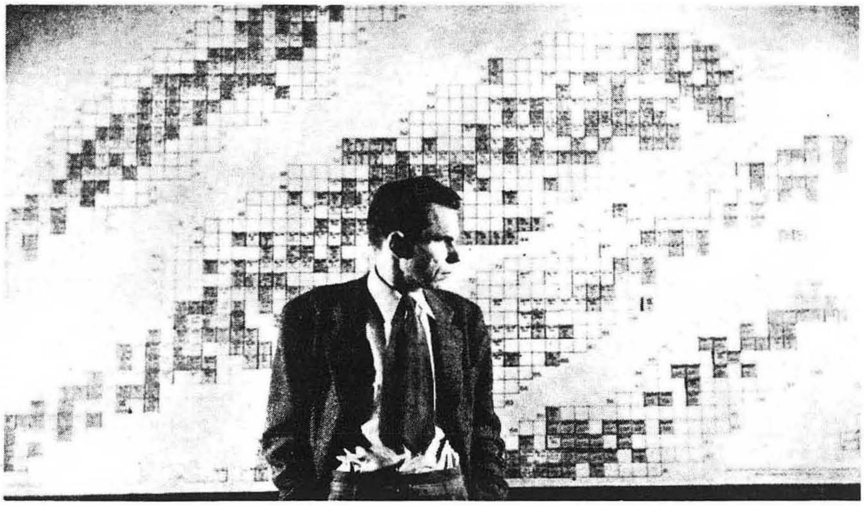


Figure 23. Glenn T. Seaborg in office, New Chem, February 1946.

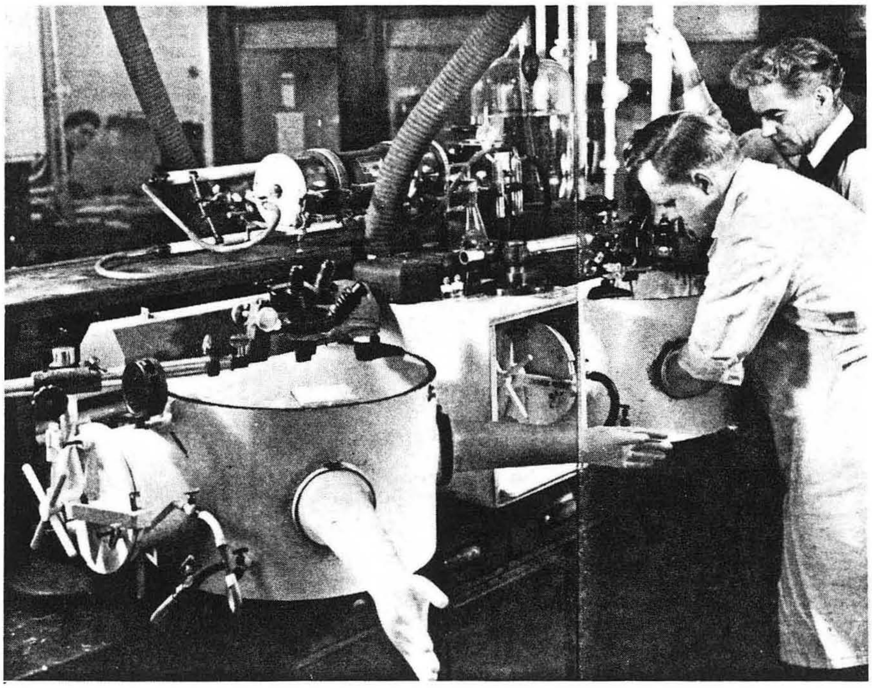


Figure 24. Edgar F. Westrum, Jr. working in dry box with T. R. Hogness on right. New Chem, February 1946.

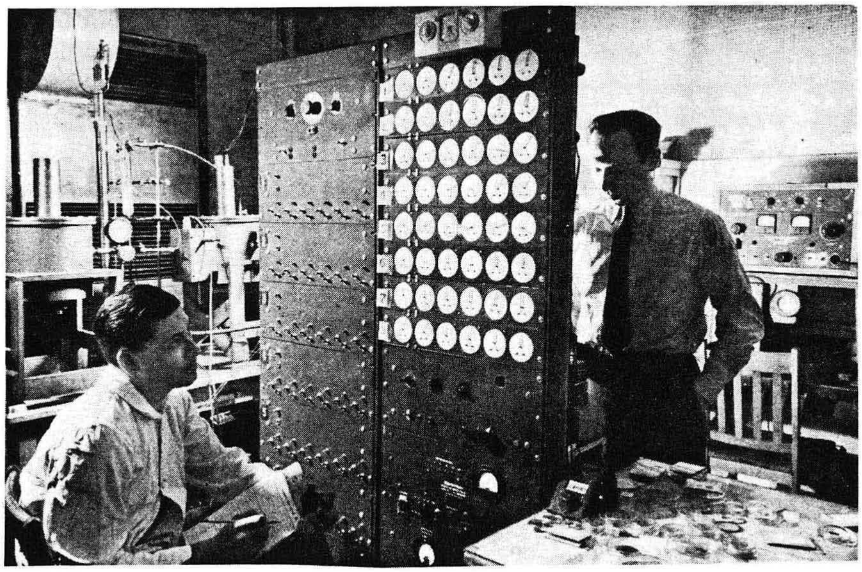


Figure 25. Al Ghiorso in front of pulse analyzer with Art Jaffey on right. New Chem, February 1946.

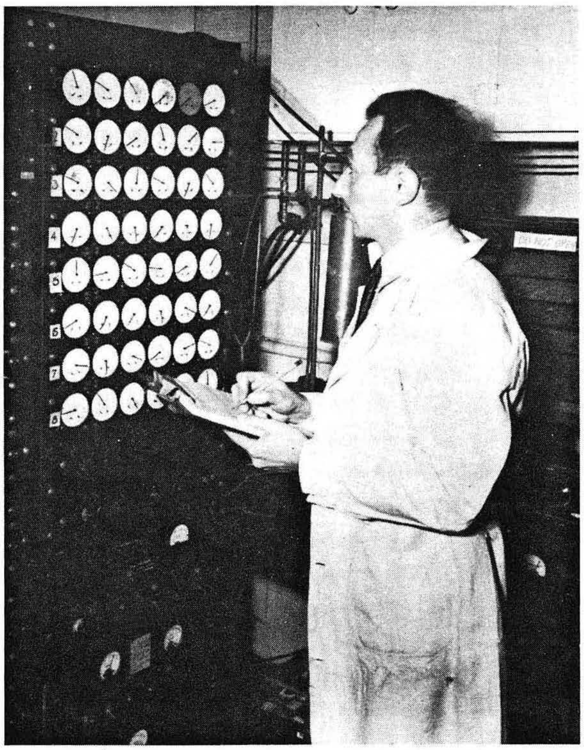


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Figure 26. Art Jaffey at pulse analyzer. New Chem, February 1946.

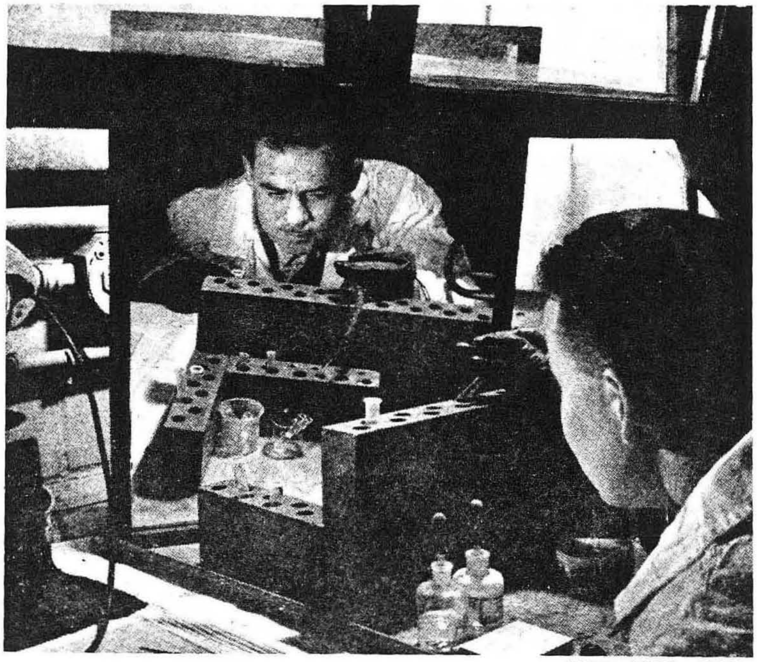

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Figure 27. Stan Thompson, watching work in mirror, while protected by lead bricks. New Chem, February 1946.

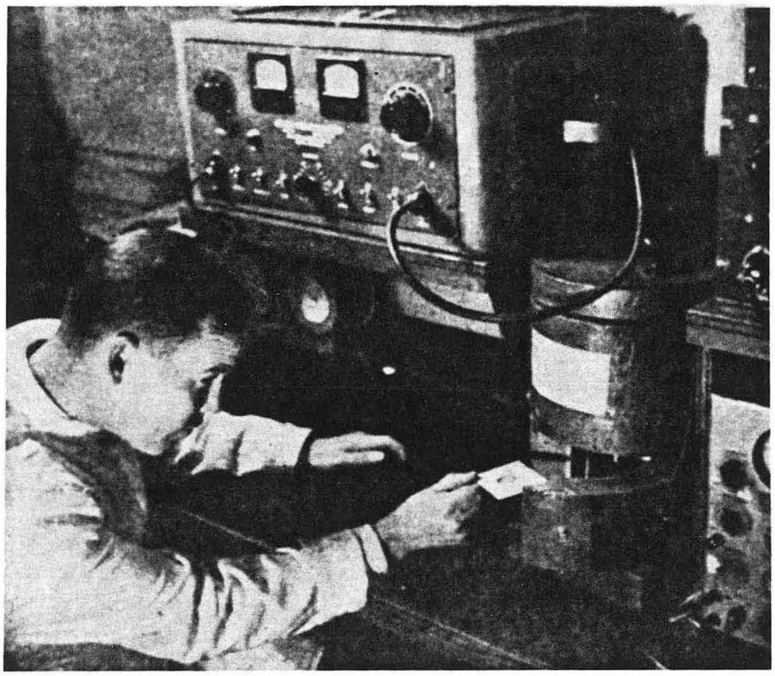

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Figure 28. Stan Thompson, counting sample in Geiger counter. New Chem, February 1946.

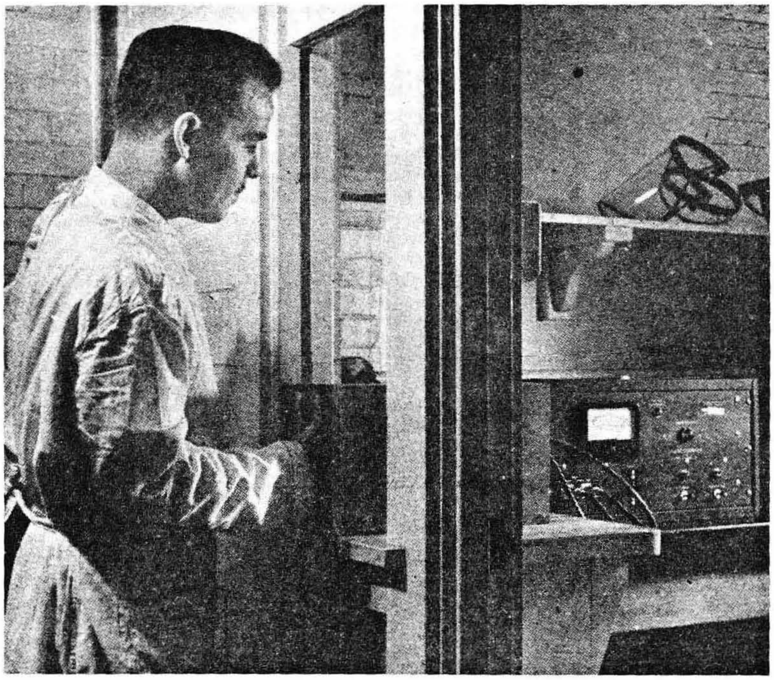

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Figure 29. Stan Thompson checking hands. New Chem, February 1946.

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Figure 30. Stan Thompson wearing protective clothing while using centrifuge. New Chem, February 1946.

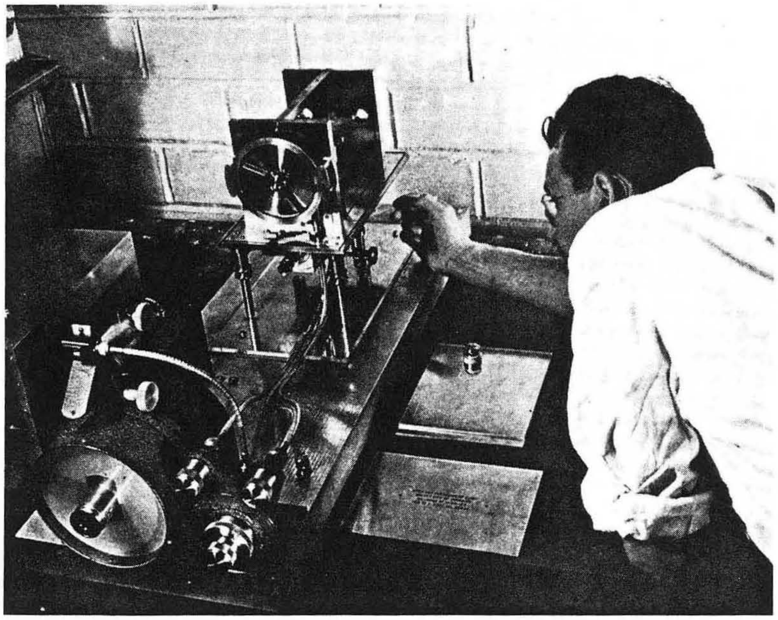

Figure 31. Burris Cunningham with torsion fiber balance. New Chem, February 1946.

Figure 32. Burris Cunningham loading microcone. New Chem, February 1946.

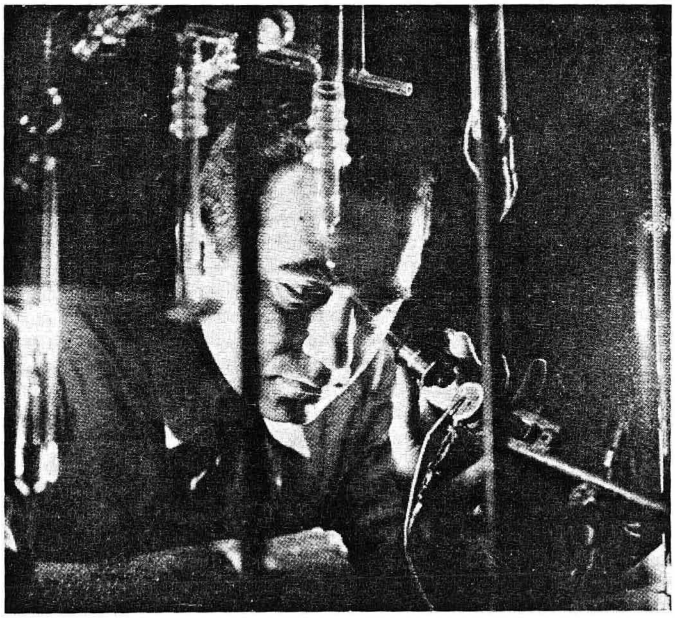

Figure 33. Sherman Fried, crystallizing compound for analysis. New Chem, February 1946.

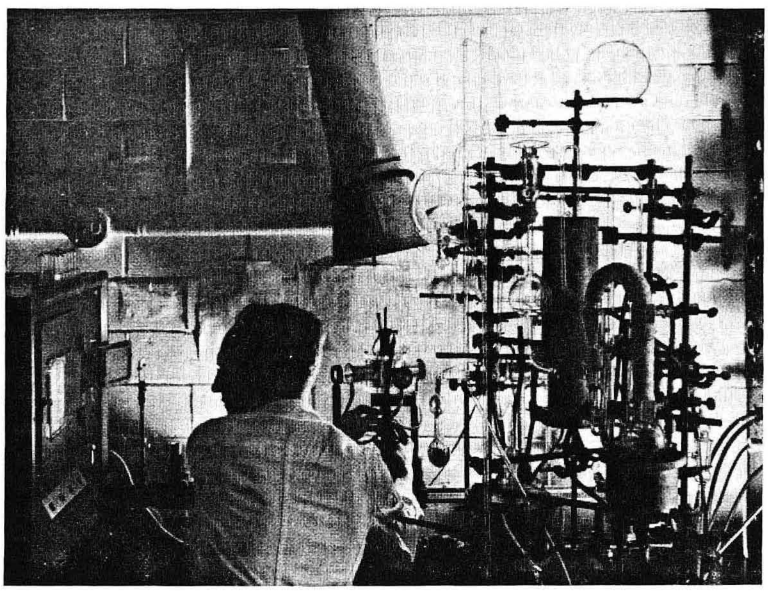

Figure 34. Frank Tomkins analyzing sample in spectrograph. New Chem, February 1946.

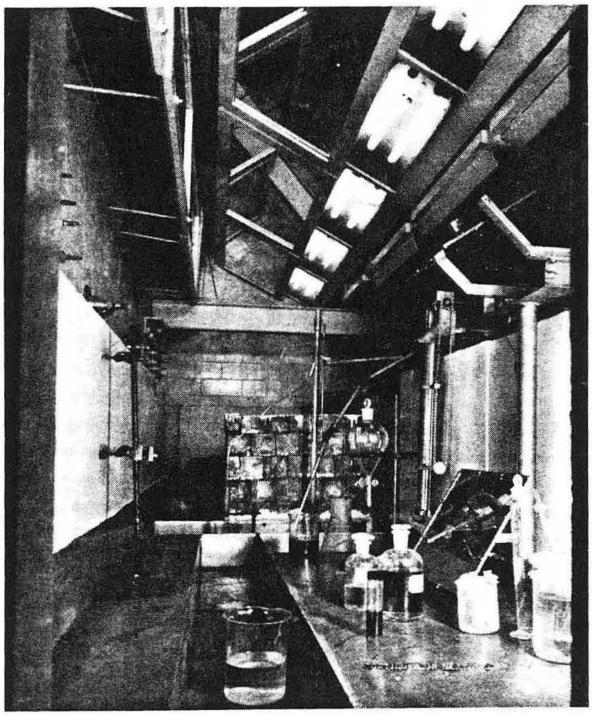

Figure 35. Inside cave, New Chem, February 1946.

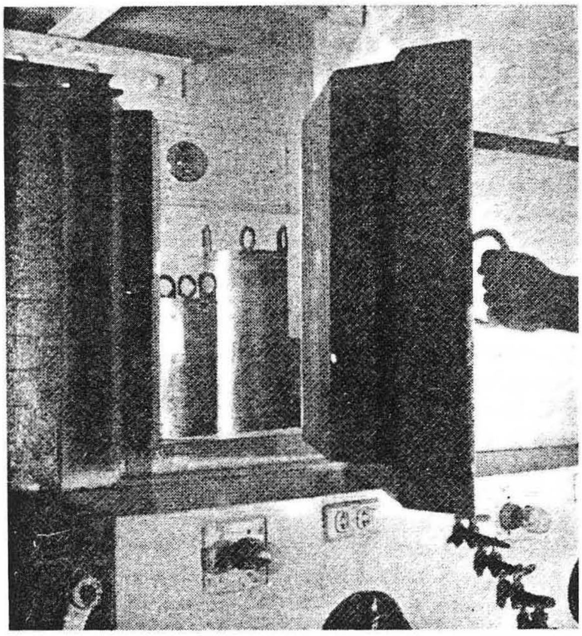

Figure 36. Lead door to cave opened to show cans of radioactive samples. New Chem, February 1946.

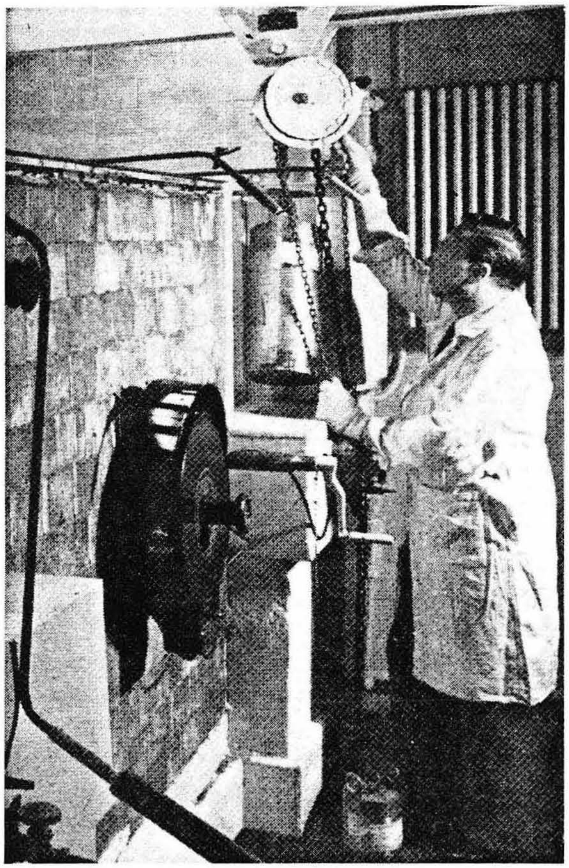

Figure 37. French Hagemann lifting lead can through door to cave. New Chem, February 1946.

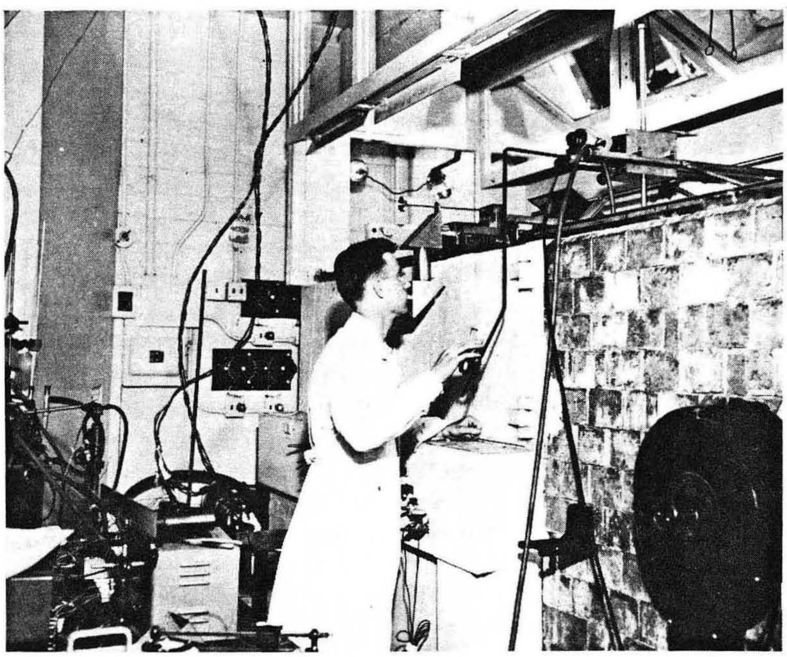

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Figure 38. Earl Hyde operating remote controls. New Chem, February 1946.

Figure 39. L. B. Magnusson,
New Chem, February 1946.

Figure 40. J. W. Britain taking hand
count, New Chem, February 1946.

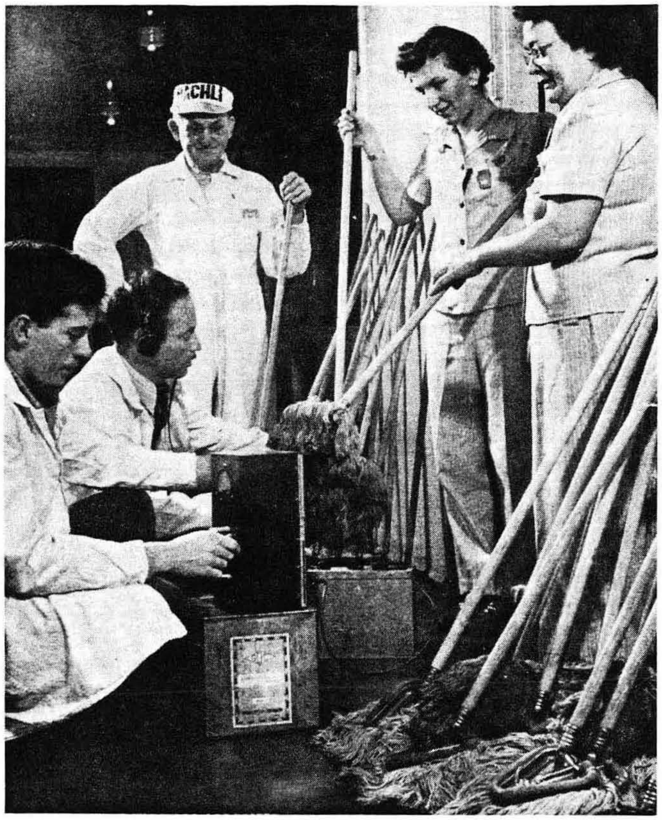

Figure 41. Checking mops of dieners for radioactivity. New Chem, February 1946.

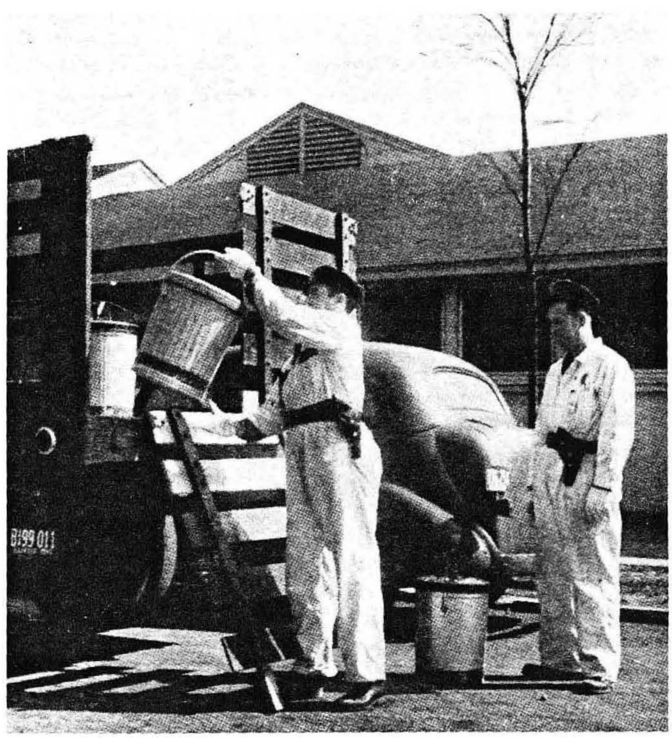

Figure 42. Waste disposal detail. New Chem, February 1946.

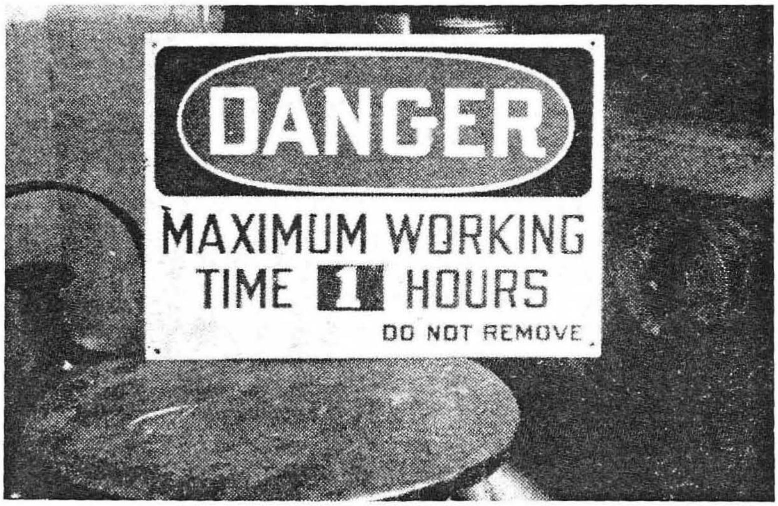

Figure 43. Danger Sign on laboratory bench. New Chem, February 1946.

Figure 44. Protective footwear. New Chem, February 1946.

Other letters today went to Robert Galvin, co-chairman of the Banquet, to thank him for the transportation and pictures of the Distinguished Service Award; to Sidney D. Kirkpatrick, Editor of Chemical and Metallurgical Engineering, inquiring about the appropriate dress for the reception of February 26; to Donald A. McPherson of John Wiley to say I shall be able to see him here in Chicago on February 20; and to J. P. Donnelly of the Hotel Shelburne in Atlantic City, requesting that he extend my reservation to include the night of April 10. Finally, I wrote to Paul Kirk to reassure him that the patents for metal production are being handled properly.

Today Helen shopped in the Loop. During the evening I met with Mr. Eisendrath to discuss buying some stocks.

According to the paper today, the United Nations Organization site committee voted to settle in the United States near New York City.

Wednesday, February 13, 1946

I wrote a lengthy memorandum to Farrington Daniels requesting the irradiation of 95^{241} in a position of maximum flux in one of the Hanford piles. In my justification I say that such an irradiation will lead to a number of new heavy isotopes and probably a new heavy element or two. I note that we expect to have at least a milligram of 95^{241} which we propose to place in two aluminum containers, irradiating the first for three or four months and the second for approximately twice that long.

I wrote several notes today about the symposium in Atlantic City. To Charles Coryell, I transmitted copies of Libby's and my abstracts and say that I have discussed with Mulliken the problem of later publication. To Bill Libby, I sent copies of Coryell's, Burton's and my abstracts. I sent Milton Burton copies of Libby's and my abstracts and say that I share some of his pessimism about the amount of new material which may be cleared since I have recently had trouble. I also mention that Sidwell, Acting Editor of the Chemical Bulletin, asked me to urge him [Burton] to rush the second article on nuclear energy.

In a teletype to Hamilton, I ask him to airmail me a copy of his review article.

I read Don Stewart's request to T. S. Chapman for arrangements to be made for a bombardment of about 10 milligrams of U^{233} with helium ions at Berkeley for 50 to 100 microampere hours. Because of the need for immediate chemical processing after the bombardment, we request that this be done at Berkeley.

Arthur Jaffey replied to a letter I received from Godfrey Hammond, Publisher, Popular Science, who is interested in educating the public, not only in science, but also in the implications it has for our way of living. Jaffey says,

> As an atomic scientist who has become deeply aware of the grave dangers before this country if the atom bomb is not controlled by some means, I think that the greatest good would come from educating the public on the real nature of the problems arising from the release of atomic energy. I think that such education might include an analysis of what one might really expect from peacetime uses of atomic energy.

Jaffey mentions that he has placed Hammond's name on the mailing list for future issues of the Bulletin of Atomic Scientists of Chicago.

Today the "Summary Report for January, 1946, for the Chemistry Division" (CS-3421) was issued. The work of Section C-I is that covered in Manning's memo to Daniels of January 19 (MUC-GTS-2155). The report of Section C-II includes the work being done on the High Temperature Pile. Comparative testing of BeO bricks is in progress, and preliminary results indicate: (1) Commercially fabricated shapes can be obtained which contain less than 0.5 ppm of boron. (2) Samples from four manufacturers have densities of about 2.05, 2.3, 2.65, and 2.85. (3) Crushing strengths of 90,000 psi, roughly equivalent to the crushing strength of cast iron, are characteristic of hot pressed samples of BeO of density 2.8. (4) The resistance of BeO to thermal shock is sufficiently great so that samples may be heated to 1,500°C and dropped into water without shattering and with a decrease of crushing strength of only 20 percent or so after two such cycles. (5) The tendency of BeO shapes to shrink on reheating for prolonged periods above 1,450°C is a function of the mesh size of the powder used in fabrication as well as of the density achieved.

Section C-II also reports that the long-lived isotope of 43 (half-life ~350,000 y), the discovery of which in uranium fission products was reported last month, has definitely been shown to be a 43 isotope by the identity of its chemical behavior with that of 6 h 43^{99}.

Harold Ickes, Secretary of the Interior, resigned his cabinet post today. He is the only remaining continuously serving member of the Cabinet appointed by President Roosevelt when he was first inaugurated as President.

It is snowing hard tonight with heavy winds blowing.

Thursday, February 14, 1946

Paul C. Aebersold called me from Clinton with the news that he is now in charge of an office whose function will be to arrange for the preparation of radioactive isotopes for the use of scientists. We talked about the problem of producing C^{14} in a usable form. We also talked about Joe Hamilton's request for heavy isotopes, such as 96^{242}, Pa^{231}, and Ac^{227}; I indicated that we could supply such isotopes.

Abraham S. Friedman, who is a member of the solvent extraction group, is being discharged from the SED today.

I gave Helen a box of candy today for Valentine's Day.

According to this morning's newspaper, advocates for switching to the metric system in the U.S. are planning a national drive because of increased interest in the system.

The blizzard starting last night brought a total of five inches of snow, with temperatures having dropped to 0°F before midnight.

Friday, February 15, 1946

Robert E. Elson began working in the recovery group today. He comes from the Pittsburgh Plate Glass Company of Barberton, Ohio.

I received a disappointing, but not unexpected, letter from Bruno Pontecorvo who informed me that he has decided not to accept the Berkeley offer. Pontecorvo says,

> Since I have been with the Canadian project for several years, I feel that it would be a pity to leave just when research, as distinct from engineering starts. The time at which the pile will be available for research has been delayed considerably, and I do not think there will be much research with the pile this year.

A. V. Grosse sent a confirmatory letter and copies of correspondence to me, which I received today, about the Beilstein-Gmelin Committee. He also includes for my criticism the digest about plutonium that he prepared for Colonel Bradley Dewey for use in his ACS presidential address.

I also received a letter from Edgar J. Murphy saying that it will be satisfactory to delay my article for C & E News until May.

I read a copy of a memorandum from Zachariasen to Daniels verifying that a melt of 2 $ThF_4 \cdot 1$ NaF gave a hexagonal phase that is $ThOF_2$. He has been unsuccessful in preparing UOF_2.

In an addendum to MUC-GTS-2195 I suggest to Daniels that the investigation of the products of the irradiation of 95^{241} will shed light on the operation anomalies which are turning up during the processing of the 400 gt plutonium.

At 6:15 p.m. Helen and I had dinner with the Chicago Section of the ACS at the Furniture Mart on North Lake Shore Drive. We rode to the Furniture Mart with Professor and Mrs. T. Fraser Young of the Department of Chemistry of the University of Chicago. I then gave the after-dinner talk, entitled "Some Chemical Aspects of the Plutonium Project." Helen's chemistry teacher (when she took her course in freshman chemistry at the YMCA College in The Loop last academic year) was present at the dinner and talk and was startled, even angry, when he learned of my connection with Helen.

Averell Harriman has resigned as U.S. Ambassador to Russia, and President Truman has appointed Lieutenant General Walter Bedell Smith as his replacement.

Temperatures fell to -3°F early this morning, but by 5:00 p.m. the thermometer read 20°F.

Saturday, February 16, 1946

David H. Templeton terminated today. He will start to work in Berkeley on February 25.

Correspondence today consisted of a letter from George Calingaert to the speakers for the 1946 Conference on Petroleum Chemistry at Gibson Island. One noteworthy item is that recreational facilities available for the free afternoons include, golf, tennis, and swimming. Sidney D. Kirkpatrick wrote, enclosing my banquet ticket, to describe the program for the Award dinner in New York on February 26.

I read a statement prepared by Iz Perlman to justify the building of a hot laboratory in Berkeley. Perlman says,

> The requested addition to the present chemistry building is to house work on highly radioactive materials for which there are at present no adequate facilities at Berkeley. The addition is requested bearing in mind the importance of carrying out this work, along with the fact that the personnel best qualified in the field are returning to Berkeley from several other Manhattan Project sites where they have been working during the war period.

This program has been started by the group under the direction of Dr. Seaborg. A large part of this group is returning to Berkeley within the next few months and it is hoped that facilities can be made available to allow this important work to proceed unimpeded. The success of the program is directly dependent upon the ability to work on material bombarded in the Hanford pile and in the Berkeley cyclotrons. Without adequate hot laboratory facilities many phases of the projected program would be impossible without endangering the health of the research men.

Another important function that the requested building addition would fulfill is to allow the semi-works scale trial of the solvent extraction plutonium separation process developed in Berkeley under Dr. Calvin's direction. The facilities that would be provided for this work would also be used in Dr. Seaborg's program to process larger amounts of material when this is necessary. Such new solvent extraction processes are being considered most promising for processing plutonium and uranium from breeder piles.

It is our considered opinion that these facilities, along with the laboratories existing here now, represent the minimum requirements for the investigation of vital aspects of future pile developments. Considered in the light of the concentration of highly experienced men to be in Berkeley, one can give unqualified assurance that the facilities will be properly used to the best advantage.

Today's top headline reads "A-Bomb Secrets Stolen!" Royal Canadian mounted police have detained at least 22 men in an investigation of the disclosure of secret and confidential information to members of a foreign mission in Ottawa. Speculation is that the information dealt with atomic energy and the country involved is Russia.

Sunday, February 17, 1946

Helen and I again visited the Museum of Science and Industry today.

This morning's paper carries a front page article headed "Canada Denies Atom Test Leak." This is the follow-up on arrests by the mounted police reported in yesterday's news. Today's paper says "atomic bomb secrets" were said not to be involved.

Monday, February 18, 1946

I attended a Laboratory Council meeting at 9:00 a.m. in Room 209, Eckhart Hall. Others present were Branch, Chisholm, Cole, Daniels, Dempster, Foote, Furney, Hogness, Hughes, Jacobson, Jesse, Lapp, Mayer, Moulton, Mulliken, Nickson, G. Young, H. Young, Zachariasen, and Zinn. Dr. Maria Mayer, who will serve as Section Chief for the Opacity Section was introduced.

Daniels said that, as a result of the unanimous poll taken at the last meeting, he has sent a letter to Senator McMahon, congressional leaders, the President, and others endorsing the McMahon Bill for future control and direction of nuclear energy.

Daniels then announced that the administrative offices of the Met Lab will move out of Eckhart Hall between April 15 and May 1. He indicated that the Metallurgical Laboratory, as such, will close on July 1 and the regional laboratory, as a continuation of the Metallurgical Laboratory, will start on July 1, 1946. The regional laboratory will have some such name as "Government Laboratory at Argonne." The budget will be essentially the same as the present budget, and the shop building will be built soon.

There was a discussion of the forthcoming nuclear weapons tests in the Pacific. There will probably be a test from a boat during the period April 1 to July 10, an air drop during the period May 1 to June 1, and an additional test during the period June 1 to July 15. Division Directors are asked to submit names of recommended personnel as observers.

It was announced that the declassification policy, long expected, may be in operation soon.

Nickson gave a report on the bills in Washington for the control of nuclear energy. The new Johnson Bill leaves the control with the Army. The McMahon bill will be rewritten to make it more acceptable because all, except Tydings and McMahon, are against the original bill (S. 1717). The Senate committee now consists of Brien McMahon, Edwin C. Johnson (Colorado), Tom Connally (Texas), Richard B. Russell (Georgia), Harry F. Byrd (Virginia), Millard E. Tydings (Maryland), Arthur H. Vandenburg (Michigan), Eugene D. Millikan (Colorado), Warren R. Austin (Vermont), Bourke B. Hickenlooper (Iowa), and Thomas C. Hart (Connecticut).

Paul Aebersold called me to talk about furnishing Joe Hamilton with more heavy isotopes--Pu^{238} and U^{232}. We also discussed the possibility of using the Berkeley cyclotron to produce radioactive isotopes for national use.

Paul J. Sykes, the Air Force captain in the Pacific, wrote a letter, dated February 9, to thank me for the suggested reading and other information I sent him about nuclear energy. Sykes says that he hopes to return home soon to Canada to resume studies in physics and mathematics.

A letter arrived from Walter J. Murphy, Editor, Industrial and

Engineering Chemistry, announcing an ACS exhibit about nuclear energy in New York. He extended an invitation to visit the exhibit on February 26 and be a guest of the Society at a luncheon in the Shelton Hotel at 12:30 p.m. Other invited guests include senators, General Groves, and other scientists.

Hood Worthington at Wilmington called to tell me that the Army made its official request for the Np^{237} extraction run at Hanford, and therefore HEW will undertake this special run soon. We also talked about the arrangements for shipping irradiated samples.

I immediately wrote to Dr. D. A. Miller at Richland to tell him of the conversation and to enclose copies of a memorandum giving some simple changes in the procedure in order to improve the yield.

The Royal Smiths, Edrey's parents, had dinner with Helen and me in our apartment at 6:15 this evening.

Under a February 17 dateline in the morning paper is the report of the arrest of a Canadian official in London in connection with the leak of information regarding atomic energy.

Tuesday, February 19, 1946

This morning I attended the meeting of the Heavy Isotopes Group. Those present were Asprey, Cunningham, Florin, Ghiorso, Hindman, Hopkins, Hyde, Jaffey, James, Katzin, Manning, Osborne, Peterson, Scott, Sedlet, Simpson, Studier, S. Thompson, Van Winkle, Weissbourd, and Westrum. I announced that it is not worthwhile to go through the progress of the writing, paper by paper, so we will start on the laboratory work. First, I made a few comments about the meeting of the Laboratory Council. I said that the Project is moving out of Eckhart Hall during the month of April. Some of the officers are going to the Armory and the others are coming to New Chemistry. The Army has requested that we make drastic cuts in the number of long distance calls and teletypes which are sent. I then asked Burris to report on the work on 95.

Cunningham said that last week he reported on the absorption spectrum which he obtained using 100 μg of 95. There are only two peaks, one in the visible and one in the near infrared. There is a very sharp and distinctive peak at 503 ± 1 millimicrons. The molar extinction coefficient is about 300, which is among the best and sharpest peaks known. There is also a rather broad peak at 780 to 815 millimicrons. The molar extinction coefficient of this peak is about one-sixth that of the sharp peak. Several oxidation experiments have been tried. (1) Argentic ion and 4 M nitric acid gave no effect on the sharp peak indicating that the oxidation potential for this oxidation state of element 95 is more negative than ca. -2.0 volts. (2) The same solution was tried with the addition of 1 M sulfate ion. No effect was observed in this solution. (3) Stan Thompson has reported apparent oxidation in concentrated nitric acid with potassium bromate. Under these conditions the sharp peak disappears in 15 minutes, and then the peak grows back in. This seems to be conclusive evidence for the oxidation of 95. It cannot be said with certainty whether this oxidation is to the 4 or higher state. Neither can he say what the value of the standard oxidation potential is. (4) In alkaline solution no sharp peaks are observed. Upon the addition of hydrogen peroxide to this solution there is an increase in the absorption in the violet. This may have been due to impurities. He cannot say for sure that oxidation occurs. The concentration was extremely low due to the low solubility of 95 hydroxide. It is possible that by the addition of a complexing ion and the use of a longer cell more definite information on alkaline solutions will be obtained. The next experiment planned is the use of the polarograph to look for a lower oxidation state. New micro-cells will have to be made, so it will probably be two days before these results can be obtained. No new peaks were observed in the oxidized solution but this does not necessarily mean that there are no such peaks. The speed of reduction of the 95 prevents an accurate spectrum determination.

I remarked that from the information available at the present time it does not seem that the upper state will be very useful, but it may be in the future when more is known about it. Cunningham noted that they plan to try extraction of the upper state into butyl phosphate. A discussion then followed about the use of other solvents, particularly the fluocarbons.

I noted that the salt solutions containing 95 came in from Site Y and asked the progress on these solutions. Stan Thompson reported that Stewart and Fineman precipitated the hydroxide at West Stands. The slurry was then shipped over here. After two precipitations the original salt solution contained only 0.6 mg of remaining 95 and about 200 mg of plutonium. The slurry which we are now working on contains large amounts of iron and plutonium in several five-gallon jugs. One-fourth of the total has been through a reprecipitation and one oxidation cycle. This procedure eliminated most of the iron and plutonium. It will be taken through one more cycle and then into the fluosilicate cycle. There seems to be 10 to 20 mg of 95 in the whole sample. The practice run has given satisfactory yields through one fluosilicate cycle. Britain and Stewart will help on this job when they get back.

Roy Thompson discussed the analysis of the samples from Agruss. He has found 16 to 17 mg of protactinium in the three fractions. Five mg were in a fraction of about 40 gm total weight which was completely soluble in hydrofluoric acid. Most of the remaining protactinium is in a large insoluble fraction of about 400 g. The indications are that about one-half of the alpha particles is due to protactinium. This would indicate a growth period of about seven years so the amount of actinium can be calculated using this information.

Van Winkle reported that the sample of Pa^{231} which was bombarded at Clinton has been examined for the Ra^{223} and the Ba^{140} content. The cross section for fission as calculated from the Ba^{140} yield is 0.15-0.2 b. The yield of U^{232} gives a cross section of 170 b for its formation by neutron capture by Pa^{231}, using a half-life of 70 years for U^{232}. This is to be compared with the previous value of 90 b obtained at Argonne. I remarked that the main discrepancy I find in these results is the one between Jaffey's value of 290 b obtained from his measurement of the beta particles of Pa^{232} and the 90 b value Van Winkle found--both measurements made in the Argonne pile.

Van Winkle said he has a purified sample of 0.75 mg of Pa^{231} for shipment to Clinton and a small sample for Ghiorso's fission measurements. He asked whether a monitor will be included in the sample which goes to Clinton. It was decided that a Np^{237} sample should be included as a monitor. The U^{232} is ready for plating whenever a sample is wanted.

Ghiorso commented that the old fission measuring chamber at Argonne is now being used by Fermi's group so new fission measurements may have to wait until the new one he has ordered is completed. The isotopes on which he intends to make fissionability measurements are: U^{232}, Pu^{238}, 95^{241}, Np^{237}, Pa^{231}, Ra^{226}, Ra^{227}, Th^{227}, Ra^{223}, 96^{242}, Ac^{227}, U^{233}/U^{234}.

I asked about the plans for the U^{238} plus helium ions bombardment, and James said they intend to mill off layers in a manner which will depend on the success of milling one mil layers and then to isolate the plutonium fraction from each layer. We will then measure the amount of all plutonium isotopes from Pu^{236} to Pu^{241} in each layer. Some discussion followed concerning the methods of obtaining a perfectly flat target for the milling operations.

I announced that our request for another neptunium run at Hanford has been approved. It is believed that the same amount of uranium will be processed even though the gt level is higher, so that we should obtain much more neptunium than in the previous runs.

Hyde reported that the information on Pu^{234} is still the same and probably nothing can be added until a bombardment is worked up at Berkeley.

Studier said the decay of x-rays in the neptunium fraction of U^{233} plus helium ions bombardment are turning over into long-life activity. The gamma-rays continue to decay with the 4½ day half-life. This is what is expected since no gamma-rays accompany the long-lived Np^{235}.

I remarked that Sedlet is to isolate the protactinium and thorium fractions from the U^{238} plus helium ions bombardment. He will consult with James on the condition of the solutions in which these fractions will be obtained.

Anderson then reported that he has prepared a compound of the composition Cs_2PuCl_6. It has a solubility of a few grams per liter in a 3 N HCl solution with a slight excess of cesium. Potassium and rubidium compounds are too soluble to isolate. The compound, $CsPuCl_4$, might be expected but has not been observed. Some work was also described on the isolation of Pu(IV) ferricyanide and ferrocyanide. Two compounds have been isolated and identified. These are $Pu_3[Fe(CN)_6]_4$ and $Pu[Fe(CN)_6]$. The Pu(III) compounds are very insoluble, but formulae have not been established.

I told the group that there is a report from Hanford on the specific activity of plutonium produced at various gt levels. They report 7 percent increase at 400 gt and a linear dependence on gt.

Manning reported that he has made some calculations concerning the amount of U^{237} to be expected from $U^{235}(n,\gamma)U^{236}(n,\gamma)U^{237}$ in the new heterogeneous pile being constructed at Clinton. This pile will operate with a flux of 2×10^{14} n/cm^2/sec. The U^{235} will be changed every ten days and processed for return to this pile. In the 225 grams of U^{235} which is processed in a daily batch, 230 curies of U^{237} will be produced. After four times through this cycle nearly 2,000 curies will be produced. This will greatly complicate the problem of fabrication of the metal foils and may also place a limit on the practicality of the U^{233} breeder piles. Manning also calculated that after four times through, about 40 mg of Np^{237} would be produced per day.

I said there are some interesting reports from Site Y on the purification schemes, probably of particular interest to Cunningham, Stewart, and Asprey.

I called M. L. Hartung about the arrangements for my talk, "Atomic Energy," for the Men's Mathematics Club of Chicago on March 15. I will be picked up at my home at 5:30 p.m. and taken to the downtown YMCA for dinner at 6:00 or 6:30; the after-dinner talk will probably be in the same room.

In reply to a letter I received yesterday from Georg Mann, Science Editor of The World Book Encyclopedia, I say that the discoverers of element 95 are Seaborg, James, and Morgan while the discoverers of element 96 are Seaborg, James, and Ghiorso. I then note that he is correct that neither element has been named.

I received another letter, dated February 13, from Paneth who is now at the University of Durham. Paneth congratulates me on the discovery of elements 95 and 96 and goes on to say that, if I think the time is ripe for a discussion of the names of some of the unnamed elements, he would be glad to write a short letter to Nature about the necessity for having internationally accepted names for elements 43, 61, 85, 87, and now 95 and 96. He then asks my opinion of Berta Karlik's claim for element 85. Paneth sees no reason to change element 86 from "radon" to "emanation," but he doubts that anyone would object. He also prefers "protactinium" for element 91 rather than "brevium." He also asks if it is advisable to call attention to Auer's name "cassiopeium" for element 71 rather than "lutecium"?

Other letters I read today include a copy of a letter from Jake Warner to Edgar J. Murphy, who has asked Warner to prepare an article entitled, "Chemical Problems and Special Techniques Associated with the Development of Atomic Energy," for C & E News. Warner wrote that he does not believe that he is the logical man to write such an article, but suggests first Seaborg and then such men as Willard, Manning, Perlman, Thompson, Orlemann, Brown, English, etc., as more suitable. Warner goes on to say that he would be willing to write an article on the chemistry, purification, and metallurgy of plutonium (work with Charlie Thomas), or the metallurgy of uranium, or the refractory problems. Another reply I read today was that of Farrington Daniels to E. P. Wigner. Daniels regretfully says that the Metallurgical Laboratory will not be able to cooperate with Wigner and Dr. Chang for the determination of the fine structure of alpha particle decay. Daniels says that the supply of some of the isotopes is so limited that we cannot spare the material now, nor can we spare the time to prepare the samples. He notes that Kohman feels it would not be possible to prepare the material until late spring or summer and apparently Dr. Chang's magnet will not be available then.

I received a report "60-inch Cyclotron Activities" (CP-3430) by Joseph Hamilton. He notes there was some operational difficulty during January and says, that among other bombardments, there was a 700 μah helium ion bombardment of U^{238}, a 60 μah helium ion bombardment of U^{235}, and a 110 μah helium ion bombardment of U^{233}, for Glenn Seaborg's groups at Chicago and Berkeley.

Another report which I received today is dated February 6, 1946, and titled, "The Relationship Between the Range and Spectrograph Energy of Beta Particles," by L. E. Glendenin and C. D. Coryell.

Helen had an appointment with Dr. Davis, her obstetrician, at 1:15 p.m.

In an article in this morning's newspaper Major Alexander P. De Seversky says the atomic bomb provided the perfect excuse for Japan to surrender.

Wednesday, February 20, 1946

Today Don Stewart began working as a civilian for Section C-I at $400 per month.

Mary Alves of Life magazine replied to my request for copies of Fritz Goro's photographs, saying that we will receive a set to be used for educational purposes. Fritz, she notes, has not yet returned to New York. She suggests that I give him my preference for pictures, if I see him.

Perlman wrote about the procurement of some relatively pure U^{236}, whose properties are important in the considerations of future enriched U^{235} piles or in converter or breeder piles that start with uranium. He believes the best source is the uranium from the last Fermi sample that was irradiated at Hanford and suggests we try to retrieve some of the uranium sent to Los Alamos and run it through the small mass separator that is being built.

I also received from Berkeley the bombardment log of U^{233} with 44 Mev helium ions.

Daniels sent Captain Chapman the formal request for the neutron irradiation of 95^{241} for which I asked. In addition he asks Chapman to procure 1 ml of high-level Hanford solution and hexone extract of waste solution for us. We will use this to attempt to determine the cause of the product loss in 400 gt plutonium, as described by Hood Worthington during a recent Chicago trip. In this regard, I also wrote to Hood Worthington to give him information about the strongly irradiated uranium and plutonium samples (Farmer's No. 2) which were analyzed by chemical and nuclear means in our laboratory.

In a memo to Chapman, I give the analysis of the Agruss material: 17.45 mg Pa^{231} and one-quarter millicurie of Ac^{227}. I say that I should like to retain the 17.45 mg to apply toward our original request for 50 mg.

I received a note from S. D. Kirkpatrick, saying that a tuxedo is the appropriate dress for the reception on the 26th. Elliott Abers sent me a check for my talk at Purdue and notes that he will send me a copy of the picture taken on that occasion in the near future. Finally, an invitation arrived from Watson Davis inviting me to be a guest at the Fifth Annual Talent Search scholarship awards dinner in Washington, D.C. on March 5.

Helen visited Lying-In Hospital today and then went downtown to shop.

Today's paper reports that Secretary of State Byrnes asserted at a press conference yesterday that, as far as he knows, the United States has exclusive possession of manufacturing "know how" of the atomic bomb.

Thursday, February 21, 1946

John Natale began working in West Stands with Lawroski today. He has a B.S. from Penn State and has been working with the Grasselli Chem Department of du Pont.

Early this morning I wrote to French Hagemann, c/o Federation of Atomic Scientists, in Washington, D.C., to suggest that we have a game of golf on Thursday afternoon February 28 when I shall be in Washington.

I also wrote to Charles D. Hodgman, Editor, Handbook of Chemistry and Physics, saying I have no objection to the publication of the "Table of Isotopes" in the next edition of the Handbook. I enclose a reprint with marked typographical errors and additions.

Manning sent to Hogness the "Summary of Work of Section C-I for Period January 15, 1946 to February 15, 1946" (MUC-GTS-2208). He includes the following:

Deuteron Bombardment of U^{233}. A 15 mg sample of U^{233} was bombarded with 22 Mev deuterons in the Berkeley 60-inch cyclotron for about 320 microampere-hours. There was no evidence in the neptunium fraction separated after the bombardment for alpha-emitting isotopes, but at least one G-M activity was observed (in addition to the 2.0-day Np^{238} and 2.3-day Np^{239} activities resulting from reaction on U^{238} which was present to the extent about 4 percent in the original U^{233}). This activity is characterized by 1.8 Mev gamma-radiation and x-ray activity and decays with a half-life of 4.44 ± 0.05 days. This activity was also observed in the neptunium fraction from a deuteron bombardment of U^{235} and helium ion bombardments of Pa^{231} and U^{233}. The fact that this activity was formed by deuterons on U^{235} appears to rule out the possibility of its being due to Np^{231} or Np^{232}. The activity, therefore, may be assigned either to Np^{234} (produced by a d,n reaction) or to Np^{233} (produced by a d,2n reaction). On theoretical grounds it is somewhat more probable that the observed activity is due to Np^{234}, going to U^{234}. G-M activity due to shorter-lived isotopes of neptunium, even if present, might have been obscured by the Np^{238} and Np^{239} activities. The fact that no neptunium alpha-particle activity has been observed is in agreement with the observation that the G-M activity decayed completely to background with a 4.4 day half-life instead of bending over into a Pa^{230} activity which would have resulted from alpha-particle decay of Np^{234}. This indicates that the alpha particle branching ratio for Np^{234} decay cannot be more than 0.01 percent. However, a small amount of Pa^{230} and U^{230} daughter activity was isolated from the original solution after bombardment. This activity could have arisen either directly as the result of a hitherto unobserved type of reaction, $U^{233}(d,\alpha n)Pa^{230}$ or as a result of slight alpha branching in the decay of Np^{234}. The amount of activity due to Pa^{230} and daughters, while small, was somewhat large to have been accounted for by the latter mechanism and it is slightly more probable that it was formed directly by the d,αn reaction.

Helium Ion Bombardments of U^{233}. In two bombardments, 15 mg samples of U^{233} were bombarded with 44 Mev helium ions in the Berkeley 60-inch cyclotron for 50 and 100 microampere-hours, respectively. In the first bombardment, the uranium contained about 4 percent natural uranium, while in the second, the natural uranium content was about 2 percent. Rapid chemical separations were much more successful in the case of the second bombarded sample, and most of the data were obtained from this sample. In the plutonium fraction separated after bombardment, there was observed the 4.3 cm alpha radiation of Pu^{236}. There was also observed in the plutonium fraction a small amount of alpha particle activity with a range of about 4.7 cm which decayed with a half-life of about eight hours (50 percent). A small amount of x-ray activity was observed in the plutonium fraction when first isolated. Most of this decayed with a half-life of about 4½ days and was shown by later separation to be due to a small contamination of the plutonium fraction with neptunium. In addition to the 4.5-day activity, there was slight evidence for a small amount of x-ray activity decaying with a shorter half-life, perhaps due to the same isotope as the 4.7 cm alpha activity. Plutonium-234 appears to be the isotope most likely to be responsible for the short-lived alpha activity. This would be formed as a result of the reaction $U^{233}(\alpha,3n)Pu^{234}$. Activity due to the U^{230} decay series was isolated from the original solution after bombardment in about the right amount to be accounted for by formation from alpha decay of the short-lived plutonium isotope. This is the basis for tentatively assigning the activity to Pu^{234} in preference to any lighter plutonium isotope. There was no 40-day x-ray activity observed in the plutonium fraction. This appears to rule out the possibility of the 40-day activity observed previously in helium ion bombardments of U^{235} being due to Pu^{235}, which would have been formed in considerable abundance in the present bombardment as a result of an α,2n reaction of U^{233}. Hence it appears that the 40-day x-ray activity can be definitely assigned to Pu^{237}, which is not formed by the reaction of helium ions on U^{233}. It may be concluded from failure to observe any activity attributable to Pu^{235} in the present bombardment that the half-life of this isotope for orbital electron capture must be either less than 12 hours or, less probably, more than five years.

In the neptunium fraction separated after bombardment there was no evidence for neptunium alpha activity. X-rays and 1.8 Mev gamma-rays decaying with a 4.4 day half-life, probably due to Np^{234}, were observed, with an x-ray decay curve later bending over into a longer-lived component, probably the eight-month Np^{235}. The Np^{235} and Np^{234} were presumably formed as a result of α,pn and α,p2n reactions and perhaps, in part, from orbital electron capture by the corresponding plutonium isotopes.

Further Measurements on Thermal Neutron Fission of Th^{227} (radioactinium) and Ra^{223} (actinium X). Further verification of the fission of Th^{227} and Ra^{223} with slow neutrons, first reported two months ago, has been obtained by retesting in the thermal column of the Argonne pile the samples which had been measured earlier. In both cases, the fission rate decreased after the 40 days which had elapsed since the first measurement by an amount expected from decay of the 19-day Th^{227} and the 11-day Ra^{223}. The observed decay of fissionability eliminates the possibility that the originally observed fission was due to contamination by long-lived fissionable isotopes.

Half-life of U^{232}. A sample containing Pu^{236} (half-life about three years) to the extent of about 2.1×10^5 c/m was allowed to stand for 49 days. The U^{232} daughter activity which grew into the sample was separated chemically using added U^{233} as a tracer to determine the chemical yield. Alpha range analysis of the separated uranium sample indicated that 274 counts per minute of U^{232} had grown into the sample. From this result the half-life of U^{232} was calculated as 71 ± 10 years. This value supersedes the earlier approximate value of 30 years.

Chemistry of Element 95. Over 100 micrograms of 95^{241} have now been isolated from plutonium which was manufactured at a high concentration level at Hanford. With this material it has been possible to determine the absorption spectrum of the III state of element 95 in an aqueous nitric acid solution. Aside from a general absorption in the ultraviolet there are only two marked absorption peaks between 320 and 1100 millimicrons--one in the visible at 503 ± 1 millimicron, and one in the near infrared at 780 to 815 millimicrons. The peak at 503 millimicrons is very sharp with a molar extinction coefficient of at least 300. The molar extinction coefficient of the peak at 800 millimicrons is about 50. Addition of potassium bromate to a concentrated nitric acid solution of element 95 resulted in disappearance of the sharp peak at 503 millimicrons in a period of about 15 minutes, and then a slow regrowth of the peak. This appears to be conclusive evidence for the oxidation of 95(III) to a higher oxidation state. It cannot be said with certainty whether this oxidation is to 95(IV) or to a higher state. No oxidation was observed when argentic ion was added. The observed oxidation in the presence of bromate and concentrated nitric acid confirms results of earlier tracer experiments under similar conditions in which it was deduced from carrying behavior that oxidation to a higher state had probably occurred.

Redox Solvent Extraction Process. Several first cycle runs have been made in an attempt to decrease the plutonium losses in the second column. The plutonium losses in this column at present appear to be 0.1-0.2 percent as compared with earlier average values of about 0.5 percent. It was found that 0.10 M ammonium acetate in the aqueous extractant and/or 0.15 M hydrazine acetate instead of 0.15 M hydrazine nitrate in the hexone scrub solution were both beneficial measures. Making the hexone phase continuous above the feed point resulted in increased plutonium losses. A column run with 0.9 M $Al(NO_3)_3$ instead of 8 M NH_4NO_3 in the first column scrub solution indicated a lower salting out of efficiency for the $Al(NO_3)_3$ as far as U(VI) is concerned. This, together with greater expense and lesser availability makes $Al(NO_3)_3$ less attractive as a substitute for NH_4NO_3 in the first column. In the second column, however, use of 0.9 M $Al(NO_3)_3$ in the aqueous extractant instead of 8 M NH_4NO_3 may have certain advantages. Plutonium losses and separation from uranium are about the same for the two salts, but $Al(NO_3)_3$ allows higher throughputs in the second column, and the plutonium solution may be concentrated several-fold by evaporation to allow working with Pu(IV) rather than Pu(VI) in the second cycle. A first cycle run was made with 50 percent UNH feed solution (instead of the usual 60 percent) prepared from irradiated "X" slug dissolver solution fortified with HEW concentrations of inactive fission product elements and plutonium. Plutonium losses were 0.2 percent and 0.1 percent in the first and second columns, respectively. Decontamination was several-fold better than in a run using the same uranium to which no inactive fission products had been added. Stainless steel packing in the second column has no great effect on plutonium losses. Use of 4 M NH_4NO_3 in the first column feed solution in addition to 2 M $UO_2(NO_3)_2$ allowed a 20 percent increase in feed throughput, keeping all other streams constant. Under those conditions, plutonium losses in the first column were the usual 0.03-0.10 percent, but the uranium losses were much lower than the usual, being less than 0.1 percent as compared with best previous values of about 0.2 percent.

The organization of Section C-I as of today is as follows:

Glenn T. Seaborg - Section Chief
Ruth P. Rogers - Secretary to Seaborg
Kathleen Florin - Clerk

Winston M. Manning - Associate Section Chief
Donald C. Stewart - Assistant Section Chief
Mary Jane Healy - Secretary
Lorraine Eisen - Secretary

Group 1, Heavy Isotopes
Seaborg, Glenn T. - Group Leader
Ames, Donald P. [SED]
Anderson, Herbert H. [SED]
Bentley, William C.
Cunningham, B. B.
Erway, Norman

Fineman, Phillip
Florin, Alan E.
Ghiorso, Albert
Hagemann, French
Hindman, J. Clark
Hopkins, Horace H. [SED]
Hyde, Earl
Jaffey, A. H.
James, Ralph
Katzin, Leonard I.
Kohman, Truman P.
Magnusson, Lawrence
Osborne, Darrell W.
Peterson, Sigfred
Scott, Benjamin F.
Sedlet, Jacob
Simpson, Oliver C. (part-time in C-I), in Section C-II
Studier, Martin [SED]
Thompson, Roy C.
Thompson, Stanley G.
Van Winkle, Quentin
Walsh, Patricia
Weissbourd, Bernard
Westrum, Edgar F.
Calhoun, Opaline (technician)
Nelson, Robert (clerk)
Thomson, Helen (technician)

Group 2, Recovery
Asprey, Larned B. - Group Leader
Britain, J. W.
Elson, Robert
Warshaw, Silvia

Group 3, Solvent Extraction
Lawroski, Stephen - Group Leader (part-time)
Blaedel, Walter J. - Assistant Group Leader
Ader, Milton [SED]
Billheimer, J. S. [SED]
Cavataio, Vincent S. [SED]
Evans, Harold
Goeckermann, Robert
Hausman, Eugene A. [SED]
Hyman, Herbert H.
Kelley, Alec [SED]
Leader, Gordon
Natale, John
Nachtman, Elliot
Post, Roy
Schaffner, Irwin J.

Schraidt, John H.
Sheft, Irving
Giacchetti, Olga (technician)
Guadagna, Lillian (technician)
Koziolek, Winifred (technician)

At 10:35 a.m. I left Chicago on "The Rocket" (Rock Island R.R., No. 501, seat 16, car D) for Peoria. The train arrived at 1:10 p.m. During the afternoon I visited the Northern Regional Research Laboratory through arrangements by Cecil D. Langford.

After dinner with the members of the Peoria Section of the ACS, I presented the after-dinner talk, "Atomic Energy." The discussion centered primarily around the chemical developments and achievements in the atomic field, with most of the emphasis placed on the synthetic element, plutonium. I reviewed the principles involved in the production of atomic energy from the nuclear fission reaction and described the development of the chemical procedures used for the extraction of plutonium from uranium and fission products at the production plants, including a history of the development of the chemical procedures. I also included a discussion of the discovery and study of a number of interesting new transuranium isotopes.

I spent the night at Hotel Jefferson.

<u>Friday, February 22, 1946</u>

I returned to Chicago on "The Rocket" which left Peoria at 7:00 a.m.

Yesterday while I was in Peoria, Stewart sent Chapman a memo saying that he is sending Chapman, Hagemann's design for lead shields to be used for the shipment of 60-80 kg samples of thorium irradiated in the Hanford pile. Hood Worthington asked for the design of these shields.

The Solvent Extraction Group met yesterday morning. Ader, Blaedel, Evans, Goeckermann, Hyman, Lawroski, Leader, Manning, Nachtman, Post, Schaffner, Schraidt, and Sheft were present. Blaedel reported that run 38U23P13F was made under flowsheet conditions with an IAF solution containing only 50 percent (1.5 M UNH) and 0.15 M HNO_3. The beta-particle and gamma-ray activity was only 0.5 percent of that corresponding to the HEW level, but flowsheet concentrations of inactive FPE had been added. The results fell short of expectations. Uranium losses in the IAW solution were 3-5 percent, and the alpha-particle losses were 0.1-0.3 percent. This was the first run in some time in which uranium losses were determined, and it is probable that they have been high right along. These high losses were checked in subsequent runs at 60 percent UNH in the feed. The difficulty is believed to be mechanical. Alpha-particle losses in the IBU solution were surprisingly low at only 0.1 percent. Beta-particle decontamination factors were only 1.1×10^4 and 1.4×10^4 in the IBP and ICU solutions respectively, and gamma-ray factors were only 3×10^3 and 4×10^3 in the same streams. An attempt was made to wash the IA column free of activity after the run was over by alternate washings with 3 M HNO_3 and hexone. At best, a decontamination factor of only 20 was obtained.

Blaedel then said that run 39U24P14F was made using the same dissolver solution as was used in run 38U23P13F, except that the IAF solution was prepared to contain 4 M NH_4NO_3 as well as 2 M UNH. Even when the IAF solution was run at 20 percent above normal flowrates, the uranium losses in the IAW solution were below the limit of detection (<0.01 percent). The other modification in this run was that the IBX solution contained 0.15 M $N_2H_4 \cdot 2$ HOAc instead of $N_2H_4 \cdot 2\ HNO_3$, and the IBS solution contained 0.15 M $N_2H_4 \cdot 2$ HOAc instead of the nitrate. Alpha-particle losses in the IBU solution were only 0.25 percent, considerably lower than usual. However, a reddish, crystalline precipitation formed in the IBX solution on standing, and was assumed to be basic ferric acetate. Decontamination in run 39U was roughly the same as in run 38U; apparently HOAc does not greatly affect decontamination.

Run 40U25P was made under flowsheet conditions, with column IB repacked with stainless steel 3/16-inch helices. Alpha-particle losses in the IBU solution were 0.35 percent, indicating no effect of stainless steel on column IB operation. Uranium losses in the IAW solution were still high and fluctuating (0.3-3 percent).

Run 41U26P is now in progress under flowsheet conditions, with both columns IA and IB repacked with stainless steel. Alpha-particle losses run about 0.05 percent in the IAW solution and 0.15 percent in the IBU solution. In this run, IBU aliquots were analyzed by throwing down LaF_3 and washing the precipitate with 1 N HNO_3 - 1 N HF before spreading to give 0.3-0.8 percent losses.

Manning remarked that dissolution of LaF_3 in ZrO^{++} may easily cause loss of plutonium in the analysis, and Blaedel commented that this was not checked, but in the past these losses were only of the order of 10-20 percent of the plutonium present.

Hyman reported that the packing was removed from the column after the washing studies mentioned by Blaedel. The following observations were made. The activity was greatest in the top of the column rather than in the bottom which contains the most active solutions. The packing and column were covered with a colored film which was darkest at the top of the column. He suspects the film to come from the gasket material. A study of beta-particle and gamma-ray absorption curves showed columbium to be the predominant activity left in the column. Appreciable amounts of both columbium and zirconium are present in the tartrate wash. An unidentified hard beta-particle was also found.

The activity can easily be removed from the column by a 1.5 N HNO_3 solution containing 1 N HF. A piece of stainless steel taken from the steel collar around the gasket was found to be active and was covered with the dark film. Over 90 percent of the activity was removed by the HNO_3-HF mixture with no effect on the film. Prolonged cleaning removed all the activity and the film from the steel. Concentrated nitric acid, carbon tetrachloride, hexone, and similar solvents do not remove the activity or dissolve the film. If these observations are taken at face value, it can be assumed that zirconium and columbium oxides precipitate on and are held by an adsorbent film formed by deterioration of the gasket material.

Goeckermann said that further investigation has been made of the effect of nitric acid, acetic acid, and substituted acetic acids on the distribution ratio and transfer ratio (ratio of percent plutonium extracted to percent strontium tracer extracted from hexone phase in a non-equilibrium experiment). It is evident from the results that the deleterious effects of nitric acid are more specific than can be accounted for by its greater acid strength as compared with unsubstituted acetic acid. Interpretation of the values of the transfer ratios must await settlement of the question as to the validity of the original value of $\sim$0.1 in the standard system. Experiments on further decontamination of the ICU solution have thus far been aimed at finding a scavenger which will efficiently remove ruthenium activity. Manganese dioxide is known to be a fairly effective carrier of ruthenium and zirconium activity, and most of the scavenging tests run so far have dealt with it. Although MnO_2 has not yet been shown to be a satisfactory scavenger for ICU solutions, its performance thus far seems sufficiently promising to warrant further study. The effect of the MnO_2 on the neptunium which will also be present in the ICU solution has not yet been determined. A number of other precipitates besides MnO_2 were tested as scavengers for ruthenium activity from synthetic ICU solution. All were found to be relatively ineffective: sodium uranate, U_3O_8(5 g/l), uranyl peroxide, uranyl phosphate, uranyl oxalate, Bi_2S_3 (3.0 g/l), CuS (3.0 g/l), PbS (3.0 G/l).

Manning asked whether the product of the MnO_2 scavenging is to be pure enough for direct use in metal production, and Blaedel responded that it should be pure enough to ship to Mallinckrodt. The committee from Hanford says decontamination factors of 10^5 or 10^6 are needed for shipping.

Lawroski said, for future programs, he hopes to get mixing chambers for column IA. The two types he has in mind are (1) enlarged section at feed introduction to prevent "slug" formation, and (2) a chamber with baffle plates. The size of the chamber will be equivalent to about four feet of column and should be able to handle maximum fluctuation of interface level. They will try s-diphenylcarbazide in a run. Ader is working on use of wetting agents to decrease interfacial tension and get smaller drops. Ader is, at present, conducting scouting experiments to obtain a suitable wetting agent for column IB. The most successful in our system seem to be the aerosols, fatty alcohol sulfates, and certain alkyl aryl sulfonates.

Helen learned yesterday that Edith (my father's cousin) and Rudolph Ericson are staying here in Chicago at the Palmer House until February 28.

* * *

In my correspondence today, I received a short note from Dan Miller saying that he plans to arrive in Berkeley the week of April 15 to start his graduate work. I immediately wrote to him to say that the arrival date is satisfactory. I also mention that the housing situation in Berkeley seems fairly good.

Martin Kamen wrote that he was out of town when my letter arrived about the ACS symposium and, unfortunately, the letter had not been forwarded to him. He does not have enough time to prepare a paper for the symposium, particularly since he is to present a paper in March for the Society of Biological Chemists and a paper to AAAS also in March. He points out that Barker, with whom he has done all the published work with C^{14} so far, will talk in Atlantic City at the Acetate Symposium. Milton Burton also wrote about the symposium. He says that since talking with Whitaker, he is less pessimistic about the release of something of value.

A note arrived from John Lewellen about arrangements for the Quiz Kids Program on Sunday in which I am scheduled to participate.

Another letter arrived from Watson Davis about arrangements for my talk at the Science Talent Institute in Washington, D.C., on March 2.

I received and read a copy of the letter Latimer wrote to General K. D. Nichols about the Hot Laboratory for my group in Berkeley and the Plutonium Isotopes Separations Laboratory. Latimer includes the plans for the Hot Laboratory, whose cost is estimated at $70,000. Latimer then states that the program Nichols and Groves requested is outside the general field of basic university research, but if Nichols requests such a task, Berkeley would be glad to do it. The estimated cost of the small chemistry building housing the Hot Laboratory, excluding equipment, would be $125,000.

Perlman called me from Berkeley this afternoon and we discussed the plans for this small chemistry building to be constructed at the Radiation Laboratory. He mentioned that he believes the six-hour activity produced from the bombardment of U^{235} with helium ions to be Pu^{233} rather than Pu^{234}. Perlman commented that Louis Werner's work on the separation of 95 from lanthanum is looking good. They also have bombarded lead with helium ions and seem to have produced some new activities.

I told Stan Thompson and Cunningham about Werner's good results on the separation of 95 from lanthanum with TTA. They asked me to write Perlman, which I immediately did, to request that Werner send Stan information on this separation and arrange to send us some more TTA.

I noted in Stewart's memo to J. E. Rose today, that Stewart corrected the radium inventory Rose sent me on February 6.

Helen visited Mrs. Farrington Daniels today, and in the evening she and I went to visit Marjorie and Darrell Osborne and their children (Kathleen, Carol, and Lorraine).

There is snow on the ground today as more than an inch fell yesterday.

Saturday, February 23, 1946

Bernard Weissbourd is being discharged from the SED today. Monday he will be placed on the Met Lab payroll as an Associate Chemist at a salary of $3,300 per year.

I wrote to Emilio Segrè saying

> As you know, I have been in correspondence with Paneth on a number of matters of nomenclature and in the course of this, the matter of naming elements 43, 61, 85 and 87 has come up. I just received another letter from Paneth in which he proposes to write a letter to Nature, pointing out the necessity for having internationally accepted names for these elements and pointing out why the present names are unacceptable.
>
> What is your thought as to which authors and corresponding "discovery" references should be suggested by Paneth in his note as the men to do the naming? These men in turn would reply to Paneth's letter to Nature with their suggestions for names. My own thought would be as follows. For element 43, Perrier and Segrè, J. Chem. Phys. 5, 712 (1937) or Cacciapuoti and Segrè, Phys. Rev. 52, 1252 (1937); which do you suggest? For element 85, Corson, Mackenzie and Segrè, Phys. Rev. 57, 459 (1940). For element 87, Perey, Comptes rendus 208, 97 (1939) or J. de phys. et rad. 10, 435 (1939). For element 61 I am especially uncertain. Should it be Law, Pool, Kurbatov and Quill, Phys. Rev. 59, 936 (1941), or some other team of this school, or Wu and Segrè, Phys. Rev. 61, 203 (1942), or both of these teams?

I then mention the question Paneth raised about Berta Karlik's claim to element 85, and ask Segrè what he thinks about the work of Leigh-Smith and Minder. I say that Paneth also wonders about element 71 --"cassiopeium" vs "lutecium."

Stewart sent L. C. Furney a summary of our stocks of isotopes of 49 and 25 saying:

> At the present time we are carrying 1,639 mg of isotope 25 on the Section C-I inventory. Practically all of this material, however, is in non-available form, being tied up in solutions resulting from the processing of irradiated targets or in such targets which have not been processed as yet (CW-4). We do have 260 mg as the tetrafluoride but are anxious to reserve this for the preparation of further targets for bombardments.
>
> Our January inventory of isotope 49 showed a total of 27,662 mg. Since that time, however, we have received a piece of metal weighing 121,636 mg, and also an unknown amount of 49 in the fifty gallons of salt solutions shipped to us from Site Y. We are not yet in a position to give an accurate estimate of the amount of 49 in this latter material, but preliminary assays would indicate the presence of at least fifty grams. Practically all of our 49 stocks, however, are relatively non-available, since they are being used for the production of element 95.

In a memo to Wayne Johnson, Manning recommends that Alan Florin be promoted to the grade of Associate Chemist.

I replied to an invitation from T. Agazim of the American Society of Brewing Chemists to address their banquet in Milwaukee on May 7, saying my schedule is very heavy until the time I return to California. I ask whether it would be possible to fill the engagement by leaving Chicago in the afternoon and returning in the evening.

A. V. Grosse, in a note, informs me that it would be best to locate him through John Dunning's office on February 26 and 27. He is anxious to contact me.

I also received a reply to the letter I wrote Samuel Grafton of the New York Post on February 11. He says, "You are quite right; I am not a world government man and I have resigned from the Writers' Board."

Helen and I went to see the movie "Stork Club" with Betty Hutton, Barry Fitzgerald, and Don DeFore at the Picadilly Theater this evening. The second feature was "Frontier Gal" with Andy Russell and Yvonne De Carlo.

"General Yamashita Executed" reads today's top headline. The general was hanged by the U.S. Army as a war criminal.

Sunday, February 24, 1946

Stan Thompson was baby-sitting this morning, so he brought Ruth Ann over to our apartment.

Helen and I went to the Merchandise Mart in the late afternoon. At 5:30 p.m., in the Executive Conference Room, Harold Urey, Joseph Mayer, Arthur Jaffey, and I met with Mr. Clifton Utley, the news commentator, who was to be "master of ceremonies" for the Quiz Kids program on atomic energy tonight. A warm-up session was held at 6:00 p.m., and the program was on the air from 6:30 to 7:00 p.m. The questions had been all cleared through Security before the program. I was surprised, however, to hear one of the questions, "Who is G. T. Sutton?" I answered that one by explaining that that was my code name during the war. No score was kept in order to make certain all "contestants" enjoyed the evening.

After the program Helen, who had accompanied me to the radio station, and I met and had dinner with Rudolph and Edith Ericson in the Empire Room of the Palmer House.

British authorities sent more police and military reinforcements into Bombay as civilian rioting continues, according to a news item under a February 23 dateline. News on the radio today indicates there is a calm after the rioting.

Monday, February 25, 1946

In the mail today I received a very appreciative letter from G. R. Barnett, Chairman of the Peoria Section of the ACS, thanking me for going to Peoria to talk.

A letter arrived from Iz Perlman noting the platinum target and Q metal are leaving on the streamliner February 26 and the lanthanum target will follow in about a week.

Also, in Berkeley, David Templeton is scheduled to start work on the Radiation Laboratory payroll.

At 3:30 p.m., with a migraine headache, I boarded the "Century" for New York (bedroom F, car 263).

The Chicago Sun started a series of articles on the "Battle for Atom Control" by Geoffrey Bancroft of the Chicago Sun Washington Bureau. The first article is entitled "Conflict Over Secrets Explodes Into War of Scientists vs Generals."

Tuesday, February 26, 1946

I arrived in New York. At 12:30 I was a guest of the ACS at a luncheon in the North Lounge of the Shelton Hotel. I then visited the ACS exhibit on atomic power in the Grand Central Palace in New York.

At 7:00 p.m., in tuxedo, I attended a reception in the West Foyer of the Waldorf. At the following banquet I received the Award for the University of California from the American Institute of Chemical Engineers. General Leslie R. Groves accepted the Award in behalf of the Manhattan District, James B. Conant discussed the role of the scientists while P. C. Keith outlined the contributions of engineers and industrialists.

Wednesday, February 27, 1946

In New York. I contacted and arranged to meet with A. V. Grosse, who asked me to criticize the speech he has prepared for Bradley Dewey, "Contributions of Chemistry to Atomic Energy."

Helen is scheduled to attend a pre-natal class at Lying-In Hospital.

Thursday, February 28, 1946

I travelled by train from New York to Washington.

In Chicago Larry Asprey is scheduled to terminate at the Met Lab in order to start graduate school in Berkeley, and Helen is scheduled for a dental appointment.

In Washington, French Hagemann, K. Bryson Fleer (ACS), Jim Crowe (ACS), and I played nine holes of golf at the Rock Creek Park Golf Course. (FH-55, BF-64, JC-53, GS-47.)

I am staying at the Statler Hotel.

MARCH 1946

Friday, March 1, 1946

In Washington, I am staying in the Statler Hotel.

French Hagemann, Melvin Freedman, and I played nine holes of golf at the Army-Navy Country Club (FH-57, MF-50, GS-50.)

Saturday, March 2, 1946

I was a guest speaker at the meeting of the 40 finalists of the Westinghouse Science Talent Search at the Statler Hotel this morning under the auspices of Science Service. My talk centered primarily around the transuranium elements in the atomic energy field, with most of the emphasis on plutonium. I reviewed the principles involved in the production of atomic energy from the nuclear fission reaction and described the development of the chemical procedures used for the extraction of plutonium from uranium and fission products at the plutonium production plants. I discussed the chemistry of plutonium and included a description of the whole history of this development with particular emphasis on the interesting work which was done with microgram quantities of material in the early days when no more than this amount of plutonium was available for the whole chemical research program. The talk included a discussion of the discovery and study of a number of interesting new transuranium isotopes.

After my talk Dr. Selman A. Waksman, microbiologist at the New Jersey Agricultural Experiment Station, New Brunswick, gave a talk entitled "The Story of Antibiotics." Luncheon was served in the South American Room of the hotel. Watson Davis and Dorothy Schriver were our hosts.

At 5:35 p.m. I left Washington (lower berth 6, car 53) to return to Chicago.

Sunday, March 3, 1946

When I arrived in Chicago this morning, Helen told me that she had visited Alice Katzin on Thursday. On Friday she met Barbara Hull at Fields, had lunch at Stouffers, and then visited Barbara's apartment.

I stopped at the lab and noted a letter from Paul Fugassi, who confirmed the arrangements for my trip to Pittsburgh and said that someone will meet me at the railroad station. I also read a letter, dated February 19 and forwarded from Berkeley, from Neil E. Gordon, Director of the Kresge-Hooker Scientific Library at Wayne University.

Gordon said that he has been given permission to publish my Detroit ACS talk in the Record of Chemical Progress of which he is editor. He would like to publish my picture and the periodic chart I used with the talk.

Helen and I then went with Stan and Alice Thompson to Cog Hill Golf Course No. 2, where Stan and I played nine holes of golf (ST-50, GS-58). The four of us had lunch in Lemont. It was a beautiful day with a high of 64.1°F at 3:05 this afternoon--a new record for March 3.

Prices of six major kinds of grain are being raised. The purpose is to accelerate the movement of the grain from the farms to get more food to people in the portions of Europe where people are starving, according to today's paper.

Monday, March 4, 1946

Events that took place in Chicago while I was away included the following:

Tuesday, February 26

The Heavy Isotopes Group met in the morning and the meeting was attended by Ames, Asprey, Bentley, Britain, Cunningham, Elson, Erway, Florin, Ghiorso, Hindman, Hyde, Jaffey, James, Jones, Katzin, Kohman, Magnusson, Manning, Peterson, Scott, Sedlet, Simpson, Stewart, Studier, R. Thompson, Van Winkle, Warshaw, and Westrum. Manning conducted the meeting and announced that the Q metal target should be in Thursday and Room 34 should be ready by the end of the week if it is to be worked up there.

Cunningham reported that attempts have been made to prove reduction of tripositive element 95 (to the dipositive state) by means of the polarograph. The results are not positive although the curves show reduction at about +0.8 volt on the Latimer scale and the amount of reducing agent indicated is consistent with the amount of 95 percent. He said there is, however, the possibility of peroxide being present, which would reduce the apparent oxidation state of the element. His plans for future work, when larger quantities are available, include: (1) attempted reduction in the spectrophotometer to settle definitely the question of a lower oxidation state, (2) dry chemistry, including studies on the relationships of the various oxides, and a new preparation of trichloride to re-check the melting point, and (3) an attempt to prepare the tetrafluoride.

Asprey announced that Thompson and Hopkins have worked the lanthanum-51 mixture from the Site Y solutions through two fluoride cycles. Cunningham is working with about 1/4 of the by-product precipitates, and assays show 3-4 mg of 51 in this portion. Asprey and Stewart have the plutonium from the solution as $KPuF_5$, and it is being metathesized preparatory to further purification.

Manning commented that we are interested in U^{236}, and Asprey plans to work a by-product of the first milking of the large amount of 364 gt plutonium recently received to obtain this isotope, which should be there as an alpha decay product of the Pu^{240} present.

Van Winkle reported that very little chemical work on protactinium has been done in the past week and most of the time has been spent on testing the new ultra-centrifuge made in our shop. This instrument should enable us to detect protactinium polymers containing as few as ten protactinium atoms per particle by means of sedimentation experiments. This will be of help in explaining much protactinium chemistry, including solubility behavior in acid and ammoniacal solutions. This ultra-centrifuge will get up to 150,000 rpm when an initial air pressure of 100 lbs/in^2 is used. It will handle cells with a capacity of about 200 microliters volume. Compressed oxygen or nitrogen will be used to maintain a constant gas pressure.

Jaffey said Ghiorso plans to go to Argonne this week or next. Cunningham will clean the plutonium out of the 51 sample previously used for fission measurements, and it will be used again. Van Winkle has a sample of U^{232} which will be electroplated and sent to Argonne for fission measurements.

Britain talked about the plutonium from CW-3 that has been put through one hexone extraction with a very high decontamination. The yield has not been determined yet. It will be turned over to Mrs. Warshaw to purify and will eventually go back into the Hanford pile in an attempt to product heavier isotopes of plutonium.

Peterson described the work done on the actinium fraction of the protactinium material obtained from Agruss. Most of the work is done blindly since there is no convenient method for following the actinium. The technique of flaming the plates to eliminate the element 87 and follow its re-growth has been found to give incorrect results because of lead and thallium daughters which appear farther down in the chain. Evidence has been obtained for the presence of a surprising amount of thorium. The actinium, radium, and thorium fractions are now all nearing separation. It is estimated that a few micrograms of actinium may eventually be obtained.

Ames said that in one run the highly purified radium gave a half-life value of 1,650 years, but the degree of accuracy of this figure is low. This figure compares with a value of 1,638 years given by Curtiss of the Bureau of Standards. In the present experiment, a 20-microgram sample was taken up in 3 M perchloric acid, an aliquot weighed out on a plate, flamed, and counting started immediately. An eight-hour decay growth curve was taken and the growth extrapolated back to zero time. Kohman commented that the method is intrinsically accurate only to about ½ percent. A special low geometry counter with a 2 channel pulse height selector will be required for better results.

In a discussion about the progress of the writing, Hindman said Chapter IV of Volume 14A is ready but is being held up by lack of typing help. Cunningham announced the chapter on neptunium is finished in rough draft, and Katzin reported that the chapters on protactinium and on thorium for Volume 17A are written, while Spedding's chapter on thorium metallurgy is still out. Kohman asked whether authors should be referred to by name in the text of survey chapters, and Jones responded that it is better to leave them out for the most part. Any historical surveys should be phrased in somewhat indefinite terms, since the PPR is an official government publication, and, as such, any statements in it can be taken as official enough to upset patent claims.

Sedlet reported that the uranium solutions just before ether extraction in the St. Louis process contain about 75 percent of the original ionium. Attempts are now being made to obtain a good analysis for protactinium in pitchblende. The ratio of protactinium to uranium will then give a ratio of U^{235} and U^{238} activities.

Kohman said that he is anxious to find out about Studier's 0.16 Mev gamma-ray--whether it is associated with U^{234} or U^{235}. If it is associated with the U^{235}, a long-range 25 alpha particle buried in the 24 peak would be possible and would account for the lower 25/28 alpha-particle ratios found by pulse analyses in natural uranium as compared with values obtained by radio-lead isotope comparisons or Pa/U ratios by chemical analysis. A final answer on this would seem to depend on counting yields for gamma- and x-rays. He said, on the basis of a paper from MIT, equations have been derived to determine this for our counter and asked if he should carry out the calculations. Manning replied that it certainly should be done eventually but wait until the writing is completed.

"The Isolation and Purification of Protactinium from Uranium Ore Residues" (CC-3365) by K. A. Kraus and Q. Van Winkle was issued today. The work for this report was carried out in the winter of 1944-45 here at the Met Lab.

Today's Bancroft article in the Sun is entitled "U.S. Scientists Fear Secrecy Will Launch World Arms Race."

Wednesday, February 27

"Scientists Say Army Passion for Secrecy Will Hold Up Progress" is the title of Bancroft's article today. Some scientists in the Manhattan project claim that army secrecy held up the atomic bomb completion for as much as a year.

Thursday, February 28

A copy of Perlman's letter to Manning arrived for me. Perlman says that our platinum target is leaving Berkeley on the 26. He would like us to determine what activities are removed from the platinum with glacial acetic acid at room temperature. He comments that if treatment with acetic acid will interfere with our experiments, perhaps a small part of the target could be sawed off and treated separately.

A letter arrived for Stan Thompson from Louis Werner at Berkeley describing his method of separating element 51 from lanthanum using TTA.

Bancroft's fourth article is titled "Groves Fights Atomic Rule by Scientists." Groves declares that the discoverers of atomic energy should be barred from any controlling commission because they are not "disinterested."

Friday, March 1

Eugene Hausman, who was discharged from the SED yesterday, was placed on the Met Lab payroll.

Saturday, March 2

Edgar F. Westrum officially terminated from the Met Lab today. He will start work at the Radiation Laboratory in Berkeley on Monday.

Stewart requested that Daniels make arrangements for the irradiation at Hanford of about 60 mg of high gt plutonium, decontaminated plutonium remaining from our CW-3 sample. This is a substitute for our request for re-irradiation of the CW-4 sample, a request that was refused.

* * *

I attended the meeting of the Laboratory Council at 9:00 a.m. in Room 209, Eckhart Hall. Others present were Branch, Chisholm, Cole, Daniels, Dempster, Furney, Hilberry, Hogness, Hughes, Jacobson, Jesse, Lapp, Manning, Mayer, Nickson, Rabinowitch, H. Young, Zachariasen, Zinn, and Zirkle. Daniels read a letter which he sent to Colonel Frye in regard to clearance for the transfer of Richard Adams to Site Y and the demand of the scientists that they have a right to a hearing in case they are denied clearance. The official clearance for Mr. Adams was immediately granted. Daniels urged that employees continue to cooperate in maintaining secrecy as requested by the Army.

It was announced that the Navy has provided opportunities for over ten persons from the Metallurgical Laboratory to participate in Operation Crossroads.

The Information Meeting will be held at Clinton beginning at 3:00 p.m., Monday, March 18, 1946. A special railroad car will leave Chicago at approximately 11:00 p.m. on March 17, 1946, and return Thursday evening.

The Council believes that the standard work week should be reduced, if possible, to 40 hours when a new contract is issued.

The program for 1946-47 has been forwarded to General Nichols and his advisory committee and calls for continuing the present activities at about their present level and transferring all the buildings, equipment, and most of the personnel to a national laboratory cooperating with the universities, particularly those in the middle west. The laboratory would be under the Army controlled by a board of directors elected by representatives from the universities, and administered for the present by the University of Chicago. The direction of the research program would be in the hands of the Director and the senior staff. New buildings would be built at Argonne as rapidly as possible and the transfer of all activities from Chicago to Argonne should be completed in two years.

Frank Wagner, Jr. began working in Section C-I today. He has been employed by General Electric as a physicist.

Chisholm relayed to me a request to call Captain Lavender in Berkeley. Kennedy, Wahl, and Segrè are also in Berkeley; and Kennedy, with Wahl at his elbow, participated in the conference call. Lavender said that he, in the name of the Government, would like to deal directly with us inventors, despite the fact that the University of California was not willing to sign a waiver or disclaimer, in order to get on with the negotiations. I agreed to this method of proceeding, provided President Sproul is first given another chance to sign a disclaimer on behalf of the University. Kennedy and Wahl agreed with me. Lavender said he would discuss the matter with Segrè before going to see President Sproul.

I answered Neil Gordon's letter, saying that the photograph was forwarded to him on February 23, but the chart he requested is not yet cleared for publication. I offer him a chart that shows a new form of the periodic table. I replied to a letter, dated February 28, from Paul R. Saunders. I say that neither Hogness nor I will be in town on March 9 or March 10, but Winston Manning, whom he met on his last visit, will be here. I sent a photograph to Miss Genevieve A. Degan, Saint Mary College, Xavier, Kansas; she wrote to say she is collecting photographs of famous scientists for the Science Club of the college.

Helen had a dental appointment this morning at 10:30.

Former Prime Minister Winston Churchill is in Washington enroute to Fulton, Missouri, to give a major foreign policy address, says today's newspaper.

Tuesday, March 5, 1946

Sigfred Peterson is scheduled to be discharged from the SED today. He will remain here on civilian status for another month. See Figures 45-47.

The Heavy Isotopes Group met this morning. I attended the meeting, in addition to Anderson, Bentley, Elson, Erway, Fineman, Florin, Ghiorso, Hagemann, Hindman, Hopkins, Hyde, Jaffey, James, Katzin, Kohman, Magnusson, Manning, Osborne, Peterson, Sedlet, Stewart, Studier, R. Thompson, S. Thompson, and Van Winkle. I asked about the samples Ghiorso and Osborne will take to Argonne and learned that Van Winkle will plate the protactinium purified from uranium today. Ghiorso said that he is leaving tomorrow morning to make the measurements. There are 10^7 c/m of U^{232} in the sample prepared by Britain. This is about 0.1 µg. In order to avoid contamination of the sample it has not been counted. Osborne will prepare Np^{237} and a sample of 0.75 µg of Pu^{238} today.

I described the status of the samples of actinium and its daughters. The spectrographic laboratory has verified that the solid in the thorium fraction is mainly thorium. A second thorium fraction milked from the actinium fraction also appears to have much solid thorium. However, sufficiently large samples, presumably free from carrier, are nearly isolated.

Ghiorso then continued and said that Cunningham has prepared 1 µg of repurified 95^{241} and that the preparation of the 96 sample is underway. He has two Ra^{226} samples of 100 µg which were prepared by Donald Ames.

Kohman, Ghiorso, and I then discussed Canadian results by Pontecorvo and West which have set an upper limit of about 0.05 b on the fission cross section of radium. The results were actually that the cross section could not be more than 10^{-3} times the cross section for fission of natural uranium with radium-beryllium fast neutrons. Ghiorso believes that we could set an upper limit of 0.003 b on the basis of a previous experiment. The main idea of testing Ra^{226} is to see if the sample is free enough from contamination to test the fission of Ra^{227} produced by bombarding it. He added that the samples used for the determination of the neutron capture cross section of Pa^{223} would be rechecked, and future measurements will be made on the mass 210 isotopes in the radium series, and on lead, bismuth, and thallium decay products.

Stan Thompson discussed Cunningham's work on the 95 isolation from the lanthanum-95 mixture from Site Y. There is trouble caused by heavy metals in the sample. There are from 10 to 13 mg of 95 in three fractions. They are now trying to precipitate heavy metals with H_2S. The three fractions will then be combined. Lanthanum hydroxide containing

Figure 45. Sigfred Peterson and Roy Thompson outside New Chem, March 1946.

Figure 46. Burris Cunningham and Stan Thompson outside New Chem, March 1946.

Figure 47. Horace Hopkins and Stan Thompson outside New Chem, March 1946.

element 95 will be isolated pure. After the heavy metals are separated, thorium and plutonium will be separated by precipitation with peroxide. Much thorium is present. This was not expected at first but is reasonable. Early Hanford peroxide precipitates contained metastannic acid, and impurities such as these would have been concentrated in this sample, which is in about 3 to 4 liters of solution. Thompson said that Fried is working on the cruds and volatilizing the residue in a vacuum system by treating it with CCl_4. A white residue which Fried had left contained all the 95, and it apparently had never been very hot (thermally). The distillate was highly colored. The method is not yet satisfactory for handling a large amount of material but is worth trying on the smaller fraction. The goal of this experiment is not La-95 separation but a volatilization of the impurities. Spectrographic analysis fails to reveal a number of elements such as tin. These elements are the troublesome ones in this sample. One early sample showed calcium, thorium, and lanthanum to be the major components. The other fractions are cruds which are insoluble in HCl and HNO_3. Sulfuric acid has been avoided since sulfate solutions would not be easy to work, and lead and calcium would precipitate with sulfate.

Stan Thompson talked about the helium ion bombardment of platinum and said that a target had been leached with dilute nitric acid and carried through a lanthanum hydroxide and a LaF_3 precipitation. There was a thick plate made, containing much alpha-particle activity with range approximately that of polonium. Metastannic acid was formed. A second layer from the target dissolved in aqua regia contained alpha-particle activity and probably no solder. This was carried through a lanthanum hydroxide and LaF_3 precipitation, a hexone extraction, and another LaF_3 precipitation. Since this sample contains 80 percent of the original alpha-particle activity, the alpha activity follows hexone. There are at least two peaks, the longest range is that of polonium. Since the samples were not thin, there may be a third peak. Ghiorso remarked that he could not give good range values since there are few counts and thick samples. The activity from the solder is the same as that of polonium. The gold fraction contains two to three peaks with polonium in the middle. Polonium could arise from lead in the solder and bismuth impurity in platinum. Thompson said that the alpha-particle activity to Geiger activity ratio differs between the two fractions by a factor of 3 to 4. He could not believe that a fourth of the solder could have been in the platinum fraction. He observed no decay.

Hyde reported that after Cunningham and Thompson had removed their samples, he mounted the target in paraffin to cover the solder and then leached it with aqua regia. The chemical separation was used to find mercury and gold activity but only 1,000-2,000 c/m of alpha-particle activity was found. There were no samples satisfactory enough to give a good count or range analysis. The Geiger activity is volatile and contaminates counters, and it is mostly mercury. If chloride is present, the activity is more volatile than would be expected for $HgCl_2$. There was no alpha-particle activity in the mercury fraction. The volatilization must have been at room temperature. Kohman asked why the $HgCl_2$ did not volatilize under the heat lamp, and Hyde explained that it was under the heat lamp only 30-40 seconds. He went on to say that a sulfide was finally precipitated on the plate but not ignited. The precipitation was from neutral or alkaline solution. The whole mercury fraction shows no alpha-particle activity, and the activity did not volatilize from the sulfide samples.

I asked if Sherr's work, in which platinum was bombarded with helium ions at Harvard, has been checked. Anderson volunteered the information that he himself separated activities for Sherr in which mercury was bombarded with neutrons to form platinum and gold activities. Hyde said that in the Harvard helium ion bombardment no chemical separations were made. Activity was also measured without chemical separation merely to identify activities formed in the neutron bombardment.

I commented that such a bombardment should not give as many activities as ours since the helium ions used were only 16 to 18 Mev. I asked how much of the beta activity was in the mercury fraction, and Hyde said over half.

Hindman talked about the neptunium program and said that both experimental work and writing is being done. He is using a Ag-AgCl electrode to study the IV-V oxidation potential and has confirmed the 3.5 power acid dependence. The absorption spectrum of the V oxidation state has been observed between 0.01 and 0.1 molar acid, and there appears to be no hydrolysis. The IV oxidation state should be expected to hydrolyse to the same extent as U(IV). The plan is to study next the absorption spectrum of the IV oxidation state as a function of hydrogen ion concentration. There is a fluoride complex of the IV oxidation state.

Magnusson said that he is working on measurement of the IV-V and V-VI oxidation potentials for neptunium in sulfate solution. The disproportionation of the V state is greater in sulfate solution. The V-VI oxidation potential in sulfate solution is 15 millivolts less negative than in HCl or HNO_3. This is reasonable since Np(VI) is probably complexed by sulfate. He is trying to make the results more quantitative.

Van Winkle reported on the protactinium work and said that he has purified 6 mg more, making a total of 16 mg. There are 30 mg left to purify. He has been working on tests with the new ultra-centrifuge. Some non-extractable protactinium was centrifuged at low speed and a 50,000 molecular weight fraction was taken out and found to contain 1/3 of the protactinium. The remainder was centrifuged at 120,000 rpm and was found to have a sedimentation weight corresponding to a molecular weight of 500. Since the solution was not completely immobilized, the sampling could not have been too good so that the 500 can be taken only as a lower limit. The results indicate roughly that the protactinium is 2/3 dimer and 1/3 higher polymer. A new cell with which values accurate to one percent can be measured is available and should be obtained. It has two compartments--one of 70 λ, the other 50 λ. Van Winkle explained that the data do not preclude the protactinium being a monomer as it is in 8 N HNO_3. He added that in such experiments, unless much electrolyte is present, there would be a charge effect but that should not appear in this experiment. He said that hydration would cause a higher molecular weight but would also cause a greater viscosity. The main conclusion is that the centrifuge can separate simple molecules from solution in the heavy region. Van Winkle, in response to a question about using it for 95-La separations, said that it should give some concentration but not much. He explained that one should get a 2 mm migration in four hours for a simple ion. If analytical methods can detect the change, one can measure a sedimentation of a simple molecule. McBain has tried to centrifuge $HgCl_2$ but could not. However, with the heavier elements and the possibility of measuring radioactivities, it is possible to measure centrifugations of simple molecules. Van Winkle noted that he assumed a density of six in his calculations; to get an absolute molecular weight, it is necessary to know the density of the molecule.

Roy Thompson said that he has measured a variety of solubilities of protactinium in HCl and HNO_3; there is much disagreement in the results. From 0.5 to concentrated HCl the solubility varies from 0.1 to 1.5 mg per ml. The solubility varies linearly with hydrogen ion concentration up to 6 to 8 N and then shoots up. Some rough experiments were done with the centrifuge on the solutions in 10 N HNO_3 in which high solubilities were found. It was impossible to centrifuge out any colloids. The solubilities in HNO_3 are very unpredictable. There are results varying from 1.8 to 10 mg per ml in the same acidity. Glass cones were used with cork stoppers sealed with paraffin. Some attack was observed on the cork possibly due to fumes, but the acid solution turned yellow. It is possible that there is an organic complex increasing the solubility of protactinium. He said that he eventually intends to measure solubility in H_2SO_4. I remarked that it would be good if protactinium is soluble in something besides HF, and Thompson added that concentrated nitric acid plus cork is good. He went on to say that the solubility is sometimes high for protactinium hydroxide precipitated with NH_3; the protactinium precipitates on standing. Thompson continued by saying that he has tested the water wash of a highly soluble protactinium sample and found that the activity could be completely centrifuged out.

Osborne reported on the Pu^{238} made from the neutron bombardment of Np^{237}. There are 6 μg, assuming a half-life of 50 years. There was only a 50 to 60 percent yield after ether extraction and only 25 percent survived the bromate cycles, so the final purified product was 1.5 μg of Pu^{238}. There is also purified Np^{237} recovered from the sample. A sample of 10^7 c/m (0.25 μg) of Pu^{238} was sent to Hanford for neutron irradiation, and the rest will be sent to Argonne tomorrow. Also a sample of 150 μg of pure Np^{237} has been plated for spontaneous fission and slow neutron fission measurements.

Anderson reported that he has prepared a green compound $Rb_4Pu(SO_4)_4 \cdot 2H_2O$ after some difficulty. Analysis shows 35.2 percent rubidium and 23.2 percent plutonium compared with 34.8 and 23.9 calculated for the dihydrate. Zachariasen has examined the corresponding ammonium salt, studying a single crystal, and finds it isomorphous with the cerium compound. The potassium compound analyzed best as a monohydrate, but the analysis is difficult. If the alkali metal and H_2SO_4 concentration are increased, then the precipitate obtained is $Pu(SO_4)_2 \cdot 4H_2O$. Sometimes it can be converted to the double salt. This is the most difficult compound to prepare yet. In one experiment all three solids, plutonium sulfate, rubidium sulfate, and the double salt, were prepared. The first precipitate was washed and redissolved in a rubidium sulfate solution low in sulfuric acid; alcohol was added. A solubility of 7 mg per liter was obtained. A high rubidium to plutonium ratio is all right. This confirms Hindman's result that a plutonium sulfate complex is stable in ½ N sulfate. Plutonium(VI) ferricyanide has also been prepared. Anderson said that sometimes pure compounds can be obtained, sometimes mixtures. The least soluble compound is obtained only if equilibrium is reached.

I asked whether anyone has milked U^{236} from plutonium. Ghiorso replied that Asprey finished it before leaving and that Britain is to plate the sample. Manning then mentioned Kohman's calculation that most of the activity in the sample would be U^{232}, 100 times that of the U^{236}. Kohman asked whether several grams of a low gt plutonium could be obtained as a source of purer U^{235}. James replied that he has 1 g that has been sitting for quite a while; and Stewart said he has 2 g which are partially purified and 1.5 g, purified. Kohman explained that he wants the pure U^{235} to find out how complex its alpha spectrum is. Ghiorso thought that several hundred micrograms of U^{235} would be needed so several grams of plutonium would be necessary to grow it.

I asked Sedlet what methods he plans to use in the protactinium extraction, and Kohman answered that the plan is to extract the protactinium from unaltered ores to determine the U^{235}/U^{236} ratio. Another way is to isolate a radium fraction or a thorium fraction from the ore and use the pulse analyzer to compare the activities from the two series. This would be easier with the thorium isotopes. However, the Ra^{223} daughter of Th^{227} from the actinium series would interfere.

Peterson suggested that purification of the thorium from radium by iodate precipitation would easily solve this difficulty, that a similar chemistry was used in an experiment to determine the half-life of Th^{227}, alias RdAc. The sample is mixed with Th^{230}, alias ionium, with some Th^{232} as carrier. A portion of this solution is purified every few days by repeated iodate precipitation, and the decay of the ratio of the two peaks is followed. The data are fitted best by a 18.6 day half-life. The literature value, 18.9 days, was found by analyzing complicated growth and decay growth and probably is not as accurate. Pressed for an estimate of probable error in this new value, Peterson suggested 0.1 day but was criticized by Katzin who did not believe the data that accurate and suggested a least squares analysis. (Note: Examination of the graph after the meeting shows 0.2 day may be a better probable error.) In regard to use of least squares, Peterson mentioned a private communication from Studier on the half-life of U^{230}. Studier had drawn a straight line through his points on the decay curve and obtained a half-life of 20.77 days. Then by spending two days doing a least squares analysis, he obtained a value of 20.77 days. Sedlet said that another method he plans to follow will be to separate protactinium from uranium, let it grow in again, separate it, and find out how much has grown in.

Studier announced that Em^{218} has a half-life of approximately 1 to 2 hundredths of a second. He used an oscilloscope to determine this. Kohman then asked what is to be used as the name of element 86, and Manning and I said that probably "emanation" is the best. There was a general discussion of the name of this element.

I suggested that, while the naming of elements was under discussion, the group consider the names "amerium" and "curium" for element 95 and 96. There was considerable objection to "amerium" for 95. I said I believe that the name amerium is good as an analogy to europium for element 63, and that 96 is named for a person analogously to element 64, gadolinium. Kohman offered that if element 95 were called "americum," its oxide would be called "america," and Peterson questioned the name for the plus 2 state of element 96 on the basis of the new name for the element. I replied that this was an unimportant question since that state should not be expected to exist for element 96. It would, indeed, be "curi-ous."

Anderson questioned the naming of oxidation states of plutonium. It was agreed that it is better to use just the roman numerals rather than naming the oxidation states. Hyde added that protactinium is another poorly named element. It was also brought out that thoron was discovered before radon. One objection to the name amerium is that it gives a first impression of sounding nationalistic. It was generally agreed that it should be interpreted as referring to both North and South America with the possible exception of Argentina. At this point I recommended naming element 95 "herperium" for the Western Hemisphere. [Subsequently, we agreed to call element 95 "americium" with a soft "c" and element 96 "curium."] There were then some questions as to the name of element 97. However, the lack of 97 precluded any conclusions.

Ghiorso asked about the origin of the samples for pulse analysis from Werner in Berkeley, and I replied that they were from the 95-96 separation. Ghiorso remarked that they were difficult to run because they had few counts of alpha particles and lots of beta activity. James commented that the tracer used in Berkeley was sent there before we knew how to decontaminate it. Stan Thompson added that the TTA method of separation was used, and he has a memo from Werner on the use of TTA to separate 95 and lanthanum.

I wrote a recommendation to T. R. Hogness for Larry B. Magnusson, who is applying for an ACS Predoctoral Fellowship. Magnusson, I say, is one of the most knowledgable men in the world about neptunium chemistry. He, with T. J. La Chapelle, was the first to isolate neptunium in the pure state. He is pleasant, trustworthy, and conscientious and one of the best young scientists I have ever known.

I received and read a copy of a letter from Cunningham to Perlman, enclosing a list of items whose transfer from Chicago should be ordered by the Radiation Laboratory. Cunningham concludes his letter by saying, "The thought that we have only two months more to live in Chicago is a source of unfailing joy."

At 2:00 p.m. I met with Roy Thompson, Al Florin, Quentin Van Winkle, Darrell Osborne, and Winston Manning to outline a program for investigating the chemical properties of protactinium. It was decided that Florin will attempt the preparation of PaF_5 after practicing on the preparation of TaF_5; Osborne will attempt the preparation of protactinium chloride after practicing with tantalum. If these experiments are successful, Osborne will try to determine the melting point, attempt to reduce it to the IV oxidation state with hydrogen, and then try to prepare the metal. Work on dry methods for extraction of protactinium from uranium ores will be postponed. Thompson will continue to measure solubilities and hydrolysis of protactinium compounds. He will also undertake absorption spectrophotometry and attempt both electrolytic reduction and polargraphic reduction of protactinium(V) in solution. Van Winkle will continue the sedimentation studies.

I then wrote a memo to Daniels, saying that I have heard that A. V. Grosse has about 500 mg of protactinium that has remained inactive for the last five or ten years. I request that Daniels arrange to purchase this material for us to use in our investigations.

MUC-FD-L-86, "Metallurgical Laboratory Report for February 1946," was issued today.

In the Summary of Organization, it is noted that during General Nichols' visit on February 11 it was generally agreed that after June 30 the Metallurgical Laboratory, including buildings, equipment, and most of the personnel, will be reconstituted as a regional laboratory which will carry on the research program along the lines being followed by the Metallurgical Laboratory. Following the conference with General

Nichols, a complete program of research on the fast-fission pile and the experimental high-temperature pile was drawn up and submitted in MUC-FD-L-182. A research program for the proposed regional laboratory covering the period from July 1, 1946, to July 1, 1947 was drawn up after discussion with and recommendations of the division directors. This program, MUC-FD-L-185, has been submitted also to General Nichols and his Advisory Committee.

Colonel Ruhoff and Major Donnell visited the Metallurgical Laboratory to discuss the proposed plans for declassification. The work for the Plutonium Project Record is continuing at an accelerating rate.

The tritium program is getting under way. Three cans of LiF prepared at Site Y were successfully sealed and made ready for irradiation in the Hanford pile.

The summary of work of Section C-I is that submitted by Manning to Daniels (MUC-GTS-2208) on February 21.

Section C-II's report includes a summary of the work on the High-Temperature Pile Program and a summary of the analytical services.

The Health Division reports that more than 50 percent of the academic group is involved with writing terminal reports. It also reports a spill of 10 mg of plutonium that occurred during February, but comments that contamination was not serious. The receipt of a relatively large amount of plutonium by the Chemistry Division has thus far produced no serious health hazard.

Among other items, the Physics and Metallurgy Division reports new results on compounds of the transuranium elements. This information was elaborated upon in Zachariasen's report in the Physics and Metallurgy report for February 1946 (CP-3445), and this report includes summaries of studies by Sherman Fried on $NpOCl_2$, of work on the $KF-UF_4$, $KF-ThF_4$, and $KF-LaF_3$ systems by Zachariasen, and of results of the analysis of the sample of Cs_2PuCl_6 prepared by H. H. Anderson.

The percent of effort devoted to various problems this month is as follows:

		Chemistry	% Effort February
C-I	1	Nuclear and chemical properties of the heavy elements	16
C-I	2	Control analysis of uranium ore	6
C-I	3	Recovery of product	3
C-I	4	Redox solvent-extraction process	17

		Chemistry	% Effort February
C-II	1	High-temperature piles	14
C-II	2	Radiation chemistry	3
C-II	3	Fission-product studies	3
C-II	4	Analytical chemistry	2
		Writing for Plutonium Project Record	37

		Health	% Effort February
H-I	1	Clinical laboratory examinations	5
H-I	2	Uranium toxicity	1
H-I	3	Studies of incipient radiation damage	2
H-I	4	Effects of irradiation and heavy metals on proteins	1
H-I	5	Health service for Metallurgical Laboratory	3
H-II	1	Development and application of radiation inhalation techniques	1
H-II	2	Preparation of and metabolism of radioactive materials	2
H-II	3	Studies of effects of external radiation	1
H-II	4	The effects of radiations on the hemopoietic system	1
H-II	5	The histological effects of radiations	3
H-II	6	The physiological effects of radiations	2
H-II	7	The chronic effects of radiations (tumors, aging, etc.)	3
H-II	8	Maintenance of animal farm	11
H-II	9	Biostatistics	1
H-II	10	Instrument development	1

		Health	% Effort February
H-III	1	Industrial hazards (monitoring)	1
H-III	2	Monitoring of human excreta	3
H-III	3	Health-physics and instrument development	3
H-III	4	Monitoring of personnel	1
		Writing for Plutonium Project Record	55

		Physics and Metallurgy	% Effort February
PM-I	1	Mass spectrometry	17
PM-II	1	X-ray studies of crystal structure	8
PM-III	1	Casting and studies of liquid metal	20
PM-III	2	Metallography	15
PM-III	3	Fabrication of beryllium and other metals	15
		Writing for Plutonium Project Record	25

The distribution of Metallurgical Laboratory Employees as of February 28, 1946 is as follows:

	SED	Academic	Non-Academic	Total
Argonne Laboratory	7	33	11	44
Chemistry	19	87	24	111
Health	3	72	118	190
Physics and Metallurgy	1	16	10	26
Opacity	6	4	1	5
Services and Development	4	23	189	212
Scientific Administration	1	4	4	8
Information	1	14	16	30

	SED	Academic	Non-Academic	Total
Patents	1	4	13	17
Associated Sites (Manhattan District Advisor, Evergreen)	—	2	2	4
	43	259	388	647
Safety and Security				138
Administration				372
				1157*

*Including 43 SED

At 6:00 p.m. I left Chicago on the "City of San Francisco" to visit Berkeley. Frances Chilson is spending the night with Helen.

According to today's paper, Canada announced yesterday that four Canadian spies and several Soviet embassy staff members have been named as a part of a spy ring. The Soviets are believed to have all left Canada.

Wednesday, March 6, 1946

Enroute to Berkeley on the "City of San Francisco."

Helen is scheduled to attend class at Lying-In Hospital. She plans to have lunch with Wilma and spend the night with Marjorie Osborne.

A newspaper I picked up at a train stop reports on Churchill's Fulton, Missouri, speech. The loudest criticism comes from the most avid New Dealers, who described the speech as too imperialistic for the present times. Churchill said that the Soviet Union is behind an "iron curtain."

Thursday, March 7, 1946

I arrived in Berkeley and checked in at the Durant Hotel where I will stay while I am in Berkeley. I then went to the Radiation Laboratory.

In Chicago the extensive report, "Feasibility and Methods of Inspection of World for Plutonium and Other Power Reactors, and for Chemical Processing Plants Associated with Reactors," which L. I. Katzin has been editing is being submitted today.

This report was prepared for the Committee on Technical Phases of Inspection and Control of Atomic Energy by L. B. Borst, H. S. Brown, C. V. Cannon, W. E. Cohn, C. D. Coryell, S. G. English, J. R. Gilbreath, H. Hyman, A. H. Jaffey, L. I. Katzin, D. E. Koshland, Jr., R. N. Lyon, D. R. Miller, K. Z. Morgan, M. D. Peterson, D. Saxon, R. Scalettar, G. T. Seaborg, H. Soodak, R. W. Stoughton, A. Wattenberg, and A. Weinberg. The appendices were prepared by W. Blaedel, J. R. Gilbreath, H. Hyman, A. H. Jaffey, and L. I. Katzin.

The outline of this 126-page report is as follows:

I. Introduction

  A. Assumptions
    1. Freedom of access
    2. World organization
    3. Premise on time scale

  B. Description of contents

II. Control of legal chain reacting piles

  A. Nature of the pile
    1. Chain reaction
    2. Purposes of piles
    3. General features of piles
    4. Classification of pile types
    5. Pile operation

  B. Problems of fissionable material control at the pile
    1. Evasion of controls
      a. Types of evasion
      b. Methods of detection of evasions
        (1) Immediate detection
        (2) Post facto detection
      c. Special problems
      d. Conclusions
    2. Denaturation as a control device
      a. General aspects of denaturing fissionable materials
      b. Uranium denaturation
      c. Plutonium denaturation
    3. Summary

III. Detection of illegal reacting piles

  A. General problem

B. Location sites of piles
   1. Air and water
   2. Land

C. Modes of detection of piles during development and construction
   1. Essential materials
      a. Uranium
      b. Moderators
         (1) Graphite
         (2) Heavy water
         (3) Beryllium oxide and metal
   2. Cooling system
      a. Easy methods of water and air cooling
      b. Recirculating cooling systems
      c. Equipment for cooling systems
   3. Instruments
   4. Shielding
   5. Provisions for handling irradiated material
   6. Construction work

D. Detection of illegal operation of a pile
   1. Detection by visual methods
   2. Detection by external radioactivity
   3. Detection by pile radiations
   4. Detection by characteristic parts
   5. Detection by contamination

IV. Control of chemical processing plants for materials from legal piles

A. Nature and purpose of such plants
   1. Separation of small amounts of fissionable materials from large bulks of other materials (e.g., plutonium from uranium)
      a. Precipitation plant at Hanford (diagrams of the Clinton Plant and the Clinton or Hanford Process are attached)
      b. Separation processes other than precipitation
   2. Reprocessing of pile materials

B. Control at legal plants
   1. Introduction - purpose
   2. Supervision of construction of plants
   3. Prevention of diversion in the operation of the chemical separation plant
      a. Control through material accountability
         (1) Control of "first stage" plants
         (2) Control of breeding stage plants (including plants for power piles)
         (3) Evaluation of methods
      b. Control through checks at critical points

4. Prevention of diversion of material in re-fabrication plants.
5. Control analyses

V. Illegal chemical separation plants

A. Conditions and problems involved

B. Detection of an illegal processing plant during period of development and construction
   1. Detection of illegal development activities
   2. Control and inspection during design and construction

C. Detection of illegal separation plants during operation
   1. Chemical aspects
      a. Radioactivity
      b. Gas disposal
      c. Disposal of waste solutions
      d. Consumption of special chemicals
   2. Detection through personnel
   3. Detection through external appearances of the plant
   4. Conclusions

VI. System of inspection and control

A. Methods of operating plants
   1. Plants nationally owned and nationally operated
   2. Plants nationally owned and nationally operated but having international personnel in certain supervisory operational positions
   3. Plants nationally owned and internationally operated
   4. Plants internationally owned and internationally operated

B. IAIC laboratories
   1. Analytical
   2. Research
   3. Development
   4. Inspection

C. Activities in the field inspection unit
   1. Reports from national governments
   2. Control of legal separation plants and piles and critical materials
   3. Detection of illegal plants
   4. Summary of suggested things for inspectors to look for
      a. Seeking illegal piles or associated chemical plants
      b. Seeking illegal diversion from legal plants
   5. Roster of technical personnel

D. Inspection personnel
   1. Their job - to check both legal and illegal plant operation
   2. Possible organizational structure of an inspection commission
   3. What must inspectors know
   4. Quality of men required
   5. Where and how to get inspectors
   6. Number of men required

VII. The role of scientists and engineers in atomic energy inspection and control

A. Introduction

B. Census of scientists

C. Scientists as inspectors
   1. Exchange of scientists and students
   2. The scientist inspection service

D. General remarks

The introduction reads: For the purpose of compiling technological information relating to the feasibility of world inspection and control of atomic energy, it is obviously necessary to make certain assumptions as to conditions which will be obtained under the circumstances of operation of such a system of inspection and control.

The following assumptions were made:

1. Freedom of access. In the following pages, it has been assumed that the inspectors of such an International Commission as may be established will have complete freedom of access to persons, places, and documents within any and all nations. If the Commission is to be informed of the good intentions of the individual nations, upon which must rest the peaceful action of such a nation, it must have assurance that there is no violation of the conditions of control imposed. For this purpose, agents of the International Atomic Inspection Commission (hereafter designated IAIC) must necessarily have broad powers. It must be possible for such agents and inspectors to approach and question any person. They must have access to any region of the country and to any document they wish to see. Some restrictions probably will necessarily be imposed upon the inspector to prevent detailed investigation of situations not involving atomic energy control. It will probably be necessary to set up a Court of Appeal to show that an inspector has just cause for investigation and to establish, among these men, a code of ethics such as exists in the present postal inspection system in this country, to safeguard industrial information.

In addition, there must be free, uncensored and immediate communication between the individual Area Inspection Organizations and the International Commission. There must also be complete freedom of communication between members of the Area Inspection Group. Even a delay in communication would be dangerous for the individuals involved and might easily jeopardize the effectiveness of the inspection group.

2. World organization. A strong world organization to delegate power and authority to the IAIC is a vital assumption if one is to avoid talking in a vacuum. To describe a system of inspection and control which has no mechanism for enforcement would be fruitless. An adverse report from the Commission must result in immediate, concrete and adequate action.

3. Premise on time scale for initial functioning of IAIC. It has been assumed that before such a Commission will have been established and can effectively begin functioning, other nations will have had sufficient time to complete research and development to a point where the construction and operation of atomic reactors and plants is a reality. It is felt that this is possibly a pessimistic but realistic viewpoint. It would obviously facilitate greatly the functioning of the Commission if controls could be established before the existence of plants and materials involved in atomic energy was widespread, but it seems best to assume the converse.

   The need for haste in the initiation of a system of inspection is thus emphasized.

The material for this report has been collected primarily from the knowledge directly at the disposal of various individuals of the Metallurgical and Clinton Laboratories staffs. The hasty preparation of the present document has precluded an exhaustive search of project literature of satisfactory tests of some of the technical methods discussed. The authors have been intimately associated with the development of the whole plutonium process and serve effectively as experts within limited fields. The opinions expressed are primarily those of the authors, since little opportunity has been available for a thorough discussion of the report. No attempt has been made to give references or acknowledgments of sources of particular ideas or discussion. In many cases no such reference is known. The source was often a discussion group meeting for a quite different purpose. While poorly documented, the technical material discussed rests on a secure foundation of experimental fact and theory.

The intent has been to present in Sections II, III, IV, and V a description of existing and proposed installations and to outline technical methods of control and inspection, indicating in the discussion the limits of sensitivity of the methods and, in a general way, the circumstances under which they would be most effective. The processes and equipment described are presented from the point of view of inspection and control. No attempt has been made to make the descriptions and discussions definitive. Section VI discusses the less technical aspects of control, such as organization of the IAIC, qualifications of personnel, and aspects of a nation's industry uniquely adapted to control. Section VII discusses the control of scientists and the exchange of scientists and students as a means of inspection.

In the original plan of this report, a section summarizing the conclusions and data presented was intended. Insufficient time for organization and discussion of such an important part has prevented its inclusion in the present document.

In the section on "Problems of fissionable material control at the pile" (Section IIB), the report states: We inquire into the possibilities of denaturing fissionable species. There are two ways: One is based on the fact that a uranium bomb is impossible to construct unless the light isotope is present to at least 15 percent. The other is based on the fact that a plutonium bomb is very difficult to make if the Pu^{240} content exceeds, say 5 percent. We can therefore tentatively establish the following code:

1. It is unlawful to possess uranium in which the U^{235} or U^{233} content exceeds, say 25 percent. (The figure 25 percent is deemed safe since 15 percent would give an infinitely large bomb--25 percent would still require a bomb of impractical size.)
2. It is unlawful to possess plutonium in which the Pu^{240} content is less than 5 percent or in which the spontaneous neutron background is less than that corresponding to this amount of Pu^{240}.

How could this code be implemented, and what bearing would its enactment have on the future development of nuclear power?

The final section, "The role of scientists and engineers in atomic energy inspection and control" (Section VII) is quoted here in its entirety:

Probably one of the most potent weapons in an inspection and control system designed to prevent extra-legal applications of atomic energy will be the use of scientists and technical men as observers and inspectors and the converse procedure of a rigid and intelligent check on their activities. The field of atomic energy is in its infancy and, at a very minimum, one may contemplate a decade of intense research and development work which will necessitate the utilization of a substantial portion of the technical personnel of any state. This is even true in the most highly advanced nation, the United States, although here a special case exists because of our possession of already completed atomic bombs. It is therefore necessary to emphasize the urgency of applying as soon as possible a general inspection and control system, for the efficacy of such a system will obviously decrease rapidly as the possibility of concealment of finished atomic bombs becomes greater. As a first step in the application of the plan herein envisioned it will be necessary to consider only scientists (i.e., physicists, chemists, and mathematicians) and the more highly trained engineers; later as procedures become standaridized it will be necessary to draw into the scheme the nuclear engineers of the future.

The problem of the use of scientists in inspection and control divides itself into two parts. Each of the technical schemes devised for detection of extra-legal activity such as, for example, detection of radioactivity in the atmosphere and water will require a certain quota of scientific personnel. The number and training of the men required for these activities will vary from scheme to scheme and must therefore be given careful separate consideration in each case. The discussion of these questions will not be considered to fall within the scope of this report. We shall attack the inspection and control problem by considering how scientists, as workers in their field, may be used as inspectors and how illegal activity may be detected by a constant survey of the activities of all scientists.

Census of scientists. The first step must be a complete census of all scientific personnel in all states. This must include physicists, chemists, metallurgists, and mathematicians in the first instance and as soon thereafter as possible all engineers beginning first with those of the highest technical training. There are three aspects to the problem of the census. First there is the formal procedure of checking and accounting for the technical members of the graduating classes of all universities since 1900 and in addition members of the scientific staffs of universities, government, and private research institutions and professional societies. However, one must not be deluded into believing that a formal survey is sufficient. A second and most important step will be the formation of a panel of prominent scientists of all nations whose specific duty it is to single out the most able members of their professions in each nation and to ensure that these latter are given the highest priority in the control plan to be outlined below. Thus, it might lead to major tragedy if just a few topflight physicists and chemists in any nation were able to evade control long enough to permit conspiracy by some government to disobey the regulations of the Atomic Energy Commission. The best safeguard against this is not a formal census, although the latter must certainly be carried out with the utmost rigor and care, but rather to ask professional chemists and physicists for whom in each country it is most important to account. The third problem arises from the fact that during the past half-decade of war a number of young men of outstanding ability may have arisen in each nation who have not yet attained national much less international prominence. It is true that the probability that in any given state a sufficient number of these will exist to constitute a serious danger is not very large. However, the formal census must be particularly careful in checking the records and secret war reports of the past five or six years so as to account for all such men. This may be difficult in war-ravaged countries; one can only say that the utmost intelligence must be used to counteract possible evasion and then hope that the compound probability of an outstanding "unknown" both existing and escaping detection be small enough to make the danger from this source negligible.

Scientists as inspectors. Given the results of the above census, the Atomic Energy Commission must undertake the control of the activities of scientists along two separate and complementary lines. The first is a relatively informal procedure based on exchange of scientists and students. The second is the establishment of a formal roving inspector system combined with the requirement of reports from each scientist on the nature of his work.

Exchange of scientists and students. The scientists are the only ones who can possibly realize fully the extent to which an exchange of scientists and students would prevent any extra-legal activity by a nation. To place competent scientific workers from other nations in all the key university, government, and industrial research centers of a given state would obviously provide a great checking system. It is clear that a necessary condition would be that these men actually engage in research and development work during their tenure at the laboratory for it is only so that an intimate knowledge of prevailing conditions can be obtained. It has been the experience of workers on the Metallurgical Project that, despite strict secrecy and compartmentalization regulations, it is virtually impossible to keep secret major developments from fellow scientists (in many cases even with the barrier of great geographical isolation). One need not enter into the scientific and psychological reasons for this fact to recognize its potency as an instrument of inspection and control. The problem will be even simpler when it will in fact be illegal to have formal secrecy or compartmentalization regulations. It remains true that policy decisions can be kept secret if in the hands of few enough men but even they must in time descend into the realm of action and it is then that an attempt at secrecy will in all probability fail.

The problem of obtaining adequate manpower for this job is not insuperable. There are many young Ph.D.'s in the sciences who would welcome an opportunity for research in the laboratories of another country. Moreover the scientists having shown themselves to be acutely aware of the dangers of uncontrolled atomic energy should and probably will welcome the opportunity to serve even if in some cases this will involve a sacrifice. It is recommended that if a voluntary system fails it be made the law that all young scientists within one to two years after completion of their doctorate be required to spend a year in service at some foreign research or development center unless it can be proved that extraordinary hardship will be worked by so doing. To increase the manpower available a voluntary system using predoctoral students in the sciences and even bright undergraduates for exchange purposes might be adopted. It should be required that semiannual reports be submitted by these scientific workers in foreign lands to the Atomic Energy Commission and that of course any suspicious developments be reported immediately.

The initial establishment of such a system will of necessity involve a somewhat different procedure than that obtained when a steady state has been reached. It may be necessary to use some of the older scientific workers as well as the younger at first. It is believed that a voluntary system would provide a substantial part of of the manpower required. In any case the urgency of the problem is so great that even if a degree of compulsion is necessary it must be adopted.

The scientists inspection service. In addition to the informal arrangement described above it is believed essential that a formal check be maintained on the activiites of all scientists. The Atomic Energy Commission must establish a Scientific Review Board, whose members are intelligent competent scientists in their own right, the duty of which board will be the review of reports by all scientists on their activities. Scientists must be required to submit semiannual reports of their work to the Scientific Review Board which shall then examine them as to their validity on the basis of coherence, estimated time necessary for accomplishment, and such other criteria as may seem reasonable. In addition it shall be the duty of the Scientific Review Board to examine reports in the aggregate to determine noticeable trends or decreases in apparent productivity which may be viewed with suspicion. Examination of professional journals by trained men should be undertaken with the same ends in view. Under the Scientific Review Board should be established a Roving Inspector Service whose duty will be to make spot examinations of the activities of various scientists (with the emphasis of course on the most capable) to determine with certainty that they are in fact engaged in the work at the place stated in the reports. Suspicious circumstances must be reported immediately. The manpower for the Scientific Review Board and Roving Inspector Service will in the opinion of the writer be more difficult to obtain voluntarily for any appreciable length of time if it involves a much more complete divorcement from research work than the informal service described in IIIA. This must be compensated for by high salaries and relatively short tenure, the latter being necessary for other obvious reasons also. It is recommended in addition that in our education system specific training be given for the work of inspector. (This will be necessary for other phases of the inspection program also).

General remarks. It is clear that the program herein outlined will entail an appreciable sacrifice in the scientific life of every scientist. It may, if administered unintelligently, place an intolerable burden on his personal existence. It will probably mean, at least in the beginning, a slowing down of the rate of scientific progress due to the necessary devotion of time to other activities. For these reasons and mainly because the inspection and control system is necessarily within the domain of the scientists it is essential this portion of the work of the Atomic Energy Commission be administered by scientists elected at periodic intervals by all the scientists of the world, this Inspection and Control Council being of course responsible to the Commission. It would seem that only so can one have a reasonable assurance that the life of the scientists would not be made intolerable. The scientist in spite of this will bear a great additional burden. However, he must now finally recognize that coordinate in importance to his devotion to science is his responsibility to his fellow men.

"Uncover Spy Ring at Atom Plant" reads the headline in today's Berkeley Gazette. The chairman of the House Un-American Activities Committee claims they have discovered a foreign spy ring operating between New York and Oak Ridge.

Friday, March 8, 1946

In Berkeley. At the laboratory Emilio Segrè and I dictated a letter to Colonel K. E. Fields in Washington, asking that he confirm our judgment that the paper, "Properties of 94^{239}," (A-33) by Seaborg, Segrè, Kennedy, and Lawrence contains no material detrimental to national defense. We explain that the paper was submitted, but withheld from publication, in 1941; however, John T. Tate, editor of Physical Review, has now contacted us about its publication.

I talked with Ruth Rogers by phone today. She read me a letter dated March 5, from Ted La Chapelle, who is disturbed by his offer from Everson--$300 per month for full-time work with no compensation for travel and moving expenses. He feels that he will not be able to save enough to start graduate school.

I dictated a letter to Ted for Ruth to sign. I say that I am not in a position to make offers, but I suggest that Ted write to Everson and say that he expects to work full time for six months or a year. Then, it seems likely that Everson will make a different offer to him.

Churchill spoke in Richmond, Virgina, today appealing again for a close union of English-speaking peoples. He spoke before a joint session of the Virginia General Assembly, the oldest law-making body in the western hemisphere.

Saturday, March 9, 1946

In Berkeley. I spent the day with my group at the Radiation Laboratory. I received by mail from Ruth Rogers, a telegram from Erle M. Billings, secretary of ACS, asking that, at Noyes' request, I accept membership in the Inorganic Chemistry Panel to discuss and review ACS's standards and procedures for evaluating graduate training. The group will meet in Atlantic City on April 5.

In Chicago Robert Goeckermann is scheduled to terminate. He will go on the Radiation Laboratory payroll about April 1.

The Northern California Association of Scientists is urging public support of the McMahon Bill, which emphasizes civilian control of atomic energy, according to today's Berkeley Gazette.

Sunday, March 10, 1946

I spent part of the day in conversations with some of the Berkeley people. I then met and visited with Jeanette.

Monday, March 11, 1946

At the Berkeley Radiation Laboratory.

A letter arrived for me from Paul Nahin of Union Oil Company at Wilmington, California. He again mentions the possibility of my speaking to the local ACS section at some indefinite future date. He notes that he has suggested to Union Oil Company the desirability of making research grants available to such institutions as Cal Tech and UC.

I left Berkeley in the evening to return to Chicago via the "City of San Francisco."

Today's Berkeley Gazette carries an item titled, "LeMay Urges Research to Keep Peace." General LeMay sees almost boundless opportunities for technical development and says, "I expect to live to see the day when a rocket vehicle can be sent to the moon and back."

Tuesday, March 12, 1946

Enroute to Chicago on the "City of San Francisco."

Wednesday, March 13, 1946

When I arrived in Chicago at 12;15 p.m. Helen met me and summarized her week's activities. She spent last Tuesday night at the Osbornes' and stayed with Marjorie until after lunch on Wednesday. Wilma stopped in to see her later in the afternoon, and then Frances Chilson and Helen had dinner at Tiffin's. On Friday at 1:00 p.m. Helen had another dental appointment; she then went to visit Wilma and stayed for dinner. At 2:30 p.m. on Monday Helen had still another dental appointment; afterwards she went shopping. Yesterday she had an appointment with Dr. Davis at 2:00 p.m.

Back at the laboratory I found the following had taken place while I was away:

Wednesday, March 6

Stan Thompson left a report for me to read, describing the Project and his role in it which he has prepared for a Standard Oil Company of California publication.

Thursday, March 7

Don Stewart sent T. S. Chapman drawings of slugs of our Hanford irradiation, which he asks Chapman to transmit to the Area Engineer at Hanford.

A very nice letter arrived for me from Norman Kharasch, thanking me for the talk to the Chicago Section of the ACS on February 15. He notes that it was a new record in size for a Chicago Section meeting.

Friday, March 8

Uncle Henry Seaborg recently asked me to speak in Ishpeming to the Marquette Range Engineers Club on April 1, and I agreed. A letter arrived from Peter P. Ribotto, Secretary of the club, acknowledging the fact that I have accepted Uncle Henry's invitation. The meeting will be held at the Mather Inn, and he invited me to be a guest of Mr. Carl Brewer at dinner before the talk.

Saturday, March 9

Report CS-3446, "Chemistry Division Summary Report for February, 1946," was issued. All of the C-I material was contained in Manning's

Report to Hogness of February 21 (MUC-GTS-2208). Section C-II, in a report on the High Temperature Pile, states that the Norton Company of Niagara Falls, Ontario, has succeeded in producing hexagonal beryllium oxide moderator bricks with density greater than 2.9. Other developmental work has been carried out. The Battelle Memorial Institute has submitted a tabulation of data on the performance of several metals at high temperatures. A survey of the literature with regard to the diffusion of helium through metals at high temperatures indicates that probably no difficulty will be encountered in this regard. Continued spectrographic and chemical analyses are being made on BeO powder and samples of BeO shapes as supplied by the manufacturer. The results of spectrographic analyses, chemical analyses, and neutron absorption analyses are reasonably consistent.

Further work on the design of the high temperature oxide pile has led to the proposal that the helium coolant be circulated downward rather than upward through the annular spaces around the fuel rods. Such a design will allow for the installation of charging equipment and control rods equipment at the top of the pile at a relatively low temperature.

Under "Special Problems," it is reported that during the month four sets of samples having different bombardments were received from HEW on which stored energy could be determined by measuring the heats of combustion.

Elastic modulus measurements have been made on BeO rods, and measurements of the thermal expansion of BeO from room temperature to about 900°C are underway. Measurements of thermal conductivity at high temperatures by the thin wafer technique are being continued. Work on acid formation in P-9 moderator is continuing. BeO and UO_2 mixture samples which were exposed to study UO_2 conversion to U_3O_8 have been removed from the Argonne pile and are now cooling to permit analysis by the Analytical Group.

The fission yield of 49 h Zn^{72} in plutonium has been determined to be of the order of 10^{-4} percent, about ten times higher than the corresponding yield in uranium. This interesting result shows that the base of the light wing of the plutonium fission yield curve is wider than the corresponding wing of the uranium fission yield curve.

The Analytical Group reports that an investigation was made of the accuracy of the determination of columbium in U-Cb alloys. Synthetic samples containing 6 to 75 percent columbium were analyzed with an accuracy of ±0.5 percent.

Spectrographic Analysis reports work is in progress on the preparation of pure beryllium oxide for use in preparing new standards. Preliminary tests indicate that repeated distillation of beryllium basic acetate gives a much purer product that the extraction of the chloroform solution of the same salt. Spectrograms of two larger samples of element 95 indicate that the tentative assignment of the lines of this element obtained from earlier plates was correct.

Don Stewart requested that Chapman arrange for helium ion bombardments of an unused platinum disc and one carrying 2 mg of lanthanum. The purpose of these bombardments is to help us interpret some puzzling activities obtained in recent studies.

Tuesday, March 12

The Heavy Isotopes Group meeting was attended by Ames, Bentley, Britain, Elson, Erway, Fineman, Florin, Ghiorso, Hindman, Hyde, Jaffey, James, Katzin, Kohman, Manning, Peterson, Sedlet, Stewart, R. Thompson, S. Thompson, Van Winkle, Warshaw, and Weissbourd. Manning presided at the meeting in my absence.

Stan Thompson spoke first on the progress of the 95^{241} isolation, on which Hopkins, Cunningham, Stewart, Sedlet, Elson, and he are working. He reported that the situation does not look so good this week. They have been working up cruds. About 5 mg of 95^{241} are now in clean solutions of small volume ready for fluosilicate cycles. Calcium, apparently not removed adequately by the first ammonia precipitations, seems to be giving the most trouble. Anions such as borates, phosphates, or arsenates present may have aggravated this trouble. The original 55 gal solution was 2 M in calcium nitrate and contained about 100 g of plutonium, 15 mg of 95^{241}, and probably some ammonium nitrate. About 100 g each of iron and sulfide insoluble heavy metals, including probably lead and tin, 10 to 20 g of lanthanum, some thorium and silicon, and maybe a little chromium seemed to be present. There was no evidence of copper or nickel. Bismuth, added as a scavenger in the procedure, is also present in later solutions. The first step involved precipitation with ammonia. This was repeated only once but probably should have been repeated as many as five times to remove calcium completely. This step was followed by dichromate oxidation and lanthanum fluoride precipitation to separate plutonium. This cycle was repeated and resulted in an insoluble dichromate precipitate during the oxidation. From this point on nearly every fraction, when concentrated, produced cruds that in several cases contained considerable calcium. It probably would have been preferable to precede the lanthanum fluoride precipitation by a sulfide precipitation from 0.5 N nitric acid and then use silver persulfate instead of dichromate for oxidation because some heavy metals give insoluble dichromates in 2 N nitric acid and argentic oxidizes chromium and manganese. The second fluoride precipitate when metathesized should be soluble in nitric acid, provided the above separations are good. Peroxide precipitation would then remove the thorium and plutonium, and the 95^{241} could be purified by fluosilicate cycles or by Werner's TTA procedure. This TTA procedure looks better than the fluosilicate for lanthanum separation.

If one uses equal volumes of TTA and benzene and aqueous phase of pH 3.5, about 90 percent of the 95^{241} and 3 percent of the lanthanum are extracted into the TTA layer. Probably thorium and plutonium and possibly iron would also extract under these conditions, if they are present. The 5 mg of 95^{241}, now ready for fluosilicate cycles, are contained in a volume of about 300 ml. One 10 cc fraction contains 1.5 mg 95^{241} and less than 100 mg of lanthanum. The ratio of 95^{241} to lanthanum is much lower than that in other fractions. It is not known where in the preceding procedures a lanthanum separation was achieved. A total of 12 to 14 mg are presently in various fractions and cruds and is being worked up. Most of it should be recoverable but since it may not all be very pure, we are planning to employ about five fluosilicate cycles which, barring interfering elements, should yield some product that is 90 percent pure 95^{241}. In the absence of interfering substances a 40 to 50 percent yield is expected. We intend to purify all of the 12 to 14 mg at once after testing each fraction for its behavior. A few milligrams of this material may be 90 percent pure by the middle of next week.

Stan Thompson then reported on the lanthanum target received last week. From this target alpha-particle activity was obtained that appears to be the same as that obtained from the platinum bombardment. The alpha-particle activity from the platinum target, however, was carried on lanthanum fluoride and extracted into hexone while the alpha particles from the lanthanum target were not extracted into hexone and not carried by zirconium phosphate after bromate oxidation. The Geiger activity from both the platinum and lanthanum targets did not extract appreciably into hexone, but from the lanthanum target a lot of activity carried on zirconium phosphate. This behavior points to cerium. Some of the Geiger activity has been characterized with half-lives of a few days. The difference in hexone extractability of the alpha particles from the different targets may be explained by a shorter oxidation period used with the alpha emitter from the lanthanum target.

There was some discussion of the feasibility of using bromate oxidation with hexone. Hindman saw no reason why it could not be used since he had often used it in the past, but Stewart pointed out that at high acidities it could not be used very well because of attack on the hexone. Stan Thompson said he is interested in using it at as high an acidity as possible in order to achieve the utmost oxidizing action. About 100 x-ray plus gamma-ray counts were present in the zirconium phosphate precipitate from the lanthanum target. It is not known how much lanthanum was present. Supposedly there were 2 mg, but there did not appear to be that much present when it was received here. In dissolving the material from the target, the technique of immersing the target in paraffin and then leaching only the face of the target with acid was followed. There should be little, if any, activity produced from solder present in the solution.

The suggestion was made that the polonium alpha-particle activities which we are apparently obtaining might arise from a general contamination of the cyclotron with polonium alpha-particle emitters. Stan Thompson then added that this might also arise from lead in the grease used in the cyclotron and that this possibility might be checked by the California people. Pulse analysis of the alpha-particle activities from the lanthanum-platinum bombardments indicate the presence of probably three polonium isotopes. There has been possibly a few percent decay over a period of a week but there are no data yet on the change of ratio of peaks. Kohman thinks it not unlikely that Po^{207} or Po^{208} might have a half-life as long as a year. He recalled that lead and bismuth were bombarded at Berkeley before the war and lots of alpha activity was found but the polonium was not worked up. Manning remarked that the Berkeley people are now bombarding separated lead isotopes.

Ghiorso then reported on the fission test of 95^{241} recently performed at Argonne. A thermal fission cross section of 3.3 b was obtained using a sample repurified by Cunningham. This cross section would be equivalent to the presence of 165 c/m of plutonium contamination. A previous value was 3.6 b, and there was a still earlier value on a very small sample of 0.2 b. The 95^{241}-96^{242} mixutre showed a cross section equivalent to 398 plutonium alpha-particle c/m. If all were due to the 96^{242} present, this would be a cross section of 2,640 b; if all due to 95^{241}, the cross section would be 4.1 b. This 95^{241}-96^{242} mixture was purified by Stan Thompson who says the chemistry followed should have cut the plutonium content down to 0.1 c/m. Old values for 95^{241}-96^{242} mixture, assuming all the fission due to 96^{242}, are 900 and 1,800 b.

Cunningham commented that he is going to give Stewart some more 95^{241} to send to Clinton for manufacture of 96^{242}. Results of the last such bombardment indicate a capture cross section for 95^{241} of 540 b assuming a 4.9 month half-life for 96^{242} and a 490 year half-life for 95^{241}. Ghiorso reported that the fission chamber he is having built should be ready by next week, and it will then be much easier to make fission measurements at Argonne. The next trip out to Argonne will probably not be made until this chamber is completed.

Hyde talked about the work on the platinum bombardment. Layers are being removed with aqua regia. The solder is being avoided by covering the rest of the target with paraffin. About a thousand or so alpha-particle c/m are being obtained with each layer, apparently the same alpha particle obtained by Stan Thompson from the lanthanum bombardments. The activity was volatilized and collected on a plate which was a few mm above the plate from which it was volatilized. Stan Thompson found the activity was not volatilized from a thick LaF_3 sample. Placing a silver foil in a 1 N HCl solution of the activity results in the deposition on the foil of nearly all of the alpha-particle activity and also nearly all of the platinum. Some of the Geiger activity also plates out, possibly due to mercury or gold isotopes. All of this behavior points to these alpha-particle activities as being due to polonium isotopes. It was pointed out that we urgently need access to Dayton reports. Some of the Geiger activity has been identified as known isotopes, but it is a little early to discuss these activities.

James reported on the U^{238} plus helium ion bombardment on which he and Florin are working. This bombardment was of U^{238} containing only one part U^{235} in 3,000 parts U^{238}. It has a bombardment exposure of 2,300 microampere-hours. Seven layers were shaved off and dissolved; fluoride precipitations and ether extractions were performed. The layer removal was quite successful. The target was about 1 mil higher in the middle than at the edges, but the difference was not so great over the active bombarded region. Four 1 mil layers were removed after the first, then a 2 and a 3 mil layer. The energy of the alpha particles should be reduced to 18 or 20 Mev in the fifth layer and should be stopped before the seventh layer. Five or six hundred thousand c/m following plutonium chemistry were found in the first layer. Seven hundred alpha-particle c/m were found in the seventh layer, which may be largely uranium. Results, not available for a couple of weeks, from this target should help elucidate the Pu^{237}-Pu^{235} uncertainty. Electroplated samples will be prepared from each layer, and pulse analysis and fission measurements will be made on these plates. These should be good plates for attempting to separate the Pu^{239}-Pu^{240} alpha-particle peaks.

Ghiorso then presented a table of the results of his recent fission measurements at Argonne:

Sample	Wt. (x 10^{-12} g)	Net Fission c/m*	Thermal σ (b)	Equivalent Pu α c/m
95^{241}-96^{242}	1520 (62) 9.7 x 10^5 (51)	6,600	2700 (all 96^{242}) 4.3 (all 95^{241}	386
Pu^{238} ($T_{1/2}$ = 115 y)	1.42 x 10^6	37,200	16	2280 (2.3% of total count)
Np^{237} (very pure)	2 x 10^8	6,190	.012	225
U^{232} ($T_{1/2}$ = 70 y)	3.53 x 10^5	63,300	75 69	2480

continued...

Sample	Wt. (x 10^{-12} g)	Net Fission c/m*	Thermal σ (b)	Equivalent Pu α c/m
Ac^{227}	250 ± 100%	0	<2	
Pa^{231}	1.7×10^{8}	4,270	0.01	
Th^{227}	97	93	$\begin{cases}500\\580\end{cases}$	
Ra^{223}	53	0	<10	
Ra^{226}	1×10^{8}	$\begin{cases}43\\18\end{cases}$	$\begin{cases}2.7 \times 10^{-4}\\1.1 \times 10^{-4}\end{cases}$	$\begin{cases}2.5\\1.1\end{cases}$

*Background of chamber about 120 c/m

There was a discussion of the limitations of the methods used in measuring these cross sections. The high beta background levels caused by induced beta activity in the chamber adds to the high level in some samples to limit the sensitivity of the fission counting. This effect introduces uncertainties, particularly in the Th^{227} and Ra^{223} determinations where the very high energy alpha particles seem to be particularly interfering. The Pu^{238} result is somewhat uncertain because of the lack of knowledge about the purity of the Np^{237} used in its preparation.

Ghiorso thought that perhaps the original Np^{237} sample could be checked for Pu^{239}, and Osborne suggested that Np^{237} might be very carefully purified using Pu^{238} tracer to check the purification. This material should then be bombarded for several months at Clinton to give very pure Pu^{238}. Ghiorso said he would like to have a Pa^{231} plus deuteron bombardment in order to obtain material for measuring the fission cross sections of U^{230}, U^{231}, and Pa^{230}.

Van Winkle reported that about 24 mg of protactinium are now in a pure state and by the end of the week the number should be increased to 40.

Ames said that he has been investigating decay curves in order to count very accurately radium for specific activity determination.

Peterson has been working up the second radium plus neutron bombardment from Clinton. Lead and bismuth sulfide precipitation was employed first to get rid of radium daughters, the solution then adsorbed on a IR-1 column, and the actinium eluted with citrate solution. The first actinium fractions obtained had surprisingly high alpha-particle activity which upon further investigation proved to be daughters of radon that had been washed through the column.

*   *   *

Mother Bernerda, Dean at Mundelein College here in Chicago, called to ask me to speak at a student assembly of about 1,000 students on any Tuesday or Thursday before April 11. I explained that, unfortunately, I have no free dates during that period. Mother Bernerda indicated that she may ask me again next fall.

A thank-you note arrived from Watson Davis for my talk to the Science Talent Search winners on March 2.

The U.S. has asked the Soviet Union to explain the reason for troop movements into Iran at this time and to explain why not all its troops were withdrawn by March 2 in accordance with previous agreement, according to this morning's newspaper.

Thursday, March 14, 1946

This morning I replied to several letters received during my absence. To Mrs. Wladzia G. Podbielniak of Podbielniak, Inc., I explain that I do not have copies available for distribution of my talk to the Chicago Section. I returned the galley of my Detroit talk to Neil Gordon, who is publishing it in Record of Chemical Progress. I note that I have made a few minor corrections and ask if it will be possible to obtain reprints of the article. I then sent Robert Machol of Science Illustrated, a copy of my Illinois-Iowa ACS Section talk. I say that it may be used as source material, but I do not want the entire article reproduced.

Lawroski and Hyman completed and gave me a copy of Chapter 23 of Volume 16A of the Plutonium Project Record, "Specific Factors Affecting Solvent Extraction Processes."

The manpower distribution of the Chemistry Division as of today is:

	Administration - Hogness,* Zarse	1½
	Seaborg, Heavy Isotopes	28
Section C-I	Asprey, Services	4
Seaborg	Lawroski,* Solvent Extraction	14½
	Anderson, Light Isotopes	2
	Administration - Seaborg, Manning, Stewart	3
		51½
	Hutchison, Pile Research	16
Section C-II	Simpson, Special Problems	11
Willard	Tomkins, Analytical	6
	Administration - Willard	1
		34
	Grand Total	87**

*Hogness and Lawroski are employed half-time by the Met Lab.
**This total of 87 includes 18 SED men.

According to this morning's newspaper, President Truman withdrew the name of Edwin W. Pauley whom he had nominated to become Under-secretary of the Navy. This was in response to pressure from some senators.

<u>Friday, March 15, 1946</u>

I attended a meeting of the Solvent Extraction Group. Others in attendance were Ader, Blaedel, Casler, Hausman, Hyman, Lawroski, Leader, Manning, Nachtman, Schaffner, Sheft, and Wagner. Sheft announced that one of the main problems left in the Redox Process is the improvement of the uranium decontamination which averages about 10^4 through the ICU column. A solvent extraction method for increasing the decontamination might be carried out on the IBU solution thus eliminating the need for another complete cycle. The general method involved is to attempt to scrub the fission products (mainly ruthenium) from the IBU stream and then to extract the uranium into an aqueous ICU solution as usual. Since decontamination of the IBU solution did not work, it may be necessary, as a last resort, to add a complete second cycle for the uranium.

An estimate was made of the amount of U^{237} present to get an idea of possible upper limit to decontamination for the ICU solution used. The best decontamination factor is of the order of 10-20. A reason for the higher factor observed in some experiments is that the scrubbing in the step III distributes the U^{237} between the two phases to give a decontamination from U^{237}. This will not happen in the column. To demonstrate a factor of 100 on the ICU solution, more active column solutions will have to be used.

I asked what the normal decontamination factor for uranium in column IC is, and Blaedel replied that they can get about 80 percent of the fission product activity out in IC. Sheft remarked that a decontamination factor of 100 is necessary for 10^6 overall. The best obtained in these experiments was 50, but since this was established at the U^{237} background, it should be regarded as a minimum. Leader commented that the maximum decontamination obtainable for the ICU solution after 50 days cooling is about 10^4 which is limited by the level of U^{237}. Decontamination above this value requires longer cooling.

Leader reported that the use of internally formed MnO_2 as a scavenger to further decontaminate ICU solutions continues to look promising. With synthetic ICU solutions in which the only reducing substance present was hydrazine, this method has sufficed to give complete precipitation of manganese after the MnO_2 treatment. With column ICU solutions containing other substances as well, the point at which the $KMnO_4$ being added stops reacting with reducing substances and begins to convert manganese ion to MnO_2 is more difficult to determine because of slow reaction with these other reducing substances. The solution after addition of enough $KMnO_4$ to precipitate the manganese as MnO_2 is digested for one hour with the MnO_2 precipitate, then cooled and centrifuged. The MnO_2 is easily centrifuged out. This method applied to synthetic ICU solution containing ruthenium activity only has consistently given decontamination factors above 10 and in one case as high as 50. The method when applied to column ICU solutions has thus far not given satisfactory results, although the low acidity of these solutions, of which a significant part is UX and U^{237} activity, makes it difficult to determine exactly how good the scavenging of fission products really is.

I commented that MnO_2 could not really be expected to carry many fission products efficiently, but Leader said it should be adequate for this purpose if it carries three of them well, namely ruthenium, zirconium, and cesium, since these elements account for practically all the fission activity in ICU solutions. Iron would not be carried in macro quantities. The amount of U^{237} present in the column ICU solutions is such that this solution can only be decontaminated by a factor of ~5 even if the scavenger removed UX and fission product completely.

Blaedel reported that several runs were made to study the effect of stainless steel and acetate in the first cycle, the results of which are summarized as follows:

Run 40U-25P
Column IB repacked with
3/16" stainless steel helices

Results		
Stream	Uranium Losses	Plutonium Losses
IAS	0.3 - 3% (1)	0.01 - 0.08% (0.1)
IBU		0.2 - 0.9% (0.35)

continued...

Run 41U-26P
Both columns IA and IB repacked with stainless steel. IBS solution contained 1 M HOAc in addition to flowsheet components.

Stream	Plutonium Losses	Uranium Losses
IAW	0.03 - 0.08% (0.05)	0.8 - 1.6% (1)
IBU	0.1 - 0.3% (0.2)	

It may be concluded that stainless steel in column IA did not interfere with plutonium and uranium recovery. Effect of HOAc on plutonium recovery in IBS solution is inconclusive; however, it is not very great and does not appear to make much improvement.

Run 42U-27P-15F. Both columns IA and IB repacked with stainless steel. IBS solution contained $N_2H_2 \cdot 2$ HOAc instead of $N_2H_2 \cdot 2\ HNO_3$. Plutonium losses in IBU solution were no lower than usual (0.8 percent). Toward the end of the run the total HOAc concentration was increased to 2 M in IBS solution, with no great effect on losses. The benefit of $N_2H_2 \cdot 2$ HOAc in IBS solution is questionable. The decontamination in this run was about as usual (10^4 for IBP, ICU solutions). However, the use of 2 M HOAc toward the end of the run in IBS solution appeared to lower this several fold.

Lawroski said that they are now trying to find the best balance between decontamination factor and plutonium recovery in different feed preparations.

I replied to several letters today. To Homer F. Priest, I wrote that I will not be able to address the East Tennessee Section of the ACS on May 18 because this date immediately follows my return to California. I suggest English or Stoughton of Clinton Laboratories or Manning of the Met Lab. I replied to E. S. Proskauer's letter about the possibility of my being editor-in-chief of Adventures in Nuclear Chemistry. I say that I, too, have a very heavy schedule for the next year or two, but I will be staying at the Shelburne Hotel in Atlantic City at the time of the forthcoming ACS meeting in April if he wants to get in touch with me. I mailed a copy of my Pittsburgh talk to both James T. Grady and Watson Davis, telling each of them that the talk has not yet been cleared by Security although I anticipate no deletions. I will call them if there are any changes. Next I sent Shirley Ann Robinson, Villa Maria College of Erie, Pennsylvania, a photograph and biographical information for the College's "Gallery of Living Scientists."

A letter arrived from Charles D. Hodgman of the Handbook of Chemistry and Physics, thanking me for permission to reproduce the "Table of Isotopes." I then wrote a note to Peter Ribotto in Ishpeming to send him a brief biographical outline and to accept the dinner invitation of Mr. Carl Brewer before my scheduled talk. Finally, a more personal letter went to the Mira Vista Country Club in Richmond, California, asking that I be placed on the waiting list for family membership.

At 5:30 p.m. I was picked up at home and taken to the downtown YMCA for dinner with the Men's and Women's Mathematics Clubs of Chicago (Maurice L. Hartung and Mary Werkman, program chairmen). I gave the after-dinner talk, entitled "A Discussion of Nuclear Energy." I began by saying,

> At the present time the public consciousness of the Atomic Bomb Project is largely confined to its military aspects. This is because of the dramatic and effective use of the bomb in helping to bring the war to a close and because of the social and political consequences of the reality of atomic explosives. It is generally known that large industrial plants are used in the bomb's manufacture and that a great amount of scientific and technical skill went into the design of the processes. However, in the future, when it will be possible to unfold a coherent account of the development, it will be seen that the manufacture of the atomic bomb involved the solution of an extensive list of extremely interesting scientific problems. This work will occupy an important place relative to the existing scientific literature, but the day for publication has not yet arrived.

President Truman reiterated his belief that ultimate control of atomic energy should rest with a civilian group. This was stated at a press conference yesterday and reported in this morning's paper.

Saturday, March 16, 1946

I attended the meeting of the Laboratory Council in Room 209, Eckhart Hall along with Chisholm, Cole, Daniels, Dempster, Foote, Furney, Hughes, Jesse, Lapp, Moulton, Mulliken, Nickson, Ohlinger, Rabinowitch, Willard, H. Young, Zinn, and Zirkle. The following is a resumé of the topics discussed:

It was announced that the evacuation of Eckhart Hall is proceeding according to schedule. The Patent Group has already established headquarters at the Armory, and the following groups will evacuate within the coming week: Daniels, Lapp, Furney, Hilberry, and the Offices Services Department.

Daniels said that the Tolman Committee Report for Declassification has been fully approved, and detailed declassification instructions will be issued soon. There are three major groups into which declassification falls and work with weapons will be placed in Group 3 and probably never be declassified. The Laboratory Director will review all documents prior to declassification, after which the reports will be sent to outside referees for final clearance.

Daniels recently conferred with General Nichols' Advisory Committee and obtained oral approval for the proposed 1946-47 research program and for the construction of a shop building to be located at Argonne. Daniels stated that approval has been received for the construction at Argonne of a fast-fission pile, but the proposed high-temperature pile will be constructed at Clinton by the Monsanto Chemical Corporation. Basic research on the high-temperature pile will continue at the Metallurgical Laboratory. Colonel Frye and W. B. Harrell are negotiating about the signing of the new 1946-47 contract. General Nichols plans to call a meeting of representatives of the midwest universities early in April, 1946. Zinn asked if wage adjustments had been considered in formulating the new contract. Daniels stated that government restrictions prohibit the granting of increases to any individual in excess of 15 percent unless the individual's work classification is changed. An attempt will be made to eliminate the University of Chicago's 3 percent limit on salary promotions. Zinn indicated that the location of Argonne is such that higher salaries should be offered to those working there. Lapp stated that the Services and Development Division is experiencing considerable difficulty with personnel because of the low salaries shop technicians receive.

I read a copy of Stewart's letter, dated March 15, addressed to M. D. Whitaker, in which he requests irradiation of two samples in the Clinton pile in positions of maximum flux. The samples are "GTS #140" - 5 micrograms of element 95 as the oxide - and "GTS #141" - 1.011 g of thallium nitrate. Each of these should be irradiated for about two months and requires no cooling time before shipping back to Chicago.

A letter arrived from Erle M. Billings, Secretary of the Committee of the Professional Training of Chemists of the ACS. He verified my membership on the Inorganic Panel, which consists of W. C. Johnson, Chairman; W. C. Fernelius, John C. Bailar, and H. N. Alyea. The Panel will meet with the Committee on Friday afternoon, April 5. The ACS will pay my expenses to the meeting.

I answered a letter from Ruth Behr, a high school student, who apparently has an uninformed science teacher. I say that the four new elements are <u>not</u> new forms of uranium, but elements with atomic numbers 93, 94, 95, and 96. Even though they are made in laboratories and production plants, they should be called "elements." I also explain that although "an element is a substance that cannot be broken down by ordinary chemical methods," they can be transmuted, one into another, by nuclear reactions. I also say that only the heaviest elements, with a few exceptions, can be broken into pieces by a process known as "nuclear fission." [I think that such information, passed from student to teacher, may eventually help educate the public.]

M. A. Williamson, Vice President and publisher of Chemical & Metallurgical Engineering, wrote to inform me that I will receive a print of a picture taken February 26 at the seventh biennial Award for Chemical Engineering Achievement.

I also received a copy of a letter (March 15) that George Everson wrote to Wayne Johnson, in which he attempts to clarify the obligation of the Radiation Laboratory toward the payment of transportation of personal items for unmarried men. Specifically, Everson asks about the contract with Herman Robinson.

Larry Asprey called me from his home in Iowa, saying that he has not yet received an offer from Everson. I said that an offer will be forthcoming. Asprey told me that he intends to drive to Berkeley and plans to arrive in the Bay Area about the 1st of April.

A copy of CP-3457, "Progress Report for the Month of February 1946, 60-Inch Cyclotron Activities," arrived. Hamilton reports a bombardment of 1215 μah (helium ions) on U^{238} and a bombardment of 75 μah (helium ions) on platinum for our Chicago group during the month. He also reports 6 μah (helium ions) on Pb^{208} and 7 μah (helium ions) on Pb^{208} for Perlman's group in Berkeley. Hamilton notes that development work continues on the problem of producing stripped carbon and beryllium ions.

The U.S. has assured Iran and Turkey that we will support them against aggression by the Soviet Union under United Nations charter obligations.

Sunday, March 17, 1946

I played 18 holes of golf with Winston Manning, Stan Thompson, and Steve Lawroski, at Cog Hill No. 2. Steve and I won a "low ball plus low total" match, 2 up. (WM-119, ST-92, SL-110, GS-93.)

Helen and I had dinner at the Osbornes where Helen is spending the night.

At 11:59 p.m. W. P. Jesse, Irwin J. Schaffner, Sigfred Peterson, and I left for Oak Ridge via Penn R.R. We will transfer in Cincinnati to L&N RR, which will take us to Knoxville.

An article on the front page of today's Sun says that the special Senate committee on atomic energy voted to make it unlawful for any person or groups including the Army or Navy, to manufacture atomic bombs or other weapons without approval of the President.

Monday, March 18, 1946

We arrived in Tennessee in the afternoon.

The General Information Meeting on Metallurgy was held at 3:00 p.m. in Building 735, with H. W. Russell as chairman. The program was as follows: Properties of High Temperature Pile Structural Materials, C. A. Hutchison; The Preparation of Aluminum-Clad Uranium-Aluminum Alloy, L. H. Grenell; Beryllium Reduction, MIT group; Studies of Beryllium Oxide Ceramic Bodies, H. R. Nelson; Preparation of Beryllium Nitride, MIT group; Beryllium Fabrication and Alloys, MIT group; Studies of the Metallurgy of Beryllium, H. W. Russell.

I am staying at Roane Anderson Guest House here in Oak Ridge.

In a paper I picked up when I arrived here, I note in an article on Iran that the big factor in the struggle for position in that country is oil.

Tuesday, March 19, 1946

At 8:15 a.m. I attended the symposium (J. Schubert, chairman) on ion exchange in the conference room of Building 703-A. I spoke about Stan Thompson's work, "The Behavior of Element 95 in the IR-1 Citrate System." Others on this program as speakers or co-workers and their topics were: Kinetics of Ion-Exchange, L. S. Myers; Use of Ion-Exchange as Physico-Chemical Tool, J. Schubert; Separation of Fission Products, W. E. Cohn; Extraction and Decontamination of Plutonium, J. A. Ayres; The Status of the Application of Ion-Exchange to Plutonium Separation, J. A. Swartout; The Rare Earths and Plutonium Separation, J. Marinsky; Separation of Rare Earths, J. A. Ayres; Actinium Separation Using IR-1, S. Peterson; Adsorption Behavior of Protactinium in IR-1 and IR-4, Van Winkle, R. C. Thompson, and Osborne; Remote Control Methods in Column Adsorption Operation, G. W. Parker; New Engineering Designs for Adsorption Processes, G. P. Monet; Analytical Application of Ion-Exchange to the Separation of Thorium from Uranium, R. Bane; Separation of Plutonium and Other Substances from Biological Material, Russell and J. Schubert; Separation of Fission Products Using Anion-Exchangers, G. E. Boyd.

Simultaneously, while the ion exchange program was taking place, Nordheim was chairing a meeting on physics. Talks scheduled were: Uranium Resonance Absorption, N. Goldstein; Cross Section Measurements with Pile Oscillator, C. D. Moak; Temperature Characteristics of the Clinton Pile, A. J. Ulrich; Capture Gamma-Rays, S. Dancoff; Some Measurements with the 180° Beta Ray Spectrometer, P. W. Levy; The Double Crystal Neutron Spectrometer, R. B. Sawyer; Neutron Polarization Experiments, W. H. Zinn; Preliminary Measurements of C^{14} Intensities with Vibrating Reed Electrometer, Swank and Jesse; The Half-Life of C^{14}, L. D. Norris.

At intermission I left the Ion Exchange Symposium and attended the physics meeting. Walter Zinn acted as chairman. At about 11:30 I spoke on "Recent Results of Heavy Isotope Work." Other speakers and their topics at this session were: Cross Sections at Fission Energies, D. J. Hughes; Mass Assignments and Enrichments, A. J. Dempster; Fissionability Studies: Po^{210} and Np^{239}, J. J. Floyd; Deuteron and Helium-Ion Bombardment of U^{233}, M. Studier; Deuteron and Helium Ion Bombardments of Pa^{231}, D. Osborne (R. Thompson and Q. Van Winkle); Production of Actinium from Radium, Properties of Actinium and Its Daughters, S. Peterson.

At 1:40 p.m. in Building 735 I chaired a meeting on chemistry. Speakers and their topics included: Contributions to Radiochemistry of the 104-Day Y^{88}, R. Overman; Summary of Fission Product Investigations at H.E.W., W. H Sullivan; Color Production in Lithium Fluoride by Radiation, A. J. Miller; Behavior of Plutonium in the Presence and Absence of Citrate Solutions, J. Schubert; Potentials of Plutonium Couples as a Function of pH, K. A. Kraus; Recent Work on the Redox Process, I. Schaffner.

Frank H. Spedding became chairman at intermission. Talks given were: Action of Electron Beams and X-rays on Highly Purified Water, J. A. Ghormley; A System for the Separation of Uranium from Thorium Using Organic Complexing Agents in Solvent Extraction, O. K. Neville; Activity Coefficients of Plutonium, R. E. Connick.

In Chicago General Groves is scheduled to visit the Met Lab today and tomorrow.

President Truman has chosen Bernard M. Baruch as the U.S. representative on the United Nations Commission on atomic energy.

Wednesday, March 20, 1946

At 8:15 a.m. I attended the meeting on "Pile Discussions" in Building 735, chaired by M. C. Leverett. The topics and speakers were as follows: Xenon Poisoning and Pile Stability, Soodak and L. Nordheim; Migration Areas in Heterogeneous Piles, Scalettar; Proposed

Experimental Facilities for the Heterogeneous Pile, J. Huffman; Pile Calculations, G. Young; Power Plants Using High Pressure Water, Murray and Weinberg; Resonance Energy Piles, M. Goldberger; Pile Operations at High Temperature, R. G. Sacks.

Elwin Covey and I had a brief conversation about his future plans. He is anxious to return to school in Berkeley but would like a part-time job while in school since he now is married and has a baby. I said I would check into the possibilities.

Peterson, Schaffner, and I took the L&N RR to Cincinnati and then transferred to the Penn RR for Chicago.

Field Marshall Alexander, newly appointed Governor General of Canada, predicted that the atomic bomb will prevent war for a long time to come.

Thursday, March 21, 1946

We arrived in Chicago at 7:30 a.m. I stopped at our apartment before going to the laboratory. Helen told me that she had spent almost all day Monday with Marjorie Osborne. On Tuesday she had two dental appointments--11:00 a.m. and 1:15 p.m. She and Frances Chilson spent Tuesday night at the Osbornes. On Wednesday Helen and Frances went downtown and Frances, who is in the WAVES, returned to Great Lakes. Helen then visited Wilma.

At the laboratory I found an updated memo (March 14, 1946) from Magnusson and Hindman on the chemistry of neptunium in sulfuric acid. They find the disproportionation of Np(V) in 1.0 M H_2SO_4 is considerably greater than the value which was calculated from the early oxidation potential data. The equilibrium constant $(Np(IV))(Np(VI))/(Np(V))^2$ is 2.4×10^{-2} at 25°. In 1.86 M H_2SO_4 the constant is ca. 0.16. A reliable re-evaluation of the formal oxidation potential of the Np(V)-Np(VI) couple in 1.0 M H_2SO_4 was obtained by potentiometric titration of Np(V) with $KMnO_4$ using a Hg_2SO_4 reference electrode. The couple showed reversible behavior and the calculated formal oxidation potential is -1.084 v at 25°. A new value of -0.99 v for the formal oxidation potential of the Np(IV)-Np(V) couple in 1.0 M H_2SO_4 was calculated by use of the disproportionation constant and the Np(V)-Np(VI) potential. The oxidation potential of the Np(IV)-Np(V) couple is thus 0.25 v more negative in 1 M H_2SO_4 than it is in 1 M HCl.

Although time did not permit extensive studies, they are now certain that the oxidation of Np(IV) by bromate ion in 1 M H_2SO_4 at 25° proceeds by the following steps:

(1) $Np(IV) + BrO_3^- \rightarrow Np(V)$ very slow

(2) Np(V) + BrO_3^- → Np(VI) practically instantaneous

(3) Np(IV) + Np(VI) → 2 Np(V) slow

Magnusson and Hindman have re-examined the records of the early ultramicro preparations and now find that the neptunium could have and very probably was in the IV state when the peroxide was precipitated.

A letter arrived Monday from Perlman, asking that I check my notebook for the assignment of thallium isotopes. He recalls abstracting a report on the subject before leaving Chicago.

I read John H. Mahoney's security report dated March 18 on the article, "Future Possibilities with Radioactive Tracers" for my Pittsburgh talk. Two minor changes were suggested: page 4, line 6, change "tremendous" to "important" and page 24, line 16, change "the huge" to "large."

T. Agazim of American Society of Brewing Chemists, in a letter dated March 18, confirmed our arrangements for my talk in Milwaukee on May 7. He would appreciate a 30 to 40 minute discussion, not too theoretical, on any phase of the nuclear energy problem.

Stewart's memo to Furney, dated March 20, about the requirements for U^{233} for the next six months was also on my desk. He states that an additional three grams, already proposed at recent policy meetings, will be needed to expand our study of this isotope.

Another suggestion for names of elements 95 and 96 arrived yesterday. George A. Davis, a lawyer and amateur astronomer in Buffalo, suggests "sirium" and "canopium" after the brightest stars.

Also in yesterday's mail was a letter from Maurits Dekker of Interscience Publishers, Inc., who said that he and Dr. Proskauer will try to get in touch with me April 8 in Atlantic City.

Yesterday Waldo Rall, formerly with the SED at Site Y, was hired as a Junior Physicist to work with H. L. Anderson in Group 4 (Light Isotopes).

A telegram arrived from Ted La Chapelle today, saying that he has not heard from Everson. I immediately called Everson in Berkeley, who explained that he has sent written offers to both La Chapelle and Asprey. I again raised the question of whether the people starting work at the Radiation Laboratory could go on the payroll as soon as they accepted employment and were on route to the laboratory. He will discuss this with Priestley in the Controller's office.

I then mailed both Asprey and La Chapelle the information about the offers.

In a letter to Miss Rebekah Young (Physics Department secretary) in Berkeley, I sent a $50 deposit on an apartment which she has informed me will be available shortly. I also say that my sister will get in touch with Miss Young if she decides to occupy it temporarily before we arrive in Berkeley.

Helen, Dorothy Paul, and Dorothy Corey had dinner together and then went to a concert.

Today's paper reports that General Groves spoke in Chicago at the opening-day luncheon of the Fourth Annual Chicago Production Conference. He spoke about the timing of the New Mexico test and the part the atomic bomb played in ending the war.

Friday, March 22, 1946

Today I sent Watson Davis and Daniel Wilkes (Public Information Department of the University of California) each a copy of the talk I will give at the Symposium on Nuclear Chemistry on April 10 at Atlantic City. I explain to them that this talk includes our suggestion of names for element 95 and 96. I also mention to Dan Wilkes that our original January and March 1941 notes announcing the discovery of element 94 are due to be published in the next (March) issue of Physical Review.

I mailed an additional copy of the talk to K. Bryson Fleer of the ACS in Washington, and thanked him for the clipping from the March 3 Washington Post about an interview with Henry Wallace, Secretary of Commerce. Wallace feels that atomic energy could be harnessed for peacetime uses within a year without endangering national security. Ultimately peacetime use of its powers will bring blessings to the world almost beyond the possibilities of mention. In fact, he declared, application of atomic energy to peaceful uses rather than restricting it to purposes of destruction would better the security of the Nation rather than imperil it. While the most spectacular of the uses expected generally of atomic energy is in the production of power, probably the greatest immediate advantage and perhaps also longtime benefit, would be in the science of medicine, Wallace said.

"There is a tendency to keep it from being used for peace," Wallace commented in his office yesterday. He suggested that the "American people put on a continuous drive" to see that this tendency is overcome.

Wallace believes peacetime use of atomic energy is bound to come eventually. As long as all study of it is hidden behind heavy walls of secrecy with the world knowing only that research is constantly going for the sole purpose of war use, a peaceful world--a world free of suspicion and fear--is impossible, he feels.

The Secretary points out that peacetime uses of atomic energy would not mean the disclosure of the secrets of the bomb.

"They can't take from us our technical competence," he declared. "It is not necessary to divulge the methods of assembling of the bomb.

"But to withhold the whole study we have made of nuclear science would be to return to the Middle Ages. Science has been free since the days of Galileo. Without freedom of exchange of ideas there would have been no scientific advance."

I then suggest to Bryson that possibly we can work in a round of golf in Washington, D.C., with Jim Crowe on April 6 or 7.

A letter arrived from Perlman giving me a brief description of Werner's method for separating element 95 from lanthanum using TTA. Perlman will send us 10 μg of element 95, purified by this method, for spectrographic analysis. He believes that negligible amounts of lanthanum are present, but he is somewhat concerned about thorium and, in particular, about neodymium.

I also received from Berkeley, the bombardment log for the platinum irradiated with 44 Mev helium ions.

Other correspondence today includes a confirmatory letter about my forthcoming talk from John R. Bowman, Chairman of the Pittsburgh Section; a periodic chart from Fred A. Rantz of Seattle, which he prepared in 1932; a thank-you note from Mrs. Mary Werkman, Program Chairman of the Women's Mathematics Club of Chicago.

Helen had a 10:30 a.m. dentist appointment today. In the afternoon she visited Wilma, and later Frances Chilson arrived to spend a few days with us.

"A-Bomb Tests Postponed" reads today's top headline. The White House announced the delay explaining that pressure of legislative business would prevent members of Congress from witnessing the tests.

Saturday, March 23, 1946

Jake Warner sent me a copy of the announcement that was sent to Directors and Heads of Departments of Carnegie Institute of Technology about my forthcoming talks there.

I also received a carbon of Stewart's sample descriptions (GTS #143 - 51-62 mixture and GTS #144 - 51) being sent to Perlman in Berkeley. Stewart also mentions that the thorium metal target is being returned by courier but Hamilton should postpone its bombardment. The request for a liter of the original Site Y salt solution cannot be filled because the whole barrel was processed, and the request for "CW" salt solution will have to be shelved until shipping containers are built.

Manning, Stan Thompson, Hagemann, and I played 18 holes of golf at Cog Hill Course, No. 2. Hagemann and I won a "low ball plus low total" match, 6 and 4. (WM-112, ST-108, FH-106, GS-98.)

Sunday, March 24, 1946

The American Review on the Soviet Union published (February issue) a brief review, "Research on Atomic Energy in the USSR," by Gerald Oster which I read today. Oster's report is based on published Russian scientific literature. Oster states, "There is abundant evidence in Soviet scientific literature of their knowledge of the energies associated with nuclear fission." He goes on to say that A. E. Brodsky has conducted research on the separation of heavy water and, in 1942, separated U^{235} by thermal diffusion of the hexafluoride of uranium.

According to the front page of today's Sun there will be a further delay in "Operation Crossroads"--the Navy test of atomic bombs in the Pacific. The most recently scheduled date is July.

Monday, March 25, 1946

Ruth Lucille Dirks, who spent a year in the WAVES, began working today as a secretary to Winston Manning. Horace Hopkins was discharged from the SED last Friday and placed on the civilian payroll today.

I received additional fragmentary information on the radiation of the 1-mg sample of Np^{237} that arrived from Hanford the end of January; I passed it on to Manning.

Winston Manning has prepared and sent to Hogness today the "Summary of Work of Section C-I for the Period February 15, 1946 to March 15, 1946" (MUC-GTS-2243) with the following information:

The discoverers of elements 95 and 96 propose the name "americium," (symbol Am) after the Americas, for element 95 by analogy with the name of the corresponding rare earth "europium." For element 96 the proposed name is "curium" (symbol Cm), after Pierre and Marie Curie, by analogy with the name of the corresponding rare earth gadolinium, which recalls Gadolin, the noted rare earth chemist.

Production of U^{232} by neutron bombardment of Pa^{231}; capture cross section of Pa^{231} for pile neutrons; fission of U^{232} by thermal neutrons. A sample consisting of about 1 mg of pure Pa^{231} as the oxide was bombarded for about a month in the Clinton pile. Uranium-232 was formed as a result of the reaction

$$Pa^{231}(n,\gamma)Pa^{232} \xrightarrow[1.4\ d]{\beta^-} U^{232}.$$

The yield of U^{232} was 1.0×10^7 alpha c/m, corresponding to 0.4 μg for a U^{232} half-life of 71 years. The cross section for the reaction $Pa^{231}(n,\gamma)Pa^{232}$ calculated from the data of this bombardment is 175 b ± 15 percent. This value supersedes the earlier rough value of 200 to 300 b.

The U^{232} produced by this bombardment was tested for fission in the thermal column of the Argonne pile. A fission cross section of 70 ± 10 b was also observed for a much smaller sample of U^{232} produced as a result of neutron bombardment of Pa^{231} in the Argonne pile. It is highly unlikely that there was any appreciable contamination with natural uranium in the case of the larger sample of U^{232}. A second order neutron reaction involving the formation of U^{233} by the reaction $U^{232}(n,\gamma)U^{233}$ during the bombardment of the Pa^{231} sample at Clinton could not produce enough U^{233} to account for the observed positive fission results, unless the capture cross section of U^{232} were extremely high, of the order of 10^5 b.

The fission yield of Ba^{140} produced during the neutron bombardment of Pa^{231} at Clinton was determined. If a fission product distribution curve corresponding to that for U^{235} is assumed, the observed yield corresponds to a fission cross section of 0.035 b for Pa^{231} if all of the fission is attributed to that isotope. That is about 3.5 times the directly observed fission cross section for Pa^{231}, but the excess fission is accounted for quantitatively by the fission of U^{232} formed during the bombardment of the Pa^{231}.

Production of Pu^{238} by neutron bombardment of Np^{237}; possible fission of Pu^{238} by thermal neutrons. A sample consisting of about 1 mg of Np^{237} was bombarded for about two months in one of the Hanford piles. The Pu^{239} content of the original neptunium sample was known to be not more than 15 ppm. About 18 micrograms of Pu^{238} (assuming a half-life of 115 years) was formed during the bombardment by the reaction

$$Np^{237}(n,\gamma)Np^{238} \xrightarrow[2.0\ d]{\beta^-} Pu^{238}.$$

The Pu^{238} was tested for fission in the thermal column of the Argonne pile. Thermal neutron fission was observed in an amount corresponding to a cross section of 18 b for Pu^{238}, if it is assumed that no Pu^{239} was present. A Pu^{239} contamination amounting to over 2 percent of the Pu^{238} would be necessary to account for all of the observed fission. This is twenty-fold greater than the amount that could have been present in the Np^{237} before bombardment. However, a capture cross section for pile neutrons in the neighborhood of 500 b for Pu^{238} would have resulted in the formation of the required amount of Pu^{239} by a second-order reaction during the Hanford bombardment.

The uncertainty will be resolved by carrying out a less intense bombardment of Np^{237}, and testing the relatively pure Pu^{238} which will be produced.

Thermal neutron fission of Pa^{231} and Np^{237}. It now appears that Pa^{231} and Np^{237} are both fissionable by slow neutrons with very small, but none the less definite, cross section. In several previous measurements in the thermal column at Argonne these isotopes showed apparent fission cross sections of the order of 0.01 b. However, these results were not regarded as conclusive because of the possibility of contamination with small amounts of uranium in the case of Pa^{231}, and Pu^{239} in the case of Np^{237}.

The neutron bombardments of Pa^{231} and Np^{237} that resulted in the formation of relatively large amounts of the short-lived isotopes U^{232} and Pu^{238}, made it possible to repurify these samples after bombardment with definite assurance that the uranium and plutonium were reduced to an extremely low level (because of the sensitivity for detection of the long-range alpha radiation of U^{232} and Pu^{238}). These repurified samples showed thermal neutron fission cross sections of 0.010 ± 0.005 b for Pa^{231}, and 0.018 ± 0.005 b for Np^{237}. The limits of error are not due to statistical uncertainty in the fission counting but to uncertainties in the degree of self-absorption of the fission recoil fragments. When the fission chamber was surrounded with cadmium, the fission counting rate for each sample dropped by a factor of 30 to 40 showing that the fast neutron effect was small.

Capture cross section of Am^{241} for pile neutrons. A 1.7 microgram sample of Am^{241} was bombarded for five weeks in the Clinton pile. After bombardment the alpha activity of the sample, which was originally 100 percent at the 4.1 cm range of Am^{241}, was observed to consist of 40.3 percent at 4.1 cm and 59.7 percent at the 4.7 cm range of Cm^{242} (formed as a result of the reaction

$$Am^{241}(n,\gamma)Am^{242} \xrightarrow[18\ hr]{\beta^-} Cm^{242}).$$

If one assumes a 490 year half-life for Am^{241} and a 0.40 year half-life for Cm^{242}, and corrects for decay of Cm^{242} during and after bombardment, a capture cross section for pile neutrons of 560 b (±15 percent) is calculated for Am^{241}. This cross section is large enough so that bombardment of Am^{241} at Hanford for half a year should convert about 10 percent of the Am^{241} to Cm^{242}. It will thus be possible to produce macro amounts of curium by bombardment of americium.

Summary of thermal neutron fission cross sections. The following table summarizes the fission cross sections determined during the past few months by the nuclear chemistry section.

Element	Mass Number	Fission Cross Section (barns)
$_{88}Ra$	223 (AcX)	<100
	226	$\leq 1.1 \times 10^{-4}$
	228 ($MsTh_I$)	<2
$_{89}Ac$	227	<20
$_{90}Th$	227 (RdAc)	540 ± 200
	229	40 ± 10
	230	0.0012
	232	$\leq 2 \times 10^{-5}$
$_{91}Pa$	231	0.010 ± 0.005
	233	<0.1
$_{92}U$	232	70 ± 10
$_{93}Np$	237	0.018 ± 0.005
$_{94}Pu$	238	≤18
	240	≤180
$_{95}Am$	241	≤2.8

Half-life of Pa^{231}. The specific activity of weighed amounts of pure protactinium oxide has been determined. A value of 5.17×10^4 α c/m per microgram of Pa^{231} (51.7 percent geometry) was obtained. This corresponds to a half-life of 34,300 years. This value is probably accurate to at least 1 percent if it is correct to assume that the formula of the ignited oxide is Pa_2O_5. The earlier value obtained by Von Grosse, who also assumed a formula of Pa_2O_5, is 32,000 years.

Chemistry of protactinium. The solubility of protactinium in various acids has been measured on an 0.5 to 1.0 mg scale. The starting material in each case was dried hydroxide. Erratic results were obtained: they confirm the earlier observations that protactinium in aqueous systems shows a very strong tendency to form colloidal solutions. Where true solubilities are observed, complex ions are almost certainly involved.

In perchloric acid, which is known to have very little tendency to form complex ions, extremely low solubilities were observed: 0.03 mg per ml in 11.5 N $HClO_4$ and 0.0045 mg per ml in 7 N $HClO_4$. In 2 N $HClO_4$, a higher apparent solubility, due to the formation of a colloidal solution, was observed. In 16 N nitric acid solubilities ranged from 1.8 to 10 mg per ml. Slightly lower solubilities were observed in 12 N HCl and 18 N sulfuric acid.

A letter arrived from Iz this morning asking about Berkeley needs for 23 for six months. He suggests 100 mg. He then goes on to mention the present Hanford irradiation of thorium, saying that if it is bombarded for 100 days, it will produce U^{233} at somewhere between 400-900 gt level. He would like to consider whether this high a level is desirable for the various uses to which the U^{233} is to be put; particularly, he questions the sizable amount of U^{232} alpha activity that would be present. Iz then asks about the amount of neutron-bombarded thorium we should request.

I had already started writing to Iz this morning when his letter arrived. In it I suggest that we originate from Berkeley a request for one batch (160 g) of 600-700 gt plutonium from Hanford to be used as a source of 95^{241}, allowing the Army to determine whether it should be shipped in one of the Hanford containers or first shipped to Los Alamos to be reduced to metal. Another point I raise is the question of future spectrographic analyses in Berkeley.

I include a few forms used at Chicago for requesting Hanford irradiations, suggesting that something similar should be prepared for Berkeley. I note that MB-IP-41 for the irradiation of 2-5 mg of 95^{241} was completed in Chicago although the irradiated sample will be shipped to Berkeley. I then ask him to try to learn the true maximum neutron flux that is contemplated for the Chalk River pile; and, if it is greater than Hanford's, we should take steps preliminary to arranging for irradiations there.

Finally, I agree with his estimate of 100 mg as our requirement for 23 and say I shall look into the other question.

Also in today's mail was a carbon of a letter Iz wrote to Senator William F. Knowland. Iz says in part:

> ...Despite the undesirable features of any form of restraint toward scientific development there is obvious need for some form of control over such tremendous forces as are involved here. The exercising of this control will be a difficult job, at best. Some of the current thoughts on control, such as those embodied in the May-Johnson bill, would be crippling to any industrial development and, more important, to any fundamental advances in our scientific knowledge. I feel that the provisions of the May-Johnson bill are so incompatible with the progress of science that we as a nation would, by its passage, relegate ourselves to a secondary position in the field of nuclear research and development. We must surely hope for more than just further military development.
>
> Of the proposals that have been made on atomic energy control, those embodied in the original version of the McMahon bill appear to be most workable. I therefore wish to give my endorsement to this bill...

At lunch today at the 1004 Club we discussed the Greensboro Open held in Greensboro, North Carolina, this past weekend. Sam Snead won $1,500 for his score of 270. Greensboro is Snead's home course. Other scores were Herman Keiser - 276, Lloyd Mangrum - 279, Jimmy Demaret - 282, and Jim Ferrier - 282. A few snapshots were taken of some of us after lunch. See Figures 48-50. Other recent noontime pictures are shown in Figures 51-58.

I learned of the death yesterday of Gilbert N. Lewis of the Chemistry Department at Berkeley. I am deeply saddened by this unexpected event. I served as his personal research assistant for two years--from 1937 to 1939.

Helen went to the library in the morning, and in the afternoon she visited Marjorie Osborne. Frances Chilson is still staying with us.

At 11:15 p.m. I left Chicago via the "Gold Triangle" (lower 12, car 624) for Pittsburgh.

World chess champion, Alexander Alekhine, was found dead in his hotel room in Estoril, Portugal, reports today's paper.

The Soviet Union announced that its troops are leaving Iran and will be out in six weeks. Moscow says that withdrawal is in accordance with an agreement with the Iranian government.

Tuesday, March 26, 1946

When I arrived in Pittsburgh, I was met by John R. Bowman, Chairman of the Pittsburgh Section of the ACS. He took me to the Carnegie Institute of Technology where I visited Jake Warner and the Institute. After an informal luncheon at the University of Pittsburgh with university and industrial people, I gave some brief remarks about nuclear energy. During the early part of the afternoon we toured the university. At 3:00 p.m. I talked to a joint Chemistry-Physics Seminar in Room 218, Engineering Hall, at Carnegie Institute on the subject, "The Heavy Elements." A dinner was held at 6:30 in the evening, and at 8:30 p.m. I spoke to the Pittsburgh ACS Section members in the Mellon Institute Auditorium on the subject, "Future Possibilities with Radioactive Tracers." I began my talk by stating,

> The present discussion will be concerned with a number of the applications of artificial radioactive isotopes for tracer or "atom tagging" experiments in the fields of chemistry, physics, biochemistry, and medicine. There are now known, in the open literature, over 400 induced radioactivities, there being at least one radioactive isotope for each of the elements of atomic number 1 to 96 inclusive...The outlook at the present

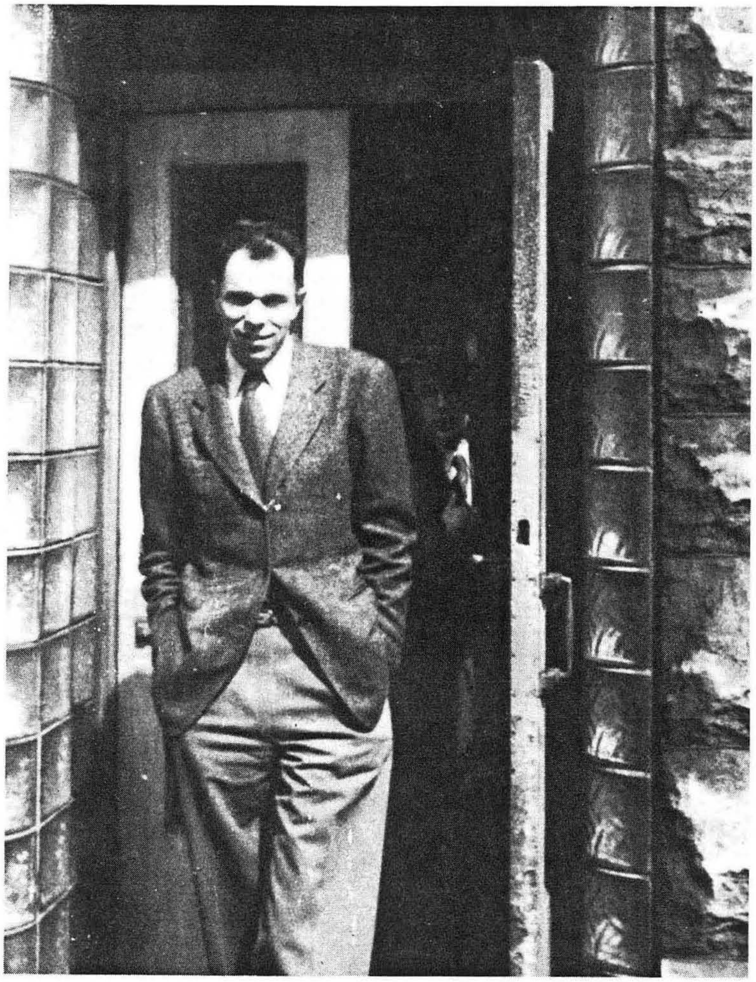

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Figure 48. Glenn Seaborg in front of 1004 Club, 55th Street near Ingleside Avenue, T. O. Jones in background. March 1946.

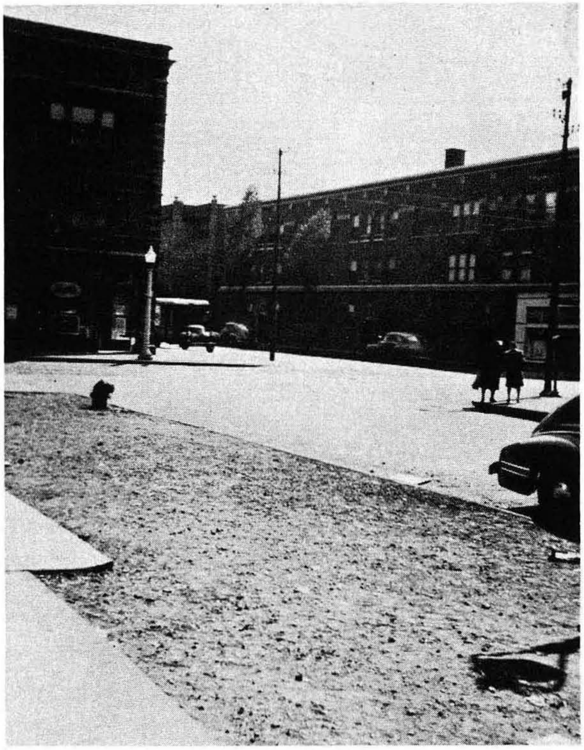

Figure 49. Corner 55th Street and Ingleside Avenue, March 1946.

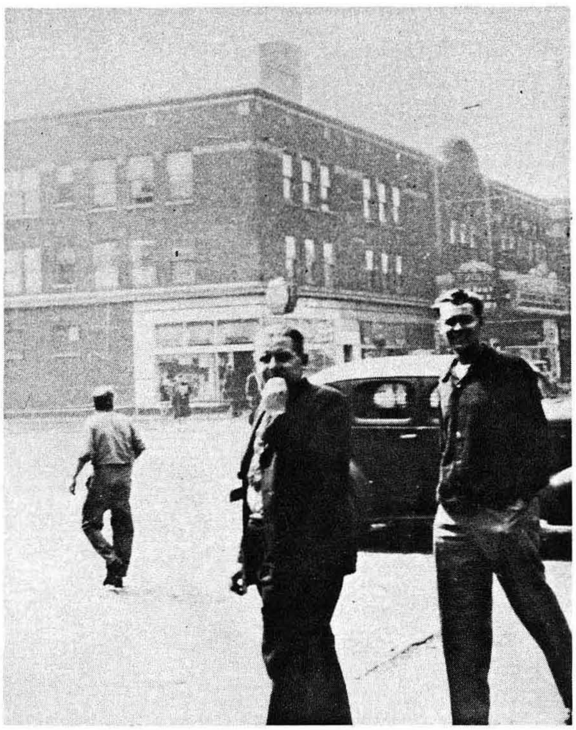

Figure 50. Sherman Fried (?), Steve Lawroski, Walt Blaedel, in front of 1004 Club, corner 55th Street and Ingleside Avenue. March 1946.

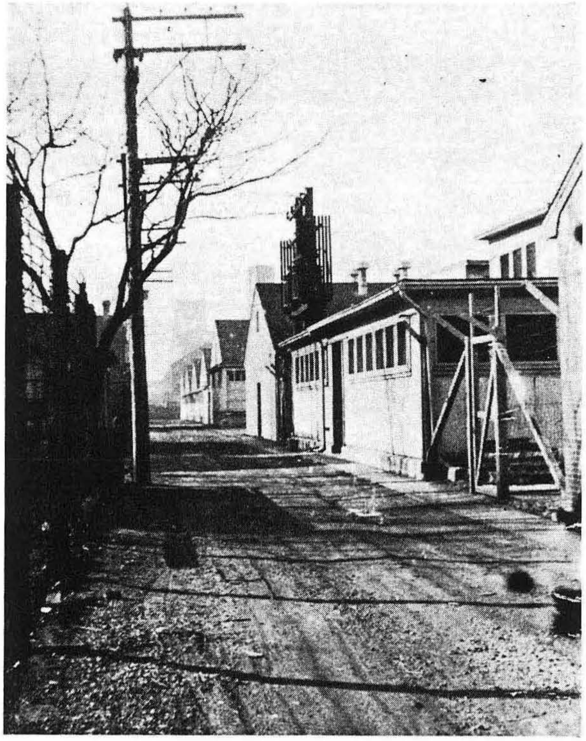

Figure 51. Alley behind New Chem, March 1946.

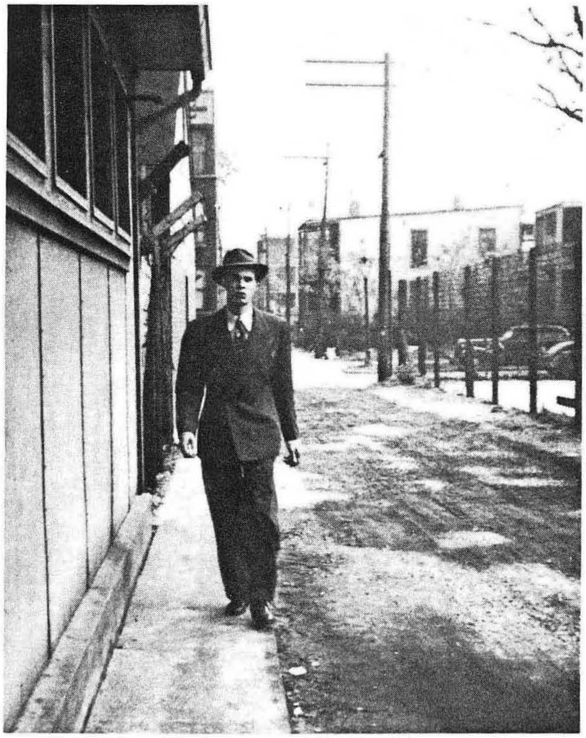

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Figure 52. Ben Scott, alley behind New Chem, March 1946.

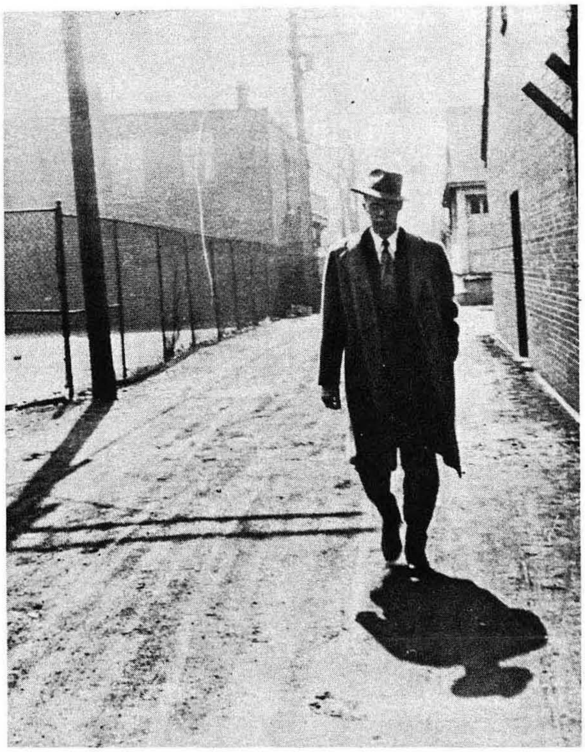

Figure 53. Quentin Van Winkle, alley behind New Chem, March 1946.

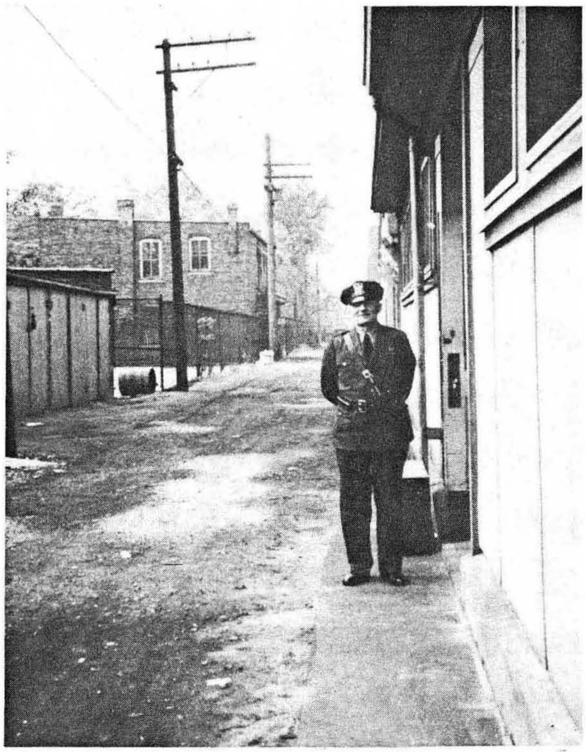

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Figure 54. Guard, alley behind New Chem, March 1946.

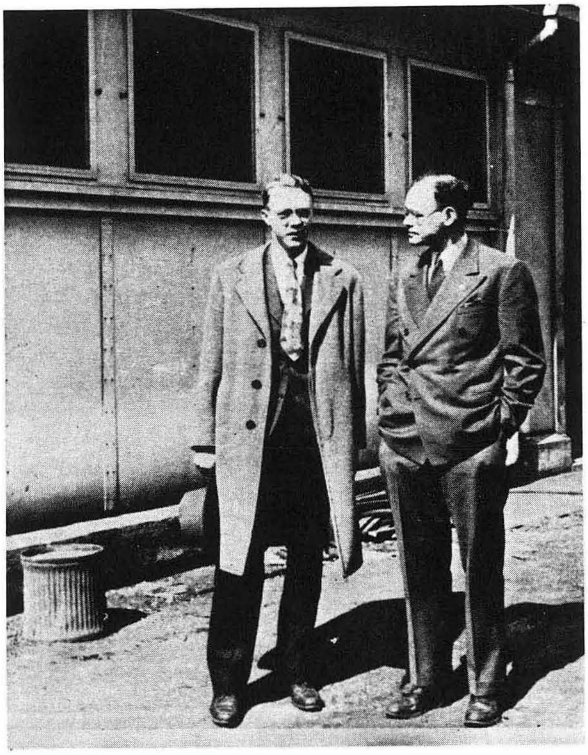


Figure 55. Quentin Van Winkle and Roy Thompson behind New Chem, March 1946.

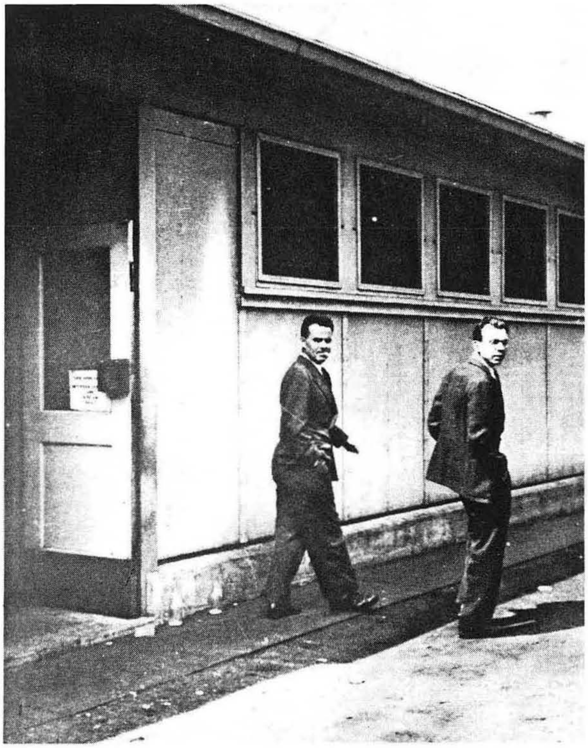


Figure 56. Stan Thompson and Burris Cunningham behind New Chem, March 1946.

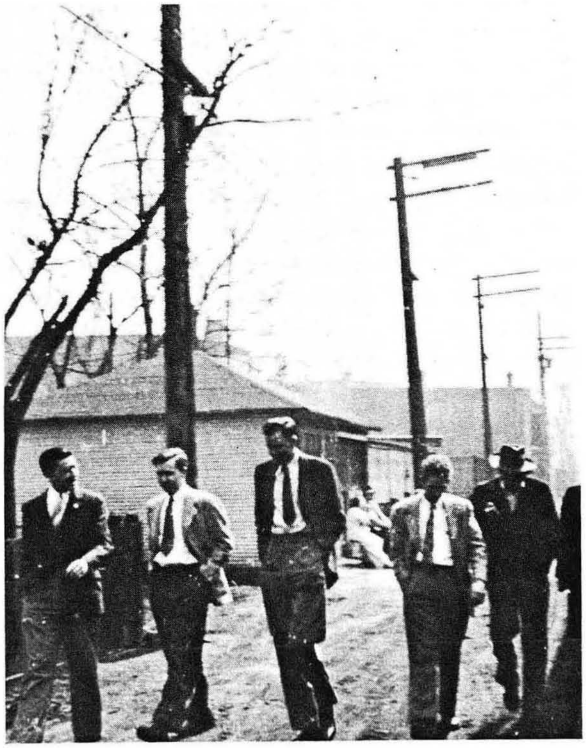


Figure 57. Ghiorso, Osborne, Seaborg, Hindman, Magnusson, Jones (partial), back of New Chem, March 1946.

Figure 58. 56th Street, (front l to r) Ghiorso, Seaborg, Osborne; (back, l to r) Hindman, Jones, Magnusson, March 1946.

> time, therefore, is a very optimistic one with almost unlimited possibilities for research with the help of these isotopes. It seems realistic and entirely safe to predict that a large number of advances and discoveries will be made in the future, a few of them epoch-making. It is not at all out of the question that the greatest gains to humanity from the atomic energy development will result from the widespread use of tracers to solve a multitude of problems rather than from the harnessing of the power itself.

I went on to describe some of the experiments already performed with radioactive tracers and said,

> Of especial importance for the study of life processes and for many other investigations is the long-lived radioactive carbon isotope, C^{14}, which can be produced in such amounts as to permit its eventual widespread use. Other interesting isotopes include H^3, P^{32}, S^{35} and I^{131}. The radioactive fission products of U^{235} and plutonium will be available in particularly large amounts as a result of the chain reacting units. Also, there will be available new, alpha-particle emitting isotopes giving rise to new possibilities.
>
> Among the interesting subjects that may be studied might be listed reaction mechanisms, exchange reactions, photosynthesis, measurements of vapor pressures, industrial applications, metabolism and other life processes, public health problems, bacteria "tagging," therapy and study of cancer, and many other problems. The large amounts of gamma-ray emitting isotopes will be useful in the field of radiography.

In conclusion I remarked that

> The future seems to hold unlimited possibilities for the application of radioactive tracers to scientific problems. It is certain that the applications of radioactive tracers which have been made so far are just the beginning of what is going to become an extremely large and successful field of research.

I left Pittsburgh at 11:25 p.m. on the "Pennsylvania Limited" (lower 1, car 56).

Reports from Washington in the papers today, under a March 25 dateline, promise that the way is open for peaceful uses of atomic energy. The paper says, "Scientists disclosed today that they know how to 'denature' plutonium so that is atomic energy could be used only for industrial purposes and not for bombs."

Wednesday, March 27, 1946

I arrived in Chicago at 7:50 a.m. and went to the laboratory. The following telegram arrived from James T. Grady yesterday.

> Can Atlantic City address discuss denatured plutonium. Does Acheson statement mean early use of atomic heat and power in industry. Can uranium also be tamed. Would appreciate statement bringing Atlantic City talk abreast of economic, military and political developments. Your talk affords opportunity to point up whole program for nuclear chemistry symposium.

I immediately replied this morning, saying

> Am in process clearing through security my text for introductory remarks which will say as much as permitted regarding subjects you inquired about. Will send you copy.

I read a carbon of Al Ghiorso's letter to Iz Perlman, describing the thorium sent to Berkeley on Sunday. Ghiorso gives instructions for the preparation of the target which he did not have time to do and asks that Perlman arrange for the machine work. The bombardment is for the purpose of producing Pa^{233}, Pa^{232}, and Pa^{230} and will be used for fission measurements. Ghiorso states that it would be desirable to have the target flown back to Chicago and suggests that the Army could demonstrate the speed of one of the new jet planes. He asks that the bombardment be scheduled during the first two weeks of April.

Robert Redfield sent me a draft, prepared by Quincy Wright, for a Convention on the Control of Atomic Energy and asks for my comments. The draft suggests an Atomic Energy Commission be established at the seat of the United Nations and discusses the establishment of an Inspection Commission. Enforcement shall be by the Security Council.

Also in today's mail was a letter from Miss A. R. Robb in President Sproul's office, thanking me for representing the University at the Chemical Engineering Achievement Award dinner and saying that the scroll has been sent to the Archives.

The log of the bombardment of U^{238} with 44 Mev helium ions arrived from Paul O'Connor at Berkeley. Watson Davis' draft article on my April ACS paper, naming elements 95 and 96, arrived along with drafts for use over the Columbia Broadcasting System. Davis also included Science Services's story written about the paper I gave Tuesday in Pittsburgh.

Shuki Hayashi was hired today to work as a technician in H. L. Anderson's Light Isotopes Group (Group 4).

Frances Chilson concluded her visit with us this evening and returned to Great Lakes.

In an article in the Chicago paper this morning, Wendell Latimer is quoted as doubting the announcement of the "denaturing of plutonium." He thinks, however, that the U.S. should be well on the way to an industrial program for atomic energy.

Thursday, March 28, 1946

I attended the morning meeting of the Heavy Isotopes Group with Ames, Anderson, Elson, Erway, Fineman, Florin, Ghiorso, James, Kohman, Manning, Osborne, Peterson, Sedlet, Stewart, Studier, R. Thompson, and Warshaw. I opened the meeting by outlining the proposed organization of the section as of April 1. This re-organization has been made to adjust to the situation which will be brought about by many men leaving for Berkeley. It is proposed that "research co-ordinators" act as the lead men on each research problem, but that rigid division into groups be avoided. Under this proposal, the organization will be:

Section Chief - Manning

Assistant Section Chiefs - Osborne, Stewart

Research Co-ordinators
Basic Chemistry - Hindman
Instruments - Jaffey
Heavy Elements - Manning
U^{233} - Katzin
Solvent Extraction - Schaffner, Hyman

The general direction of 49 recovery and special service jobs will be under Stewart.

Hyde asked whether new men will be hired to replace the departees. I answered that the section is to be kept to present strength if good men can be hired as replacements. We can probably keep our present space if we can keep the rooms filled. He then asked how much liaison there will be between Berkeley and Chicago. I replied that we hope that Berkeley men can come here for pile measurements and that Chicago men can go to California for cyclotron bombardments. The Army desires a rigid division of research problems between the two sites, but this obviously cannot be made to hold too closely since some overlapping is inevitable. At the present time the Berkeley cyclotron will be used entirely for heavy isotope bombardments.

Kohman inquired if any other cyclotrons will be available for Chicago. Manning and I answered none at the high energies. The Carnegie machine is now producing 15-17 Mev deuterons, and both it and the St. Louis instrument could be remodeled to give higher energy particles. Such modification would involve major changes and would probably require government help. The oscillator frequency is the final limiting factor, although any given magnet can be extended only to its saturation limit. James remarked that the low energy deuterons produced by the St. Louis machine are actually more desirable for certain reactions.

Ghiorso then asked if fissionability measurements will still be made at Argonne. He said that if so, one man should be put on the job half or even full time, as new methods will have to be developed to study some of the isotopes which are tougher to handle. I answered that fissionability measurements may be done at both locations, depending on the availability of people to do the work. Kohman suggested that some of the physicists have problems along similar lines and they might be interested in collaborating on a group development scheme. He then asked as to the type of work planned at Berkeley.

I replied that theses will probably have to be on secret subjects at first. No separations studies are planned, or, if any are made, they will be done in a separate building under separate supervision. Brewer's group may work on pile problems. Where individuals at each site are working on related problems, the division of labor will probably be made by personal communications. The work at Berkeley will probably just be reported in monthly abstracts and a final report issued when the state of the work justifies it.

Ghiorso said that the Th^{229} plate prepared by Hagemann has revealed two long range alpha-particle peaks along with the main peak, and with energies 50-100 kev higher than the main peak. The presence of these peaks would account for the x-rays observed from this isotope. He said the actinium isotope has not been examined. No extra peaks have been detected so far in the U^{233}.

Hyde reported on a new determination of the half-life of ionium. The material used had been analyzed by Rall and Dempster with the mass spectrograph. It was converted to the dioxide, weighed, dissolved, and a specific activity determined on an aliquot in a standard 52 percent geometry alpha counter. The value found was 8.3×10^4 years, which agrees exactly with the literature value given by the Curies. Their value was good only to 3 percent however, while this should be accurate to ½ percent.

Studier and Hyde then reported on their other new half-life determinations. They have assigned Pa^{230} a half-life of 17 ± 0.5 days and U^{230} a half-life of 20.8 days. They feel this latter value is quite accurate. Manning asked how the value on the Pa^{230} agrees with that found by Osborne for the hard radiation he detected in his processing of the Pa^{231} plus deuteron target. Osborne stated that the value he found (18.2 days through 5 g of lead) might possibly be in agreement if some daughter of Pa^{230} was producing a little additional hard gamma-ray in his sample. The number of counts at the end of the curve were down to 30 c/m and made it difficult to draw any final conclusion. I commented that the Berkeley people have found a 10-hour Geiger activity in working up the last U^{233} plus helium ion target.

I then asked for reports on the writing. Hyde replied all his papers except the one on U^{233} plus deuterons are in fairly good shape. Studier reported that he has been spending much time on the U^{230} and U^{233} papers. I asked about the papers on fissionability, etc., being written by various chemists in collaboration with Ghiorso, and Ghiorso stated that he plans on spending his last month in Chicago writing full time. I pointed out that he should be certain to get all information from Van Winkle, Peterson, Cunningham, etc., before these people leave.

I then asked about the Pa^{231} target which is to be sent to Berkeley for deuteron bombardment, and Osborne said the target itself is ready, and the sample should be on the cyclotron in a day or two if there are no problems. I said that Osborne and Roy Thompson will work up this target on its return. Studier and Hyde will handle the thorium plus deuteron target. No one has been formally assigned, as yet, to process the Np^{237} plus deuteron target which soon will be sent out.

Roy Thompson then reported that the solubility of protactinium in concentrated HCl and in 18 N H_2SO_4 seemed to decrease over a period of two weeks. The material was agitated constantly in all-pyrex containers at room temperature and was centrifuged in the usual air-driven microcentrifuge before assaying the supernatant.

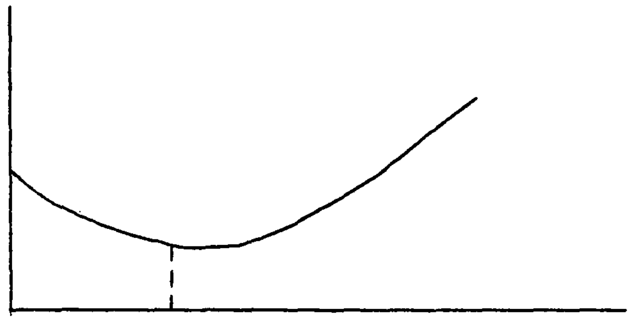
Conc. HCl	Solubility	18 N H_2SO_4	Solubility
After 3-4 hours	1.23 mg/ml	After 4 hours	5.1 mg/ml
After 2 weeks	0.4 mg/ml	After 1 day	1.7 mg/ml
		After 2 weeks	1.1 mg/ml

Evidence of polymer formation was obtained in perchlorate acid solutions:

$HClO_4$	Solubility
11.1 N	30.0 mg/ml
7.0 N	2.7 mg/ml
1.7 N	~44 mg/ml (apparent solubility)

By using the ultra centrifuge, he determined different insolubilities at different speeds in the case of the 1.7 N experiment. Measurement of the settling rates will indicate a molecular weight of 3-20 million for the polymer.

Ames reported on the status of his work on the determination of the half-life of radium. If counts per minute are graphed against time, the curve is of the type:



The first portion of this curve is a result of the decay of RaA and RaC, the second part to the growth of the radon and its daughter. The question arises as to the method to be used to extrapolate this second portion back to zero time to give the c/m of radon-free radium. It was decided to take the plate after 100 minutes, to dissolve it, heat to drive off the radon, to re-plate, and to resume the counting.

Repeating this procedure several times gave a plate in which only growth was observed, and extrapolation of this curve to zero time gave a half-life of 1,670 years. By modifying Bateman's equations (Proc. Camb. Phil. Soc. 15, 423 (1910)), a theoretical curve was drawn and a comparison of it with the experimental curve was attempted to evaluate the effects of diffusion of the radon on recoil from the sample. Thus modifying the experimental curve to confirm to the theoretical shape gave a half-life of 1,690 years.

Peterson reported that he has finished working up the second radium target bombarded at Clinton. The IR-1 resin column was used in separating out the actinium. The capture cross section of Ra^{226} has not been re-calculated as yet, but improved chemical and physical measurements in this run should give a more accurate value than that estimated from the data of the first run.

Anderson then summarized his results on the preparation of the ferrocyanide and ferricyanide salts of plutonium. Strong adsorption of solutions occurred with all these compounds, which made analyses of the Pu/Fe ratio difficult to determine accurately. The results are given below.

Compound	Formula	Color ppt	Solubility in solution in which formed
Plutonyl ferricyanide	$(PuO_2)_3[Fe(CN)_6]_2 \cdot x\ H_2O$	red brown	0.4 g/l
Plutonyl ferrocyanide*	-	-	-
Plutonium(IV) ferricyanide	$Pu_3[Fe(CN)_6]_4 \cdot x\ H_2O$	black	-
Plutonium(IV) ferrocyanide	$PuFe(CN)_6 \cdot x\ H_2O$	black	10.0
Plutonium(III) ferricyanide	$PuFe(CN)_6 \cdot x\ H_2O$	black	0.012
Plutonium(III) ferrocyanide	$HPuFe(CN)_6 \cdot x\ H_2O$ or $KPuFe(CN)_6 \cdot x\ H_2O$	sky blue	0.0088

*No compound. Plutonium(VI) reduced to (IV) by the ferrocyanide.

Anderson stated that the black plutonium(IV) ferrocyanide turned blue on standing. This and its higher solubility would indicate it to have the less stable thermodynamic configuration of the two compounds plutonium(IV) ferrocyanide and plutonium(III) ferricyanide. Work is now being done on the (III) mixed salts of plutonium with alkalies and sulfate. Rubidium produces a precipitate in these mixtures.

Hopkins reported that about 4 mg of pure americium have been obtained by a series of fluosilicate cycles and TTA extractions. It contains less than 10 percent impurity. The TTA extraction gave considerable purification from lanthanum. Some 5.5-6 mg should be available by next Thursday.

According to today's newspaper, Iran says that it has no agreement with Russia. Russia stamped out of the Security Council meeting because it could not gain a delay in consideration of the Soviet-Iranian dispute.

Friday, March 29, 1946

Stephen Lawroski terminated his work at the Met Lab to return full-time to Standard Oil Development Company in New Jersey.

A letter arrived from Jack Marinsky and Cyrus Feldman at Clinton, who ask for information on the measurement of the absorption spectrum of americium. They are studying the properties of element 61 using similar quantities (~100 µg) of material. I immediately replied that I am particularly interested in the spectrum of 61 because of its bearing on the question of the structure of the lanthanide and actinide elements. I then say that I am referring their request to Burris Cunningham for reply.

Ted La Chapelle again wrote, in a letter I received today, about his concern about Everson's offer of only $300 per month while his present salary is $340.

A reply arrived from Iz Perlman to my request for information about the neutron flux at Hanford. His best estimate from the information he has is a flux of 2×10^{13}.

I sent Joe Kennedy a note, enclosing a copy of a letter involving the work on H^3 that we did in 1941. I say that as I recall, I wrote this up and submitted it in the names of Kennedy and Sam Ruben, so I am now turning it over to him (Joe) for action.

In another note I ask Segrè whether he has heard from Colonel Fields about permission to publish our paper, "Properties of 94^{239}."

Eugene Wigner sent me a tentative schedule of the addresses to be given at Princeton's Bicentennial in September. He says that Linus Pauling will lead the discussion of my paper and asks that I send Pauling a copy when it is prepared.

Howard Warrington of Prentice Hall notes that he is pleased to learn from Mr. Enenbach that, at some indefinite date, I intend to write a book on nuclear chemistry. Prentice Hall is interested in publishing such a book, and Warrington would like me to visit them any time I am in New York.

I also read carbons of two letters from Don Stewart; one to Whitaker at Clinton, apologizing for the mistaken recanning and shipping to Clinton of "GTS #125,"--1 mg of protactinium. Stewart did not realize the shield was not empty when he returned it to Clinton. However, he says that we would like the sample, now called "GTS 125-A," irradiated for a period of one month. Another of Stewart's letters went to W. Rubinson in New Mexico, saying that he will be an "observer" at the Crossroads tests. Stewart asks if we will be able to obtain a sample of the cloud material.

In the Chicago Times today there was a report by Howard W. Blakeslee on the AAAS meeting in St. Louis. He says in part, The big fact on which scientists rely to prevent surprise atomic attacks--namely, that all atomic energy is limited strictly to two natural metals, without even a slight prospect of extension--came out here today at the meeting of the American Association for the Advancement of Science.

The metals are uranium and thorium, both scarce and controllable at mines and in power houses, so that scientists believe no nation will be able to prepare for an atomic attack without automatically giving the world advance warning of 18 months to several years.

These facts came from a plan for international control of atomic energy issued simultaneously last night in Washington and St. Louis. Here the plan was explained by Dr. Charles A. Thomas, Vice President of the Monsanto Chemical Company, himself a member of the plan committee and an atomic bomb expert.

Uranium makes both bombs and power. It also makes a new metal, plutonium, for bombs and power. Uranium converts thorium into a new form of explosive uranium. Uranium alone is the base and no other chemical in creation now appears to yield atomic energy.

Important also is denaturation that renders the metals non-explosive, but leaves them useful for power. Denaturation is what happens when an egg is hard boiled. The metals are not boiled; they are denatured by adding a secret chemical element. But, Dr. Thomas said, there is no doubt that "boiling" them will take a long time and large plants.

The proposal is to give an international atomic development authority three controls over: (1) all mining of uranium and thorium, (2) all plants making the metals, (3) inspection of large peacetime power plants (because these could be secretly modified to make bomb materials)...

Helen had a 10:00 a.m. dentist appointment today.

At 4:10 this afternoon I left Chicago on the "400" of the Chicago and Northwestern Railroad (seat 11, car 24), arriving in Ishpeming at 11:35 p.m. I am staying at the home of my aunt and uncle, Minnie and Henry Seaborg (1115 N. Fifth St., corner of Park St.)

<u>Saturday, March 30, 1946</u>

In Ishpeming, Michigan.

This is only the third visit to Ishpeming, my place of birth, since my family--father, mother, sister Jeanette, and I--left in October, 1922, when we travelled by train to Los Angeles, California. There we took up residence in Home Gardens (now South Gate), a suburb of Los Angeles.

I spent the day visiting scenes of my boyhood, including our home at the corner of Seventh and Wabash Streets, the golf course out at the end of Wabash Street where I caddied, the downtown area of Ishpeming, the surrounding hills, etc. I also visited some of my boyhood friends and some of my relatives.

Sunday, March 31, 1946

I spent the day looking around familiar places of my youth and visiting with some of my many relatives, both on mother's and father's side, who also live in Ishpeming, nearby Negaunee (three miles away), and Marquette (ten miles away). In the afternoon we had a large picnic reunion.

Monday, April 1, 1946

In Ishpeming.

Quentin Van Winkle and Walter J. Blaedel are scheduled to terminate in Chicago today. Van Winkle will return to Ohio State University to complete his doctoral work while Blaedel will begin work at the Radiation Laboratory in Berkeley on April 15.

After dinner at the Mather Inn in Ishpeming as a guest of Mr. Carl Brewer, I gave one of the two talks to the Marquette Range Engineers Club. The talk, "Atomic Energy," was arranged by my uncle, Henry Seaborg.

Tuesday, April 2, 1946

I left Ishpeming at 7:10 a.m. on Northwestern RR (seat 10, car 31) and arrived in Chicago at 2:35 p.m. Helen met me at the train station with a freshly packed bag, and Ruth Rogers was also there with a few letters for me to see. Helen mentioned that she had had an appointment with her obstetrician, Dr. Davis, yesterday morning at 8:45. Among the items Ruth brought to the station was a reply for me to sign to Ted La Chapelle which I had dictated to her before I left Chicago. I say to La Chapelle that I believe I have done all I can and that the offer he received was probably the best they can do.

I then went to the airport to continue on to Washington on PCA Airlines, leaving Chicago about 6:30 p.m.

Today's paper reports on the tidal wave, caused by an earthquake, that struck Hawaii yesterday. The big waves also hit the California, Oregon, and Alaskan coasts.

Wednesday, April 3, 1946

At the War Department in Washington. General Groves has called together a group of scientists (L. W. Alvarez, R. F. Bacher, M. Benedict, H. A. Bethe, A. H. Compton, Farrington Daniels, J. R. Oppenheimer, J. R. Ruhoff, F. H. Spedding, C. A. Thomas, W. H. Zinn, and me) to discuss the concept of denaturing plutonium. See Figure 59.

Alec E. Kelley is scheduled to be discharged from the SED today in Chicago. He will be placed on the Met Lab payroll tomorrow as a Junior Chemist at $273 per month.

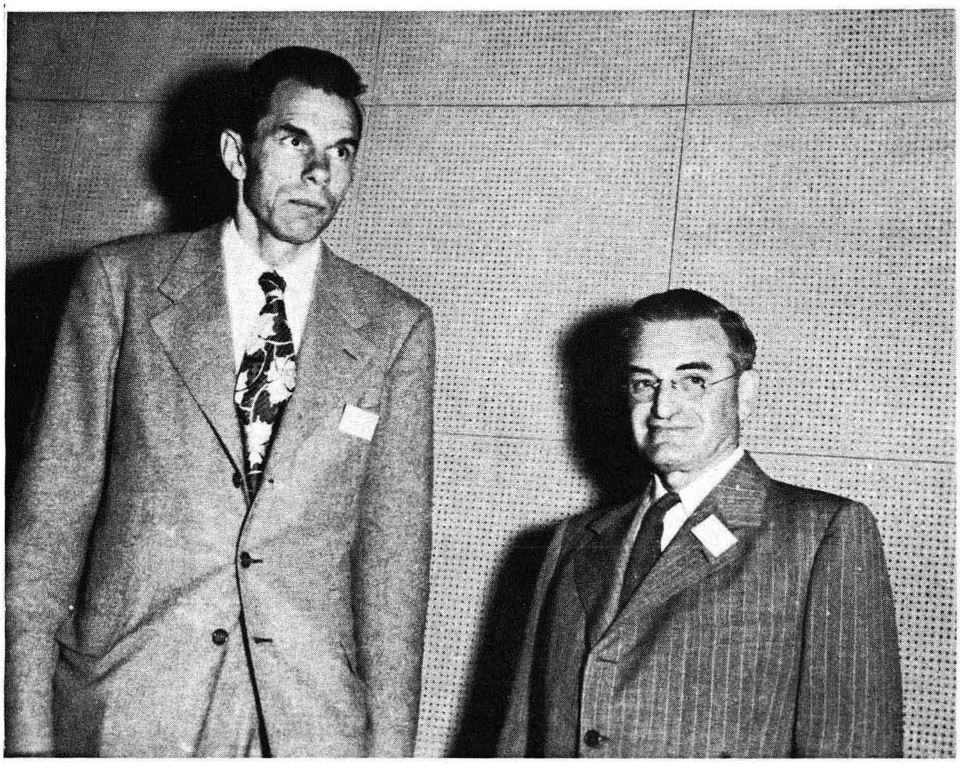


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Figure 59. Glenn Seaborg and Farrington Daniels, Spring 1946.

Thursday, April 4, 1946

I spent the afternoon at the War Department with the committee working on the problem of plutonium denaturing.

Friday, April 5, 1946

At 8:00 a.m. I left Washington by train for Atlantic City. Upon arrival I checked into the Shelburne Hotel.

At 2:00 p.m. I and other members of the Inorganic Panel (W. C. Johnson, chairman; W. C. Fernelius; John C. Bailar; and H. N. Alyea) met with the Committee on the Professional Training of Chemists in the Chart Room of the Hotel Claridge. The Committee agreed to accumulate data and publish a report on the present status of graduate work in chemistry in the United States.

Saul Winstein, Robert Woodward, and I had dinner together. Saul told me that Woodward is one of the most promising young organic chemists in the United States. After dinner I saw Melvin Calvin and Ken Pitzer.

Saturday, April 6, 1946

I called my office this morning, and Ruth Rogers told me that a reply from Colonel K. E. Fields arrived yesterday about the publication of our report A-33 (Properties of Pu^{239}) and my letters incorporated in report CN-1488 (describing the discovery of element 94). Fields says that the contents of A-33 are not declassifiable but my letters are. However, the Manhattan District has no authority over the material in A-33 since the work was done on an NDRC contract. Fields has contacted Dr. Cleveland Norcross of NDRC who wishes to coordinate their policy with that of the Manhattan District. Therefore, both Norcross and Fields believe that A-33 should not be published at this time. Report CN-1488 is declassifiable although it, too, is a report over which the Manhattan District has no control.

Ruth mentioned that she has sent a copy of Fields' letter to Emilio Segrè yesterday.

Jim Crowe and I played twelve holes of golf at Ocean City-Somers Point Country Club (JC-60, GS-53 for nine, JC-78, GS-73 for twelve.)

I am staying in the Shelburne Hotel here in Atlantic City, and before retiring I wrote Helen a letter.

Sunday, April 7, 1946

In Atlantic City for the ACS meeting.

I met Dr. H. F. Jordan, Assistant Manager, Research and Technical Development Department of the United States Rubber Company, who discussed Darrell Osborne's ability as a chemist with me. I told Jordan that I would put my remarks in writing for him sometime after I return to Chicago.

Monday, April 8, 1946

In Atlantic City for the ACS meeting.

At 11:00 a.m. I attended the meeting of the Beilstein-Gmelin Committee at the Marlborough-Blenheim Hotel.

I had a discussion today with Maurits Dekker and E. S. Proskauer of Interscience Publishers, Inc., who would like me to act as editor for a series "Advances in Nuclear Chemistry."

Secretary of War Patterson addressed the opening session of the American Chemical Society here today. He paid tribute to chemists for their help in the war.

Tuesday, April 9, 1946

In Atlantic for ACS meetings.

The State Department released the following report of our meeting at the War Department on the problem of denaturing:

> The possibility of denaturing atomic explosives has been brought to public attention in a recent report released by the State Department on the international control of atomic energy. Because, for security reasons, the technical facts could not be made public, there has been some public misunderstanding of what denaturing is, and of the degree of safety that it could afford. We have thought it desirable to add a few comments on these points.
>
> The report released by the State Department proposes that all dangerous activities in the field of atomic energy be carried out by an international authority and that operations which by nature of the plant, the material, the ease of inspection and control, are safe, be licensed for private or national exploitation. The report points out that the possibility of denaturing

explosive materials so that they "do not readily lend themselves to the making of atomic explosives" may contribute to the range of licensable activities, and to the overall flexibility of the proposed control. The report does not contend nor is it in fact true that a system of control based solely on denaturing could provide adequate safety.

As the report states, all atomic explosives are based on the raw materials, uranium and thorium. In every case the usefulness of the material as an atomic explosive depends to some extent on different properties than those which determine its usefulness for peacetime application. The existence of these differences makes denaturing possible. In every case denaturing is accomplished by adding to the explosive an isotope, which has the same chemical properties. These isotopes cannot be separated by ordinary chemical means. The separation requires plants of the same general type as the plants at Oak Ridge, Tennessee, though not of the same magnitude. The construction of such plants and the use of such plants to process enough material for a significant number of atomic bombs would probably require not less than one nor more than three years. Even if such plants are in existence and ready to operate some months must elapse before bomb production is significant, but unless there is reasonable assurance that such plants do not exist it would be unwise to rely on denaturing to insure an interval as much as a year.

For the various atomic explosives the denaturing has a different effect on the explosive properties of the material. In some cases, denaturing will not completely preclude making atomic weapons but will reduce their effectiveness by a large factor. The effect of the denaturant is also different in the peaceful application of the material. Further technical information will be required, as will also a much more complete experience of the peacetime uses of atomic energy and its economics, before precise estimates of the value of denaturing can be formulated. But it seems to us most probably that within the framework of the proposal advanced in the State Department report denaturing will play a helpful part.

In conclusion we desire to emphasize two points, both of which have been challenged in public discussion. (1) Without uranium as a raw material there is no foreseeable method of releasing atomic energy. With uranium, thorium can also be used. (2) Denaturing, though valuable in adding to the flexibility of a system of controls, cannot of itself eliminate the dangers of atomic warfare.

L. W. Alvarez	A. H. Compton	G. T. Seaborg
R. F. Bacher	Farrington Daniels	F. H. Spedding
M. Benedict	J. R. Oppenheimer	C. A. Thomas
H. A. Bethe	J. R. Ruhoff	W. H. Zinn

Wednesday, April 10, 1946

I gave a few introductory remarks at the Symposium on Nuclear Chemistry at 9:00 a.m. this morning. Willard F. Libby was the first invited speaker; he discussed the subject "Chemistry of Energetic Atoms Produced by Nuclear Processes." Milton Burton then spoke on "Radiation Chemistry." In introducing Burton, I remarked that his work and that of his group was overlooked in the Smyth report. Charles D. Coryell then talked on the subject "Radiochemistry and the Fission Products."

I then presented my talk, "The Heavy Elements." In this talk I made public our suggestions for the names of elements 95 and 96. I said,

> Elements 95 and 96 should of course have names and we propose the following in which these actinide elements are given names by analogy with the corresponding members of the lanthanide earths. We suggest for element 95, with its six 5f electrons, the name "americium," symbol Am; thus this element would be named after the Americas, or New World, by analogy with europium, with its six 4f electrons, which was named after Europe. For element 96, containing seven 5f electrons, we suggest "curium," symbol Cm, after Pierre and Marie Curie, historical leading investigators in the field of radioactivity; this is by analogy with gadolinium, containing seven 4f electrons, which recalls Gadolin, the great investigator of the rare earths.

Thursday, April 11, 1946

Darrell Osborne and I travelled by train from Atlantic City to Philadelphia, where we caught the "Broadway Limited" (roomette 13, car X-10) to Chicago. I am suffering from a stomach upset brought on by some bad food eaten in Atlantic City.

Friday, April 12, 1946

Darrell and I arrived in Chicago in the morning. In the apartment Helen told me about her week. A week ago Wednesday she dropped in on Wilma in the afternoon. They made arrangements to meet Thursday at Fields at 10:30 a.m. and to shop downtown. Helen then had dinner at Wilma's home. Helen had a dental appointment Friday afternoon at 3:30 and dinner with Kathleen Hughes (a friend who works in the Information Division). On Monday of this week Barbara Hull and Helen met at noon at Fields and had lunch together. On Tuesday at 10:30 a.m. Helen had another dental appointment. She spent Tuesday night at Marjorie Osborne's. Wednesday at 12:30 she had lunch at Mrs. Fraser Young's. Yesterday brought another dental appointment at 10:00 a.m., and Wilma came over to sew in the afternoon. Helen then took Marjorie Osborne and the girls out for dinner; she spent the night at their home.

Events that took place in Chicago while I was away include the following:

Saturday, March 30

At my home a letter arrived from John E. Pfeiffer, Science Director of CBS, who announced that CBS is beginning a new science program--15 minutes weekly of news in scientific and medical research. They plan to cover the Gibson Island meeting of AAAS. Pfeiffer realizes that I probably have not prepared my paper for the meeting, but he would appreciate receiving an abstract and any pertinent reprints in advance of the meeting--not for a "scoop," but in order that they will have the background necessary to do an accurate story.

A thank-you letter came from K. Bryson Fleer of the ACS for my letter of March 22 and the copy of my manuscript, "The Heavy Elements." He and Jim Crowe like the prospect of a golf game in Atlantic City, but they must leave the arrangements tentative.

I received a copy of Perlman's follow-up to a letter of mine (MUC-GTS-224). He requested that Donald Cooksey arrange to procure for use in Berkeley approximately 160 g of 600-700 gt plutonium.

My office also received a copy of Kohman's letter to G. L. Martin at Mallinckrodt, saying that we have not yet sent the three emanation chambers because we are working on ways to increase the sensitivity.

Sunday, March 31

Ralph Lapp, Assistant Laboratory Director, prepared a summary for Farrington Daniels of his many objections to the present Argonne site for the successor to the Metallurgical Laboratory from a long-range viewpoint. Lapp believes that a survey should be taken of other available sites near Chicago and presented it to the Regional Board meeting on April 5.

Monday, April 1

Don Stewart sent Perlman a description of material being shipped by courier:

GTS #145 - 800 mg of plutonium, consisting entirely of dry samples prepared by Westrum, and shipped for use by him in his work at Berkeley.

GTS #145-B - 1200 mg of plutonium(IV) nitrate stock solution. This is 171 gt material, and assays about 61.5 mg/ml.

GTS #146 - This is a sample of 8 mg of Pa^{231} which has been prepared as the oxide and dried on a special platinum interceptor target. We would like to suggest that the cooling tubes be snipped after the bombardment to permit shipping of the treated sample back to us. We assume that this will be irradiation 12b-2.

Stewart includes a written copy of the spectrographic analysis (previously given by phone) of Werner's sample of element 95.

Stewart submitted a formal request to Chapman for the bombardment of the protactinium sample (GTS #146) with 500 μah of 22 Mev deuterons. He then requested of L. C. Furney that the remaining 6.0 g of 171 gt plutonium officially charged to the Project office stock be transferred to Section C-I since we are using 171 gt plutonium as standard stock.

Tuesday, April 2

The Laboratory Council met at 9:00 a.m. in Room 209, Eckhart Hall. In attendance were Branch, Chisholm, Cole, Daniels, Dempster, Foote, Furney, Hilberry, Hughes, Jacobson, Jesse, Lapp, Manning, Mulliken, Nickson, Rabinowitch, Willard, Zachariasen, Zinn, and Zirkle. It was announced that the declassification code is scheduled to arrive on April 2.

Daniels said that graphite from Hanford has been received at the Metallurgical Laobratory. He appointed O. C. Simpson to handle and process all requests from local scientists who desire to use the material for research purposes.

It was announced that representatives of the midwest colleges and universities interested in the formation of a regional nucleonics laboratory will meet in Chicago on April 5-6. The group will meet in a downtown location and will choose a Board of Advisors. On Friday, the visitors will be shown through the Argonne Laboratory, and on Saturday, there will be conducted tours through New Chemistry, Ryerson Shops, and the Instrument Section.

R. E. Lapp discussed his formulated plans for the proposed Argonne Nucleonics Laboratory. A topographical map and aerial photographs of the present site have been prepared and will be shown to all members of the Laboratory Council and to representatives of the midwest universities and colleges. Lapp stated that considerable opposition has been encountered with reference to a large building program at

Argonne. Specifically, he pointed out inadequate transportation facilities, lack of gas, insufficient water, and the small area of land has made the site less favorable than possible new sites. Hilberry suggested the possibility of securing land used in the past war by the Ordnance Department of the U.S. Army. He stated that these sites would provide good housing facilities, adequate land, and would eliminate the need of legal transactions needed to secure new land. However, it was pointed out by others that if the laboratory is situated too far from a city on a military reserve it will be subject to the same handicaps to which Los Alamos and Clinton are now subject.

Daniels said that Division Directors of the Manhattan Engineer District will meet at the Metallurgical Laboratory on April 15-16, to develop the future research program. He asked the local Division Directors to submit a list of the research problems contemplated for next year.

Jesse and Wakefield consulted with Hindman about the possibility of plating 37 onto a copper plate for their work.

Thursday, April 4

Burris Cunningham sent Jack Marinsky (with a copy to me) a description of the method he used to obtain the optical absorption spectrum of americium in aqueous solution on the 0.1 mg scale. Cunningham offers to lend Marinsky one of his microcells until such time as Marinsky can get one. Marinsky hopes to measure the optical absorption spectrum of element 61 using this technique.

My office received a copy of a letter from Al Ghiorso to Mrs. Sally Barieau, housing coordinator of the Radiation Laboratory. Al explains that his transfer to Berkeley has been advanced one month and he now would like a house reserved for him, if possible, on June 1.

Friday, April 5

A thank-you note for my talk on April 1 arrived from Peter P. Ribotto of the Marquette Engineers Club. H. F. Beeghly, Pittsburgh Section of the ACS, also wrote, expressing appreciation for my Pittsburgh speech. He encloses three copies of the March issue of The Crucible (the publication of the Pittsburgh Section) and a clipping from the Pittsburgh Press. A check for $45.49 arrived from John R. Bowman also of the Pittsburgh Section to cover my expenses for that trip.

The representatives of midwest colleges and universities are meeting today in Chicago to discuss formation of Argonne National Laboratory.

MUC-FDL-197, "Report for March 1946," was issued today. The work of Section C-I is that covered by Manning in his "Summary of Work from February 15 to March 15" (MUC-GTS-2243) and sent to Hogness on March 25.

Saturday, April 6

Don Stewart wrote to M. D. Whitaker that we have been able to increase our supply to element 95 and therefore we would like to request that a somewhat greater than usual amount (0.1 mg as nitrate) of element 95 be neutron-irradiated at Oak Ridge to produce element 96. The sample is identified as GTS #149.

The meeting of midwest representatives is continuing in Chicago today.

Monday, April 8

Ruth Rogers, my secretary is taking a leave of absence for one week.

My office received a letter addressed to me from Geoffrey Wilkinson who has been working in Chalk River. Wilkinson asks if it would be possible for him to work under my direction in Berkeley on radiochemistry for a year or two if it can be arranged from the security angle. He says that Dr. Cockcroft is willing to give him a leave of absence and he is prepared to come at his own expense.

"The Atomic Scientists of Chicago" prepared a questionnaire on attitudes toward working in the field of nuclear energy in peace time which is being distributed today.

It is designed to determine how many American scientists and engineers will be willing to work under each of the various proposed government controls of science and industry in the nuclear field, if these proposals were enacted into law. One is asked to answer in terms of whether he will or will not be willing to work under given conditions, and not in terms of how he thinks these conditions will affect research and development generally. The questionnaire deals only with the problem of domestic control of nuclear energy before the establishment of international controls.

The Laboratory Council met in Room 209, Eckhart Hall at 4:00 p.m. The following people were present: Branch, Cole, Daniels, Dempster, Foote, Furney, Hilberry, Hughes, Jesse, Langsdorf, Manning, Moulton, Mulliken, Rabinowitch, O. C. Simpson, Zachariasen, Zinn, and Zirkle. Daniels announced that copies of the declassification guide for the Plutonium Project have been received. The guide is classified "secret." All persons interested in having papers published should submit a copy of the material to him to determine if such material can be declassified. Later, the material will be referred to other persons for checking purposes. Manning suggested that persons submitting papers for declassification should state the clauses under which declassification is authorized.

Mulliken spoke about the relationship between the Plutonium Project Record and the release of declassified material. He agreed with many persons that Laboratory personnel are entitled to publish any declassified material in any journal if they so desire, but urged that copies of all papers should be offered for incorporation into the PPR.

Daniels then announced that representatives of 24 midwest colleges, universities, and private research institutions met in Chicago on April 5-6, 1946, for the purpose of organizing the new laboratory which will be known as the Argonne National Laboratory. One representative of each college and university served on the Council which then elected seven persons to serve as an advisory committee. The advisory committee to be known as the Board of Governors consists of the following persons: Dr. A. H. Compton, Chancellor, Washington University (Chairman, pro tem); Dr. Farrington Daniels, University of Wisconsin; Dr. Ovid W. Eshbach, Dean, Northwestern University; Dr. R. G. Gustavson, Vice President, University of Chicago; Dr. F. Wheeler Loomis, University of Illinois; Dr. F. M. Spedding, Iowa State College; Dr. John T. Tate, University of Minnesota.

It was announced that the Argonne Laboratory will be constructed on the site of the present Argonne Laboratory. General K. D. Nichols has given tentative approval for necessary construction, and work for the erection of a shop building will be undertaken soon. The Board of Governors of the Argonne National Laboratory recommended (1) that the University of Chicago act as Contracting Agent for the year July 1, 1946 to June 30, 1947; (2) that Daniels serve as Laboratory Director for as long as he is willing; and (3) that Argonne be retained as the site for the continuing laboratory operations. The Council of the Argonne National Laboratory will meet again in June 1946, and the Board of Governors will meet on May 6, 1946.

<u>Tuesday, April 9</u>

The Heavy Isotopes Group met this morning with Ames, Anderson, Cunningham, Elson, Erway, Fineman, Florin, Hindman, Hyde, Jaffey, James, Katzin, Kohman, Magnusson, Peterson, Scott, Sedlett, Stewart, Studier, Warshaw, and Weissbourd in attendance. Manning opened the meeting by discussing the meeting of the regional University representatives, and saying that Daniels reports that the representatives were quite enthusiastic about the plans. There was agreement that fundamental work should be done here. After July 1, Daniels will remain part-time on the Project. The plan is to move the laboratory to Argonne. There was then a discussion of the pros and cons of keeping the laboratory at Argonne. Cunningham asked whether the facilities will be available to industrial concerns as well as to Universities, and Manning replied that there would presumably be four laboratories: Clinton for industrial development; Chicago for fundamental research with emphasis on piles and pile reactions; Berkeley for fundamental research on high energy particles; and a northeastern laboratory probably to be established in the New York-Boston region.

Manning then announced that in the general program this laboratory is to continue work on solvent extraction no matter what du Pont does. Du Pont has been requested by the Army to put in a solvent extraction pilot plant at Hanford. We will cooperate with Clinton on the decontamination of U^{235} from the Clinton heterogeneous pile. The plan is to have 95 percent U^{235} sandwiched between aluminum and rolled up in a jelly-roll fashion. Fifteen to 20 percent of the U^{235} should be fissioned when the material is to be worked up. James asked about the problems caused by U^{237}, and Manning answered that remote control fabrication has been considered but the uranium will probably be allowed to cool instead. After the uranium has been used three times, a 120-day cooling period will be necessary. Manning, Katzin, and Kohman discussed the economic problem of the uranium being tied up during cooling. Manning said that Np^{237} and Pu^{238} would be produced as a by-product of the pile operation. After a year of operation, substantial quantities of Np^{237} and Pu^{238} could be produced in a full-scale pile. Answering a question by James, Manning said that the short cycle of operations of the pile was chosen because of the possibility of poisons being built up. Later, if possible, a longer cycle could be adopted. The uranium in the sandwiches will be alloyed with a large amount of aluminum. A decontamination factor of 10^6 will be sufficient.

Manning then brought up the problem of handling large amounts of alpha-particle activities in the laboratory. The health physics group has been concerned with the large air counts found in some of the rooms. Stewart said that the problem of solving this difficulty resolves itself into two phases. A temporary solution is necessary first. The air counts in rooms 35, 11, and 30 are probably due to the centrifuge in room 35. The large centrifuge in room 11 will probably be moved back to room 37. Precipitations in milking separations could be made in a jar hooked up to an aspirator rather than in a beaker. Not much thought has been given to permanent measures. Possibly a fume-hood could be re-built resembling a big dry box with work done with rubber gloves. James remarked that that method had been tried in room 4 and found unsatisfactory.

Manning asked what is involved in milking up to 100 grams of plutonium for 95 and uranium. Stewart replied that glass-lined steel tanks are available in sizes down to 3 gallons. It is hard to draw off the supernatant from a precipitate or separate solvent layers if the interface cannot be seen. Manning continued that Nickson is writing a letter saying that gas masks must be used for milking peroxide precipitations over a ½ gram level, and Stewart commented that the gas masks increased the chances of a spill. Stewart said that at Site Y everything has been done with more elaborate equipment and he described an outfit resembling a diving suit which was worn for the work. Cunningham recalled that the laboratory milking was done rather safely except for the dissolving of the metal. Stewart continued that jets are poor to use for small scale separations--siphons could be used for transferring liquids. Cunningham suggested that when the same amount of material is to be milked several times, the siphon could be kept at the same level. Florin remarked that when hoods are used, the hazard from spray is transferred to the street.

Stewart continued by saying that the doorknob to room 11 is highly contaminated. When Ed Holeb goes in to take an air sample, he grabs the doorknob and then grabs the filter with the same hand. Possibly some of the high air counts are due to the contamination from that source. Cunningham said that a test of air in the hood during dissolving of plutonium metal showed several micrograms on the filter. Stewart commented that counts of only tolerance level have been found in air samples from fume hoods where large amounts of plutonium were worked. He continued that possibly the main trouble was neglect of centrifuges. James remarked that the way centrifuges are now, there is no airflow through them but just a slight vacuum in them. Stewart then said that the centrifuge in room 35 is all right but the one in room 11 is bad. There was once a spill in the latter and the material is probably now dried. One bad practice is opening the centrifuge while it is running. Katzin suggested that all this be kept in mind when the building at Argonne is being designed.

Stewart in discussing samples said that he has not yet called Hamilton about the cyclotron targets. One gram sample of thallium and a 5 microgram sample of 95^{241} are being neutron-bombarded at Clinton. Katzin asked why thallium was being bombarded. Manning explained that it was James' idea. Thallium is a mixture of isotopes 203 and 205. It is worth testing Tl^{204}, a three-year isotope formed from 203, for fissionability. Also the bombardment could supply thallium tracer and form pure Pb^{204}. Kohman said that he does not believe slow neutron fission of Tl^{204} to be possible since even fast neutron fission of nearby isotopes had not been demonstrated. He suggested that it would be better to work on fast neutron fission of bismuth, lead, and thallium. Stewart said that the thallium sample will be bombarded two to three months and the 95 will be bombarded until May 15. The milligram of protactinium, from which U^{232} has been separated, has been sent back to Clinton.

Manning remarked that there is a question on the fissionability of Pu^{238}. According to measurements on the Pu^{238} made at Hanford, it might have an 18 barn fission cross section, a 500 barn n,γ cross section (to produce fissionable Pu^{239}), or a combination of lower values for both reactions. Clinton bombardment of Np^{237} will be made to make Pu^{238} free from Pu^{239} for fission measurements. Stewart continued that a 100 microgram sample of 95^{241} was sent yesterday for a month bombardment to make 96^{242} tracer. Cunningham commented that it should be bombarded two months since it would be that long before the group at Berkeley would be ready to work with it. Manning explained the reason for the deuteron bombardment of very pure neptunium. The plan is to make fission measurements on the plutonium isotopes formed. Plutonium isotopes 238, 237, and 236 will be formed in relative amounts of the order of 4, 1, and 1½ respectively. The Pu^{237} is almost certain to undergo fission with a large cross section. Its decay with a 40-day half-life can be followed. Then the possibility of Pu^{236} fissioning can be looked for in a 2.7 year decay of fissionability. If Pu^{238} fissions with a <18 barn cross section, it will give a long-life background. (At this point, Fineman opened the window.) If Pu^{238} undergoes fission, so should Pu^{236}. Three milligrams of neptunium bombarded with 500 microampere hours of deuterons should give 0.002 micrograms of plutonium which will be sufficient for the measurements. However, any Pu^{239} in the neptunium would interfere.

Kohman brought up the subject of helium ion bombardment of uranium. James said that Ghiorso has the samples. The first layer is mostly Pu^{238} with a little Pu^{239} and Pu^{240}. No Pu^{236} was found (produced from an α,6n reaction), but the measurements have not been finished. There was a discussion of the ability to detect Pu^{236} in the sample. The α,6n reaction is more easily observed in thorium since U^{230} is easily detected. Kohman then asked about the possibility of measuring fission of Pu^{241} in these samples, and James replied that he is not sure it will be possible because of the Pu^{239} present. James explained that work on Pu^{241} with the windowless counter will be done at Berkeley.

Stewart resumed his discussion of bombardments by saying that the protactinium target, containing 8 mg, left April 1 for Berkeley. The reason for this bombardment is analogous to that of neptunium but to determine, in this case, the fissionability of uranium isotopes. Uranium-231 with its 4.5-day half-life should be easy to spot. Uranium-232 probably fissions with a 60-70 barn cross section. It will be formed with a higher yield than U^{230}. The thorium metal target was sent in an unmilled condition on April 1. It will be necessary to do some work preparing it in Berkeley before its bombardment. Studier and Hyde will work on that target. Manning explained the interest in this target. Protactinium-233 will be produced by d,p and d,n reactions; Pa^{232} by d,2n; Pa^{231} by d,3n; Pa^{230} by d,4n in relative yields of 4, 1, and 1½, and 0.05 to 0.3 respectively. The Pa^{232} could be measured for fissionability. It may show up clearly because of its short half-life. Protactinium-230 is complicated by formation of U^{230}. Studier said that the protactinium will have to be periodically purified from uranium. (Uranium-233 will grow in constantly from Pa^{233}.) It is also planned to study fissionability of U^{230} from this bombardment. The bombardment is planned to be a one-day bombardment of several hundred microampere-hours.

Kohman then presented his chart, based on the Bohr-Wheeler theory, for predicting fissionability. For each class of isotope (those of even of or odd number of neutrons), the possibility of fission is greater for a given atomic number, the smaller the mass number; and for a given mass number, the possibility of fission is greater the greater the atomic number. The chart suggests experiments. Fissionability should be tested for the following isotopes with odd neutron numbers: U^{237}, Pa^{232}, Ac^{228}, Ra^{225}: U^{237} would probably be difficult to obtain free from other isotopes, Pa^{232} is planned, Ac^{228}(MsTh$_2$) would require a very careful measurement of its parent, and Ra^{225} can be measured after several grams of U^{233} are available. Manning asked what was known about fissionability of U^{236}, and whether any information was obtained from the CW-1 and CW-3 samples. Jaffey and Kohman recalled that there was something peculiar about the fissionability of the uranium samples but it was not interpreted at this time. Manning then asked about the milking of uranium from the 100 grams of plutonium and learned that Asprey had worked on it but no one knew whether he had finished before leaving. Manning said that the plutonium that came with recent Hanford neptunium would probably be good for milking U^{236} since it is the highest gt plutonium that we have. It would give 20 percent U^{236}.

Hindman reported on the neptunium from Hanford. It was separated from the plutonium by bromate cycles. The yield will be known soon. Jesse will get 2.5 milligrams for preliminary work on a fast neutron counter. Clinton has requested 20 milligrams, but it may be necessary to reduce this.

Cunningham talked about the 95^{241} work. Eight milligrams have been separated. The final purification used TTA. According to spectrographic analysis, it contained 3 percent lanthanum and less than 0.2 percent other elements. A specific activity determination on a 4 mg sample gave a half-life of 513 years, while a recalculation of the old value determined on 10 micrograms gives 498 years. He estimated that his recent value is probably good to 5 percent.

Ames reported that he has been making measurements on a radium sample in the argon counter. He finds that there is no diffusion of radon from the sample, but some is lost by recoil -- about 0.6 to 0.8 of the radon remains. The growth is governed by the accurately known half-life of radon.

Fineman said that he has performed an experiment using some six to eight-months old U^{233}. The sample, collected by the emanation technique, gave a good half-life for Pb^{212}(ThB). To eliminate this, the apparatus is being rearranged to freeze out the thoron. No alpha branching of Ra^{225} was detected by this experiment.

Peterson reported on measurement of the alpha branching of actinium. Growth of alpha activity in a freshly purified sample was used to measure the total amount of actinium present. In the same sample the alpha particles due to actinium were counted with the pulse analyzer. Actinium decay is 1.25 ± 0.2 percent by alpha emission. He voluntered to show the algebra used in the calculation but since the meeting was running rather long, no one showed interest.

Anderson next talked about the preparation of $RbPu(SO_4)_2 \cdot 4H_2O$. Analysis showed 13.3 percent rubidium, 40.4 percent plutonium, and 33.1 percent SO_4. The values calculated from the formula are 14.5, 40.6, and 32.6. It is a light lavender-blue crystalline solid; a previously known compound of similar formula is $NH_4Ce(SO_4)_2 \cdot 4H_2O$.

Stewart made a number of announcements to conclude the meeting. The section has the use of the part-time technician; she is at present being supervised by Mrs. Warshaw and will be available for cleaning platinum plates, pipettes, etc. Gumersoll wants to give one room each week a very thorough cleaning. He is working in Room 36 today. The crew uses TSP on the walls. This is in addition to the regular weekly cleaning. There is a memo from Furney that Homer E. Brown is available for fixing all types of meters. Kohman asked whether this included thermo-meters. As the group left the room, Sedlet was heard to remark that he had a broken meter stick.

K. T. Compton was in Chicago today and predicted that atomic energy will be harnessed for civilian use in five to ten years. He thinks the probable first use will be to propel warships and, after that, cargo and passenger ships.

Wednesday, April 10

Stan Thompson wrote an extensive letter to Iz Perlman with some last minute questions. He describes the status of the papers and notes that he is turning them over to Tom Jones for proper handling. Some drafts, Stan says, will have to be finished in Berkeley. He then asks when Tom Jones should visit Berkeley in order to help finish the volumes. Stan goes on to describe the work and method used to isolate some of the 51 from the barrel obtained from Los Alamos and suggests Perlman officially request 3 to 4 mg of 51 in the form of cruds and about 3 mg of pure 51. In addition, Stan asks if there is anything he can bring or have shipped to Berkeley in order to expedite the work there.

The "Suggested Program for Chemistry on Project W-7405" (Berkeley) arrived from Latimer who includes the following:

Basic chemistry of plutonium, uranium, and thorium. We have a group of five or six men who are now working on a number of problems such as the application of the fluorodiketones to solvent extraction separations, equilibria involving the various oxidation states of plutonium, peroxide complexes of plutonium and uranium, potentials of the U^{+3}-U^{+4} couples, thorium fluoride complexions. Problems of this type should be continued by the men in this group as they are all experts in this field and are familiar with all the previous work and well informed as to the needs for further developments.

High temperature thermodynamics as applied to pile problems. This laboratory has maintained leadership in the application of thermodynamics to chemical engineering problems. We now have under way research on the following subjects: intermetallic compounds of uranium for the purpose of determining the solubility of uranium in mixtures of sodium with tin, bismuth, and other metals with relation to fast neutron pile materials; gaseous equilibrium and vapor pressures by the flow method of metals and refractories; vapor pressure of graphite; heats of formation of $TaCl_5$ and other halides by the hot wire method; high temperature calorimetry to determine the heats of formation and stability of intermetallic compounds; heats of formation and free energies of plutonium and uranium compounds.

Chemistry of heavy isotopes. Data on the heavy isotopes which may be found in pile operations are obviously essential to pile designs. The work of Dr. Seaborg and his group speaks for itself.

Application of tracers to medical and biological problems. Since carbon 14 was discovered in this laboratory, it is only natural that we should have a number of men who are far advanced in the application of carbon 14 to medical and biological problems. We have a well organized group of "synthetic" organic chemists who have solved many of the problems of the synthesis of intermediate organic compounds from radioactive carbon dioxide and have an excellent start on some of the fundamental problems being investigated by Dr. John Lawrence.

Thursday, April 11

The Solvent Extraction Group met today. Attendees at the meeting were Ader, Casler, Davis, Hausman, Hyman, Leader, Manning, Nachtman, Post, Schaffner, Schraidt, Sheft, Turk, and Wagner. Manning announced that Argonne was definitely picked as the site for the permanent laboratory. He said General Groves has requested du Pont to install a pilot plant for solvent extraction at Hanford, and a solvent extraction program will continue here at Chicago for an indefinite period.

Hyman described the following first cycle runs made since the last meeting. One of the variables investigated was the acidity of the IAF stream.

A possible explanation for the effect of acidity can be postulated. For uranium, tests have shown a decrease in interfacial tension with higher acidity. Thus the smaller drops give better contact and effectively more plates. The plutonium case is not this simple. The decreased interfacial tension should decrease losses. The oxidation of Pu(IV) to Pu(VI) is rapid in the presence of uranium and hexone. Most losses of plutonium in column IA are due to persistence of Pu(IV) in the extraction portion of the column and its formation in the scrubber section. Higher HNO_3 may result in higher steady state nitrite concentration and therfore more Pu(IV) formed in the scrubber section. A more rapid extraction of uranium in the bottom of column IA may reduce the rate of reoxidation. Obviously this is sheer speculation, but the factors mentioned may well play an important part in determining the observed results.

The following second cycle runs were also made.

Run	Pu Losses IIAW	Pu Losses IIBW	Decontamination β	Decontamination γ	Crossover Oxidation
11P	0.7%	0.8%	7×10^2		0.5 M HNO_3, 1 hr. air sparge at 100°C
8F					0.1 M $Na_2Cr_2O_7$, 1 hr. at 100°C
12P	0.3	0.015			1 M HNO_3, 3 hr. air sparge at 100°C; 0.1 M $Na_2Cr_2O_7$, 3 hr. at 100°C
13P	0.4	0.01	10^2	10^3	0.5 M HNO_3, 3 hr. air sparge at 100°C
9F					0.1 M $Na_2Cr_2O_7$, 2 hr. at 100°C

Run 11P8F is invalid because NH_4NO_3 was found in the IIBP column.

Leader reported that further tests have been made on the removal of activity from ICU solutions by manganese dioxide. The manganese dioxide is precipitated by addition of $KMnO_4$ solution to the hot ICU solution at an acidity of 0.1-0.2 N.

Ader described batch experiments and column runs that show the influence of acetate in reducing plutonium losses in column IB. In the event that acetate is included in the IBS stream, the resultant IBP solution will contain a considerable concentration of acetate that must be removed before processing in the second cycle. Acetate in the second cycle is quite harmful to plutonium recovery. Laboratory scale experiments were run to test the feasibility of removing acetate from IBP solutions by distillation before the crossover oxidation. When synthetic IBP solutions are used, the HOAc was found to distill at a constant rate. Air sparging and a temperature of about 100°C were employed to effect the distillation. Ninety-six percent of the HOAc was removed if air sparging and boiling were employed.

Sheft conducted experiments to test plutonium recovery in the second cycle with IBP solutions from column runs in which no acetate was present. The results indicated favorable recovery in the IIA column for all cases, and normal distribution ratios in the IIB column for all cases.

Wagner said that better separation has been observed in column IA when the HNO_3 in solution IAF is increased to 1 M. The column behaved as though one or more plates had been added. This indicated better contact or reduced interfacial tension between the two phases. Consequently experiments were conducted giving a rough comparative measure of the interfacial tension of the system made up of various concentrations of the components of column IA. The number of drops of an equilibrated hexone solution from a constant volume which rise from the pipette through the aqueous phase is a measure of the interfacial tension. Systems examined were hexone and aqueous with various concentrations of HNO_3, NH_4NO_3, and $UO_2(NO_3)_2$. The results were as follows:

UNH - large decrease in interfacial tension with increasing concentration.

HNO_3 - moderate decrease in interfacial tension with increasing concentration.

NH_4NO_3 - small increase in interfacial tension with increasing concentration.

Hausman described preliminary experiments carried out to determine the feasibility of a solvent extraction process for the recovery of U^{235} from the Clinton U^{235} heterogeneous pile. Tentatively a feed containing 1 M $Al(NO_3)_3$ and 3 x 10^{-3} M UNH was chosen. This corresponds to the ratio of the metals in the spirals. For these experiments the UNH came from our usual dissolver solutions. The following results were obtained by contacting the above feed with hexone of various HNO_3 concentrations:

HNO_3 in Hexone	Feed	UNH D.R. $\frac{hex.}{aq.}$	D.F.
0.1 N	3×10^{-3} M UNH 1 M Al^{+++}	4.2	337
0.5	"	10.5	290
1.0	"	11.6	284
1.0	3×10^{-3} M UNH 1 M Al^{+++} 0.05 M Fe^{+++}		428

A simulated column run was made using the following solutions:

Feed: 3×10^{-3} M UNH, 1 M Al^{+++}

Scrub: 1 M Al^{+++}, 1 M HNO_3

Hexone: 0.5 M HNO_3

The results are given in the table:

Solution	D.F.	U not extracted
10 ml Feed + 5 ml Scrub + 3-5 ml hex.	158	8%
Hexone from above + 3-5 ml Scrub	384	

Since the photometric method for uranium analysis used here is not very accurate at the concentrations used, the loss may be less than 8 percent.

* * *

I received a phone call from Ernest Lawrence, who told me that funds have been acquired from Rockefeller for the construction of a hot laboratory building for our group of nuclear chemists at the Radiation Laboratory. He also said that Eastman Kodak is planning to assign two men to the Radiation Laboratory. One, Amos Newton, worked with Spedding and will be assigned to work with me. The other will be a physicist. The initial assignment period will be five years. Eastman Kodak will also assign two men to work with Melvin Calvin. Lawrence and I then briefly discussed the program for our group of nuclear chemists.

A letter arrived for me from Iz Perlman, enclosing two copies of Chapter 9 (The Chemistry of Elements 95 and 96) for Volume 14A. He notes that it turned out considerably longer than he had estimated and that, if everyone estimates so poorly, some new techniques in book binding will need to be developed. Iz asks that I have someone familiar with adsorption, such as Schubert, go over that section.

In administrative matters today, W. M. Branch confirmed an irregular time schedule for Kay Florin and announced a revision in the telephone service. I. J. Schaffner sent Branch a clarification of the time schedule for the operating group at West Stands. The group now consists of the following men: J. H. Schraidt, H. Evans, R. G. Post, J. S. Billheimer, John Casler, A. E. Kelley, V. Cavataio, and J. V. Natale.

When I returned this morning, I learned that yesterday's newspaper carried the story of elements 95 and 96 as I described them at the Atlantic City meeting of the American Chemical Society, giving their names--americium and curium.

Saturday, April 13, 1946

Sigfred Peterson terminated work at the Met Lab today. He will return to the University of Minnesota to complete his graduate work.

A very pleasant personal letter, dated April 9, arrived from Fred Albaugh, who is happy with the trend toward a more liberal nuclear energy bill. Fred goes on to say,

> Before reading the current issue of Time, I had come to the following conclusion about the "denaturing" of plutonium: the idea must be to form a high percentage of Pu^{240} by long pile bombardment. However, since any nation that could construct an atomic bomb could also separate isotopes, the overall scheme is a half-truth concocted by the scientists to fool the May-Johnson crowd, on the thesis of the end justifying the means. Or do I give the scientists too much credit in assuming that they have learned so soon to fight politicians with their own tactics?

In a less serious vein, Fred remarks about his golf:

> My game has gone from rancid to putrid. I have gotten into a monstrous, unbreakable habit of reaching out at the top

> of the backswing and then cutting across the ball toward the inside of flight. The angle is proved by observation of the invariable divot, which is really a deep gash in the ground rather than a respectable displacement of turf. Those who have seen me play lately say I resemble a baloney slicing machine. Starting next Saturday I take lessons.

He then suggests a golf reunion when I visit Los Angeles.

D. P. Ames, J. Sedlet, H. H. Anderson, and T. P. Kohman have now completed a detailed paper, "Rapid Radiometric Assay for Radium, and Application to Uranium Ore Process Solutions," which I read today.

Manning, Stan Thompson, Hagemann, and I played 18 holes of golf at Cog Hill Course No. 2. Winston and Stan won a "low ball plus low total" match, 8 and 4 (WM-121, ST-94, GS-103, FH-101).

Sunday, April 14, 1946

Helen and I had breakfast in the Anchorage of the Windermere West Hotel with Stan and Alice Thompson and Winston and Dorothy Manning. Later in the day Melvin Calvin visited Helen and me.

Today's Sun carries an article about Franco's suggestion that friendly nations investigate the charge that Germany was working on development of an atomic bomb in Spain. Poland and Mexico are opposing Franco because of the restrictions Franco is imposing on the investigations.

Monday, April 15, 1946

Larry Magnusson terminated today to go to Berkeley. There he will continue with his graduate studies and work at the Radiation Laboratory. Rachel Gilbreath, who has been a technician with Health Physics, transferred to Section C-I today.

Elton Turk has been visiting the Met Lab for several days in order to study and become familiar with our solvent extraction program. He plans to set up similar laboratory experiments at Oak Ridge later.

I attended and talked at the meeting on the future program for the Metallurgical Laboratory and our research group at Berkeley. I presented a list of research investigations suitable for the future chemistry program: the chemical and nuclear properties of the transuranium elements with identification of the isotopes produced through cyclotron bombardment, investigation of americium working with the pure element, the isolation of curium in pure form, the separation of americium from curium, the production of metallic americium and metallic neptunium, the search for additional transuranium elements through the bombardment of americium with neutrons and helium ions and plutonium with C^{12} ions, the chemical properties of neptunium and protactinium, a search for transuranium elements in nature, the chemistry of protactinium, the investigation of heavy isomers, the radioactive properties of heavy isotopes, the investigation of spontaneous fission, spectrophotometer properties, nuclear spins and so forth of americium, neptunium, and protactinium, the bombardment of bismuth, neptunium, and protactinium, the bombardment of bismuth with helium ions and deuterons to produce isotopes of elements 85 and polonium, bombardment of separated lead isotopes with helium ions and deuterons to produce isotopes of bismuth, the identification of reactions and products from the bombardments with 200 Mev deuterons and 400 Mev helium ions and protons in the 184-cyclotron and from 300 Mev electrons and gamma-rays in the Berkeley synchrotron, the bombardment of separated isotopes, the basic chemistry of the rare earth elements, special chemical separation methods, and isotope separation by solvent extraction methods.

An item in today's newspaper quotes the Premier of Iran on the progress of the evacuation of Soviet troops from some parts of Iran.

Tuesday, April 16, 1946

Ralph James terminated today. He will go on the payroll at the Radiation Laboratory in Berkeley approximately May 1 and attend graduate school there to work for his PhD degree.

Last night in Room 35 a 250 ml glass centrifuge cup containing about 2 grams of plutonium hydroxide and covered by a fairly large volume of supernatant exploded, shattering the cup and cracking a second cup containing a similar quantity of material. No one was present at the time of the explosion and the hood doors were closed confining the greater amount of the material to the hood. This morning when an attempt was being made to clean up the remainder of the material a beaker containing about 1 gram of hydroxide detonated upon being picked up. Fortunately the chemist was wearing a mask and protective clothing at the time and suffered no injury other than a few drops of solution on his arm. The cause of the explosion is thought to be due to the presence of silver which could have formed an explosive nitride, amide, or azide since there was some ammonium ion present.

I read a copy of a memo from Nickson to Hogness, dated April 12, recommending that a specially prepared hooded area be provided for manipulation of large quantities of short-lived alpha active materials as soon as possible.

I again attended a meeting at which the future programs for the Met Lab were discussed.

Wednesday, April 17, 1946

While I was away from Chicago an invitation arrived from Karl K. Darrow asking if I would be in Chicago and available to speak on June 20-22 when the "Summer Meeting in the East" of the American Physical Society is held. He believes physicists, too, have a right to hear about the new elements. Today I wrote him saying that I will be in Chicago on June 22 on my way home from the AAAS symposium on Gibson Island and will be willing to speak late Saturday morning on the transuranium elements.

I sent Dan Wilkes of the Public Information Department on the Berkeley campus, copies of the plutonium discovery papers which he requested in a letter I received Monday. I mention that I will be moving to Berkeley about the middle of May.

In answering another request I mailed John E. Pfeiffer, Science Director of CBS, an abstract of my Gibson Island paper and other reprints on the same subject. I say,

> I believe that you are right in believing that the story of good research and scientific method can and should be popularized. Your proposed method of accomplishing this is an essentially new one and I shall watch it with interest.

I sent Dr. Jacob Sachs, in response to his request, a reprint of my Chemical Reviews article "Artificial Radioactivity" and say that I am looking forward to seeing his review article on radioactive tracers in biology.

Alfred B. Garrett of Ohio State University wrote to request information on the chemistry of the transuranium elements to be used at a symposium on artificial radioactivity at the May meeting of the Ohio Academy of Science meeting in May.

I also received and read a copy of Kohman's letter to G. L. Martin of Mallinckrodt, in which he encloses diagrams of the emanation chamber. Kohman describes the adaption that must be made by Martin and says that he will send three chambers shortly.

On the front page of this morning's newspaper there is an item about a man who claims to be the checker champion of the world. He says he has a new "atomic move" that he will not describe but states it is his secret weapon.

Thursday, April 18, 1946

I answered the letter of April 2 that I received from Geoffrey Wilkinson of Chalk River. I say that we will be glad to have him work with us in Berkeley although, I warn him, space and facilities are crowded. I also mention that it is well he is prepared to do this at his own expense as I am not certain we can arrange for financial aid through the University.

Roy Thompson gave me a carbon of a letter which he wrote to George Everson. Thompson's concern is that Berkeley apparently makes no subsistence allowance for one's family while one is seeking permanent housing. He notes that the Met Lab has always made such an allowance.

I also read Iz Perlman's reply, dated April 15, to Stan Thompson's questions. Iz suggests Stan obtain a list of all references he will need for his writing since he will be limited to the use of Latimer's reports. Iz also asks for information on the procurement of micro-cells for the spectrophotometer.

In the late afternoon Helen and I went shopping in the Loop, had dinner out, saw the movie, "The Outlaw" at the Oriental Theater on Randolph at State Street. We then saw Jane Russell and Beatrice Kay on stage, along with Borah Minevitch and his Harmonica Rascals. This was in celebration of my birthday tomorrow.

José Iturbi's daughter committed suicide in Beverly Hills last night, according to this morning's paper. Iturbi is a famous pianist with fascist leanings. His daughter, who resided in Iturbi's home, had been going through difficult divorce and child custody hearings. Iturbi had gained custody of his grandchildren.

Friday, April 19, 1946

This morning I attended the meeting of the Heavy Isotopes Group in my office, now adequate in size because of a somewhat shrunken group. Others in attendance were Britain, Erway, Fineman, Florin, Fried, Ghiorso, Hagemann, Hindman, Hopkins, Hyde, Jaffey, Katzin, Kohman, Manning, Osborne, Sedlet, Stewart, Studier, R. Thompson, S. Thompson, and Weissbourd. I announced that Rubinson is recruiting radiochemists for the Bikini test work at Los Alamos and Bikini. All travel expenses will be paid as well as present wages, plus a $200 per month bonus and an extra expense allowance at Bikini. There is no guarantee, however, that those going to Los Alamos will actually take the trip to Bikini. It is also true that all who accept a job must agree to stay on the job until October 1. Florin said he is expecting to volunteer for this work, and Britain is interested but will first have to consult his wife.

There was then a general discussion of the Tolman declassification report with a general feeling of dissatisfaction. It was pointed out that although material may be listed in Class I as immediately declassifiable, this does not mean that you can start talking about it. The procedure to be followed is to write a paper which must then be cleared through a very complicated and unimaginative process involving many readers and referees, many of whom will not understand what the paper is all about. It was generally agreed that we should continue to place primary emphasis on the completion of the PPR and press for as early a release on each volume as is possible. It was generally agreed that publication by a reputable publishing house with scientific experience is to be preferred to the U.S. Government printing office, even though the price per volume might be somewhat higher under these circumstances.

I then announced that I am leaving in four weeks and I feel that all the chapters for survey volumes should be finished by that time.

Fried announced that Zachariasen has obtained excellent x-ray patterns for the dioxide and trichloride of americium. The lattice constant for the dioxide is 5.377 ± 0.002 A. This is slightly smaller than the constant for plutonium dioxide which is 5.38 A. The situation is confused by the fact that the lattice constant for the chloride is a little bigger than that for plutonium. This may be due to the presence of lanthanum, but he does not think this is probable.

Manning announced that Dr. Nickson is very concerned about the recent outbreak of alpha contamination as the result of the large amounts of material with which we have been working. Stewart said he is thinking about setting up semi-permanent large-scale facilities for handling such materials and Dr. Nickson feels that no further work with amounts equivalent to one-half gram of plutonium or more, should be allowed until such facilities are available. Stewart said such large-scale work should be done in completely closed fumehoods working through sealed-in gloves as in a dry box. Operations which will require the handling of large quantities of material include: the milking of plutonium for americium and uranium, the processing of Np^{237}, recovery runs, and the final stages of plutonium recovery. The chief danger results from air contamination which has recently been as high as 40-50 times tolerance. Peroxide precipitations are the chief offenders in this respect. It was suggested that such procedures could be perhaps entirely replaced by solvent extraction processes with not too much increase in difficulties of handling and considerable increase in safety.

Ghiorso reported briefly on his recent activity at Argonne. He has now perfected his fission counting to the point where an amount of Pu^{239}, corresponding to one alpha count, will give 150 fission c/m. The background can be reduced to 10-50 c/m. Preliminary results of recent measurements give the following fission cross sections: Th^{228}, 1 barn; Cm^{242} in americium-curium mixture (made in Clinton, approximately 60 percent curium), 900 barns; Cm^{242} in americium-curium mixture (old, mostly curium), 1400 barns; Th^{227}, 620 barns; Ac^{227} (10^4 micro-micrograms), 2 barns or less. He said that starting Sunday afternoon, we will have the use of the Argonne pile on a 24-hour basis, if desired, for the next week. It was agreed that there should be a meeting in the afternoon to coordinate activities of those working on the targets which will arrive Saturday evening and those who will be measuring samples derived from these targets at Argonne. Measurements to be made will include fission cross sections on the uranium and protactinium fractions from the thorium plus deuterons target, and the uranium fraction from the protactinium plus deuterons target. These measurements will have to be repeated at intervals to distinguish between the various isotopes present in each fraction. A new curium-americium (Cm^{242}-Am^{241}) fraction purified by Hopkins; an Am^{241} fraction; an Ac^{227} sample; a Th^{228} sample; and some other old samples will be measured also. Platinum plates are now being gold-plated for use in fission measurements. This procedure has been found to reduce the background of fission counts. Analysis by Price indicates that the aluminum we have been using contains 1 ppm uranium and lucite contains 0.002 ppm uranium in agreement with the deductions from fission counting.

Hindman announced that he will have the Np^{237} target ready for shipment to Berkeley tomorrow or Monday. Manning suggested bombarding a U^{238} backing target at the same time as the Np^{237}. There was considerable discussion of this and of the possibility of measuring other neptunium fission cross sections before the growth of plutonium would obscure them. Hopkins would be the obvious choice to work on the target with someone else to assist him.

I suggested someone should work on the problem of the separation of UX_1 from our pure U^{238}, its purification from uranium to the greatest extent possible, and the measurement of its fission cross section and the fission cross section of its U^{234} daughter.

Studier announced that he has obtained a value of 0.04 second for the half-life of Em^{218}. He used a method involving the usual observation of oscilloscope pulses.

A letter arrived from Edwin O. Wiig of the University of Rochester in which he states that the Rochester Section of the ACS voted unanimously to invite me to present the Harrison Howe Lecture for the year 1946-47. This is the second lecture in this series Linus Pauling was selected as the first lecturer. Wiig suggests that the lecture be given in the late fall although the date is somewhat flexible. It carries an honorarium of $200.

In a note to T. Agazim, I verified the arrangements of my talk to the American Association of Brewing Chemists on May 7 in Milwaukee.

James B. Parsons, Woodrow Wilson Branch of Chicago City Junior College, sent me a thank-you note for the up-dated reprint of the "Table of Isotopes." He includes an old copy of a Bohr-type periodic table, saying that the four new elements when placed in the table, fall naturally into the places I indicated in my recent paper.

I received a letter from Alexander Gabriel of New York, asking if the rays from fission could demolish weapons made of steel, if the rays would be destructive on living organisms, and if the long-heralded "death ray" is about to become a reality.

Stewart received a negative reply from Furney to his request that the remaining six grams of 171 gt plutonium be transferred to Section C-I. Furney says that it is the opinion of Colonel Arthur H. Frye and other authorities that this plutonium be preserved and not used for research purposes.

W. M. Branch announced that the routing of the campus intersite station wagon will revert to the customary plan and stop at the Eckhart entrance on University Avenue instead of at Ryerson. The latter stop caused hazards to pedestrians.

Helen had lunch at the home of Mrs. Farrington Daniels. She then stopped to see Alice Thompson to say good-bye. The Thompsons are leaving Chicago tomorrow so Stan can start his graduate work for the PhD degree at Berkeley.

The League of Nations ended tonight and willed its assets to the new United Nations organization, according to today's paper.

Saturday, April 20, 1946

Stan Thompson terminated at the Met Lab today. He and Alice will drive to California via a southern route. They will spend some time in San Diego and Los Angeles before going to Berkeley around the middle of May.

Today Manning prepared "Summary of Work of Nuclear Chemistry Section for Period March 15, 1946 to April 15, 1946" (MUC-GTS-2290) for Hogness. The following work is covered:

Recovery of Americium from Los Alamos Residues. In the time interval between the final peroxide precipitation of plutonium at Hanford and subsequent further purification at Los Alamos, americium ($_{95}Am^{241}$) is formed in the plutonium as a result of beta decay of the long-lived Pu^{241}. In the ether extraction step of the Los Alamos purification procedure, this americium remains in the aqueous salt layer and is thus separated from the bulk of the plutonium. Fifty gallons of aqueous calcium nitrate solution representing the combined salt layers from the processing of a number of batches of plutonium at Los Alamos, have now been processed for americium at Chicago. A number of milligrams of pure americium has been obtained from this material. The fifty gallons of salt solution was approximately 2 M in calcium nitrate. In addition, it contained of the order of 100 grams of lanthanum, 100 grams of iron, a number of grams of plutonium and silicon, and appreciable amounts of thorium, lead, chromium, and manganese. Many other elements were present in small amounts.

In brief outline, the procedure for recovery of americium was approximately the following: several hydroxide precipitations with an excess of ammonium hydroxide for separation from calcium; several fluoride oxidation-reduction cycles to remove plutonium, iron, and chromium; sulfide precipitations from acid solution to remove lead, bismuth, mercury, and tin; leaching of hydroxide precipitates with 45 percent potassium carbonate to remove residual calcium; fuming with HF and perchlorate to remove silicon (at this point a large excess of lanthanum was the principal impurity present with the americium); a number of fluosilicate cycles for partial separation of americium from lanthanum and further separation from other impurities; finally, two successive extractions with TTA (thenoyl trifluoracetone) at pH 3.5 for further americium-lanthanum separation.

The actual procedure was much more involved than that outlined above, in that considerable recycling was necessary. Spectographic analysis of the final americium showed that the only appreciable impurity was 3 percent lanthanum.

In the course of the final isolation of the americium, it was observed, in confirmation of an earlier observation on a microgram scale, that a reddish-brown, higher valent americium hydroxide precipitates from alkaline solution in the presence of hydrogen peroxide. A specific activity determination was made on a four-milligram sample of americium with a resultant calculated half-life for Am^{241} of 513 years (±5 percent).

Most of the americium is being bombarded in one of the Hanford piles to produce macro quantities of curium (${}_{96}Cm^{242}$).

<u>Search for "Neptunon" (Em^{221})</u>. By means of the emanation counter developed for radium assay, a search is being made for Em^{221} among the decay products of a sample of 140 mg of U^{233} which has stood for eight months. This noble gas isotope is expected as a result of rare alpha branching of Ra^{225}, which might be expected to emit roughly 10^{-4} as many alpha particles as beta particles. Because of the presence of considerable U^{232} activity and consequently of thoron (Em^{220}), interference from the latter is eliminated by freezing out the emanation, allowing the 55-second thoron to decay, and then

evaporating the emanation into an ion chamber. Preliminary measurements indicate a probable upper limit to $\alpha:\beta$ branching ratio of Ra^{225} of 5×10^{-4}. Further experiments are expected to lower this limit or to reveal the presence of Em^{221}, which is expected to have a period of the order of an hour.

Uranium Ore Work. Preliminary measurements of the protactinium and ionium (Th^{230}) content of samples from the Mallinckrodt pitchblende extraction pilot plant indicate that a large fraction of the protactinium goes into the gangue-lead-sulfate residue, which contains radium, and that a large fraction of the ionium stays with the uranium up to the ether extraction. Presumably, ionium ends up in the residue of ether-insoluble impurities. Quantitative data are lacking because of failure to receive suitable samples of process solutions and residues.

Redox Solvent Extraction Process. [I - first cycle; II - second cycle; A - first column; B - second column; C - third column (first cycle only); S - scrub; U - uranium effluent; P - plutonium effluent; X - extractant; W - waste effluent.] Several additional first cycle runs have been made with various concentrations of acetic acid in the IBS stream. These verified previous data which indicated that acetic acid lowered plutonium losses in the IBU or hexone effluent from column IB. The improvement was shown to be definite but hardly great enough to warrant the use of acetic acid, since attempts to remove the acid from IBP solutions by distillation prior to oxidation for second cycle feed showed this to be difficult. Prolonged distillation is required, and a precipitate was formed in at least one experiment.

Uranium losses in column IA have been rechecked in a series of runs, and found to be of the order of 1 percent with a feed 0.3 M in HNO_3, and 0.1-0.2 percent with 1.0 M HNO_3. Laboratory data showed this effect to be due not only to poorer distribution ratios but also to higher interfacial tension (less dispersion of hexone phase) at the lower acid concentration. No definite improvement was noted when a 3-inch diameter mixing chamber, with or without baffles and equivalent to five feet of column, was inserted in column IA at the feed point. Less variation of the interface was noticed. Use of stainless steel packing instead of glass packing, in column IA as in column IB, has no apparent effect on either plutonium or uranium losses.

A first cycle run was made using a wetting agent, Duponol LS, in various concentrations in the hexone scrub of column IB. Uranium losses in IBP solution appeared lower than in runs without wetting agent. Plutonium losses in IBU solution seemed lower, but results were inconclusive since the decreased interfacial tension caused near-flooding conditions in the extractor section of the column throughout the run.

Sym-diphenyl carbazide, substituted for hydrazine as a holding reductant in column IB, failed when used in BIS and IBX solutions, and also in IBS solution alone in conjunction with N_2H_4 in IBX solution.

Five second cycle runs have been made, in which the HNO_3 concentration and time of oxidation of IIA feed solution was varied. A tentative final procedure appears to consist of heating IBP solution made 0.5 M in HNO_3 for three hours at 100°, followed by adding $Na_2Cr_2O_7$ to 0.1 M and heating an additional one to two hours. Loss of plutonium in IIAW solution with this procedure is now 0.3-0.4 percent. IIBW solution losses are less than 0.1 percent.

A series of flowsheet runs through both cycles, in order to establish recoveries and decontamination more exactly, have been partially completed, with results in good agreement with earlier data.

Stewart wrote to Bradbury requesting information needed in order to make vapor pressure measurements on the plutonium we received from him on February 1. Stewart requests the gt level (364?); date the material was purified from element 95; and an analysis, if available, of the material.

In another memo Stewart sent Whitaker a description of our sample GTS #150 - 1/2 mg of americium. Stewart explains that the sample is in poor shape because of trouble we had in processing the material. In still another memo Stewart describes for Ralph Lapp the canning needs for our chemistry section for the next year.

I received today and routed to members of our section an index of reports (dated April 15) on "The Chemistry of the Fission Products," by Lester Winsberg and W. H Sullivan.

Helen and I had dinner at the Fraser Youngs' home. Other dinner guests included George and Rose Jura and the Whelans. George told us he has accepted a position in the Department of Chemistry at Berkeley and they will soon move.

Sunday, April 21, 1946

Hagemann, Ghiorso, Manning, and I played 18 holes of golf at Evergreen. A "low ball plus low total" match--Hagemann and Ghiorso versus Manning and me ended evenly (FH-108, AG-116, GS-101, WM-114).

During the evening I had a migraine headache.

Monday, April 22, 1946

Ruth Rogers returned to work today. Her original one-week leave of absence was extended to two weeks. Kay Florin filled in for Ruth during this period.

The organization of Section C-I as of today is as follows:

Glenn T. Seaborg - Section Chief
Ruth P. Rogers - Secretary to Seaborg
Rachel Whelan - Clerk

Winston M. Manning - Associate Section Chief
Donald C. Stewart - Assistant Section Chief
Darrell W. Osborne - Assistant Section Chief
Ruth L. Dirks - Secretary
Kathleen Florin - Secretary
Mildred Bolden - Secretary, on loan from Information Services

Group 1 Heavy, Isotopes
Seaborg, Glenn T. - Group Leader
Anderson, Herbert H. - Research Associate
Bentley, William - Research Associate
Cunningham, Burris B. - Research Associate
Fineman, Phillip - Research Associate
Florin, Alan E. - Research Associate
Ghiorso, Albert - Research Associate
Hagemann, French - Research Associate
Hindman, J. Clark - Research Associate
Hyde, Earl - Research Associate
Katzin, Leonard I. - Research Associate
Kohman, Truman P. - Research Associate
Jaffey, Arthur H. - Research Associate
Simpson, Oliver C. [1/4 time] - Research Associate
Studier, Martin - Research Associate
Thompson, Roy C. - Research Associate
Wagner, Frank - Research Associate
Weissbourd, Bernard - Research Associate
Ames, Donald P. [SED] - Research Assistant
Erway, Norman - Research Assistant
Hopkins, Horace - Research Assistant
Scott, Benjamin - Research Assistant
Sedlet, Jacob - Research Assistant
Walsh, Patricia - Research Assistant
Calhoun, Opaline - Technician
Thomson, Helen - Technician
Nelson, Robert - Clerk

Group 2, Recovery
Stewart, Donald C. - Group Leader
Britain, J. W. - Research Associate
Elson, Robert - Research Associate
Warshaw, Silvia - Research Assistant

Group 3, Solvent Extraction
Schaffner, Irwin J. - Group Leader
Hyman, Herbert H. - Assistant Group Leader

Leader, Gordon - Research Associate
Schraidt, John H. - Research Associate
Ader, Milton - Research Assistant
Billheimer, John [SED] - Research Assistant
Cavataio, Vincent [SED] - Research Assistant
Davis, Horace [SED] - Research Assistant
Evans, Harold - Research Assistant
Hausman, Eugene A. - Research Assistant
Kelley, Alec - Research Assistant
Nachtman, Elliot - Research Assistant
Natale, John - Research Assistant
Post, Roy - Research Assistant
Sheft, Irving - Research Assistant
Wagner, Roberta - Research Assistant
Casler, John - Technician
Giacchetti, Olga - Technician
Guadagna, Lillian - Technician
Koziolek, Winifred - Technician
Tolmach, Emma - Technician

Group 4, Light Isotopes
Anderson, H. L. - Consultant-Group Leader
Novick, Aaron - Research Associate
Rall, Waldo - Research Assistant
Steinberger, Jack - Research Assistant
Hayashi, Shuki - Technician

I received from General Groves a copy of the notes on denaturing that our group (consisting of Alvarez, Bacher, Benedict, Bethe, Compton, Daniels, Oppenheimer, Ruhoff, Seaborg, Spedding, Thomas, and Zinn) prepared for him early this month. He also included several copies of the press release (released April 9). Groves says, "I want to thank you for your cooperation and assistance in taking time to come here on sudden notice to discuss this vital matter."

I called Coryell at Clinton Laboratories to discuss with him the work that has been done there on the discovery and isolation of isotopes of artificially produced elements with atomic numbers 61 and 43. He told me that the work on the 4.5-year 61^{147} should be credited to C. D. Coryell, L. E. Glendenin, and J. A. Marinsky at Clinton Laboratories. The experiments on the identification of the 10^6-year 43^{99} was done by Dwight Lincoln and W. H Sullivan at Hanford and by R. P. Schuman and E. P. Steinberg of Rubinson's section at the Met Lab.

In other correspondence today I received a note from Ted La Chapelle, saying that he will arrive in Berkeley on the evening of May 15 and will go to the Radiation Laboratory the next morning. He comments that the boys in Dayton are pleased with the names for 95 and 96. E. S. Proskauer of Interscience Publishers again wrote asking me to be an editor for the series, "Advances in Nuclear Chemistry." Proskauer explains that the editor's share of the royalties is 2½ percent. A letter addressed to me at the Department of Physics, UCLA, from Robert A. Mitchell of Salsbury Cove, Maine, arrived. Mitchell wants information about elements 95 and 96, comments on his equations for the production of these elements, and offers suggestions of the names "victorium" and "pacifium" for these elements.

Tuesday, April 23, 1946

Iz Perlman has located a house for us in Albany on the side of Albany Hill (836 Washington Street). Helen and I have decided it would be better for us to live in a house than in the apartment we had rented. Today I wrote to Miss Rebekah Young (secretary in the Department of Physics), owner of the apartment, explaining this and telling her that the Roy C. Thompsons, good friends and desirable tenants, would like to rent her apartment.

I dictated a number of other letters which I asked Ruth to type tomorrow. To S. C. Lind, Editor of the Journal of Physical Chemistry, I explain that three of the four papers from the ACS symposium in Atlantic City will be published in "Nucleonics and Atomic Energy Forum," and I do not believe it worthwhile to publish them additionally in the Journal of Physical Chemistry. I sent Mr. Alexander Gabriel a copy of my Atlantic City paper although, I say, it does not answer the questions he asked in his letter of April 15. I thanked Mr. Fred Rantz of Seattle for his version of the periodic table which he sent me in March, and I informed Mr. George Davis of Buffalo, who suggested some names for elements 95 and 96 last month, of the names we have chosen for these new elements. I also sent Richard Hamer of Ottawa the requested copy of the article describing the discovery of the new elements. Fred Heath, Department of Chemistry, University of Florida, in a letter dated April 18, described some work he and Mr. James D. Ross (deceased) did in attempting to find elements 43 and 75 in platinum ores. Today I replied and said that work during the last five to ten years indicates there are no stable isotopes of elements 43 and 61; the work of Noddack and Tacke on element 43 must be assumed to be in error although they were able to isolate element 75 (rhenium). I also answered a letter from Lewis G. Bassett, Rensselaer Polytechnic Institute, saying that I agree with Allison's suggestion that Warren Garrison would be a good man to lead a nuclear chemistry program at Rensselaer. I also suggest the name of Raymond Stoughton as another possibility and say that in about two years more men will be available when they have completed their graduate work. I also replied to the letter I received from Robert Mitchell yesterday. With regard to the equations he wrote for the production of the new elements I say, I am not allowed to confirm them, "but I believe that any nuclear physicist would regard them as very reasonable. Therefore you probably would not be making any mistake to include them, as your own idea, in the book which you are writing." A more personal letter went to Fred Albaugh in which I told him of my recent activities, and sent him a copy of the press release on denaturing plutonium. I also say that

> it is humiliating to have to admit that I am finding it increasingly difficult to beat Stanley (in golf)--in fact, it is becoming necessary to break 95, or even break 90, in order to do this and I don't find myself able to play at such a pace very consistently.

At 5:45 p.m. Foster York and I left Chicago for St. Louis on the Chicago and Southern RR. We are staying at the Gatewith Hotel.

Helen had a 1:00 p.m. appointment with her obstetrician, Dr. Davis.

Chief Justice of the Supreme Court, Harlan Fiske Stone, died last evening at age 73, according to this morning's paper.

Wednesday, April 24, 1946

Foster York and I met with Kennedy and Wahl at Washington University to pursue our attempts to reach agreement with the Federal Government on the ownership of our plutonium patents.

I played nine holes of golf in the late afternoon on the St. Louis Forest Park Municipal Golf Course. Joe Kennedy and Art Wahl accompanied me but did not play. I shot 51.

In Chicago "The Atomic Scientists of Chicago" are scheduled to meet at 8:00 p.m. to discuss the present ammended version of the McMahon Bill, the result of long arduous work.

I took an American Airlines plane and arrived in Chicago at midnight.

In an article in this morning's newspaper Admiral W. S. Parsons is quoted as saying that "atomic bomb" is a misnomer because it is really a "nuclear fission bomb" and has very little to do with atoms.

Thursday, April 25, 1946

Yesterday the Chemistry Division "Summary Report for March, 1946" was issued. Section C-I's work described in this report was covered by Manning in his report to Hogness on March 25 (MUC-GTS-2243). Section

C-II's work described here includes that done on the "High Temperature Pile," special problems such as "Tests on Irradiated Graphite," preliminary measurement of the "Specific Heat Of Beryllia," work on the "Thermal Conductivity of Beryllia-Uranium Oxide Mixtures," work on the continuation of the measurements of the "Elastic Modulus of Beryllia," and studies of the "Fission Product Diffusion and Volatilization from Mixed Oxide Samples." In the section on analytical work it is reported that a spectrogram of a 10 microgram sample of element 95 shows only a few more lines than had been observed using much smaller samples--indicating that the spectrum of this element is relatively simple.

Manning sent Wayne Johnson a performance review report for Ruth Rogers yesterday. Manning said that he would like her to remain as his secretary when I leave and, in addition, she has been asked to familiarize herself with many of the aspects of Don Stewart's work so that she can handle this when he leaves in June. Manning requests a reclassification to Secretary A and a maximum increase in salary.

In another memo to Johnson, Manning notified him that Waldo Rall will terminate May 1 and requested that John Steinberger be placed on a 30-hour week.

Gerhard Derge of Carnegie Institute of Technology is visiting the laboratory today, and I spent quite a bit of time with him.

The log of our thorium target and our Pa^{231} target arrived from Paul O'Connor at Berkeley.

I also received and read a copy of George Everson's reply to Roy Thompson's letter of April 18. Everson reiterates that the Radiation Laboratory does not pay "per diem" for wives en route or while looking for permanent housing.

I noted a letter from Ruth Rogers to Mary Millard, our secretary at the Radiation Laboratory, describing our filing system. Ruth explains that Mary will need two four-drawer filing cabinets for the correspondence and three files for the reports. She says that packages of this material will begin arriving at any moment.

Friday, April 26, 1946

One of the first things I did today was to write to Fritz Paneth about the question of naming elements 43, 61, 85, and 87. I say,

> For element 43, it seems that Perrier and Segrè should be suggested to do the naming, and the appropriate

reference is J. Chem. Phys. 5, 712 (1937). These were the first investigators to produce artificially and identify positively radioactive isotopes of this element. In addition, there is the following information which is relevant. At the recent Atlantic City meeting of the American Chemical Society (on April 10, 1946, at our symposium on nuclear chemistry), C. D. Coryell described the work of D. C. Lincoln and W. H Sullivan, and another team, R. P. Schuman and E. P. Steinberg, who have independently observed the radioactivity (approximately 10^6-year half-life) due to the lower isomeric state of 43^{99}, formed in the fission of uranium. This is interesting because it will be possible now to isolate this isotope in weighable amounts. This particular isotope of element 43 was discovered by Seaborg and Segrè (Phys. Rev. 55, 808 (1939)), who produced it from the deuteron and neutron bombardment of molybdenum and found that the upper isomeric state has a half-life of 6.6 hours and the lower isomeric state has a half-life of greater than 40 years.

The situation with respect to element 61 now seems clear as the result of information released at this same symposium at Atlantic City. Early work by two teams, (sample references are Law, Pool, Kurbatov and Quill, Phys. Rev. 59, 936 (1941) and Wu and Segrè, Phys. Rev. 61, 203 (1942)), on the bombardment of praseodymium and neodymium, led to the production of a number of radioactivities, some due to element 61 and some not due to element 61, with no possible clear conception by these investigators as to which were and which weren't isotopes of this element. At the Atlantic City meeting C. D. Coryell reported the positive identification by C. D. Coryell, L. E. Glendenin, and J. A. Marinsky of the isotope 61^{147} with 4.5-years half-life, formed in the fission of uranium. I know from first-hand contact with these people that they have been able to prove unequivocally that this radioactivity is due to an isotope of element 61. Therefore it seems clear that the naming of this element should be done by C. D. Coryell, L. E. Glendenin, and J. A. Marinsky, with reference to the nuclear chemistry symposium at the Atlantic City meeting of the American Chemical Society.

For element 85 it is clear that the naming should be done by Corson, MacKenzie, and Segrè, with reference to their article, Phys. Rev. 58, 672 (1940). I agree with you that there probably is no need to go into Miss Karlik's work from the point of view of naming this element. Similarly, I do not believe the work of Minder (for example, Helvetica, Physica Acta 13, 144 (1940)) and Leigh-Smith, who have also been investigating radioactivities in the natural series which they believe are due to element 85, need be considered from this point of view; in addition, their work seems to be wrong.

> For element 87, it seem clear that it is Perey, and the corresponding reference is probably either Comptes rendus 208, 97 (1939) or J. de Phys. et rad. 10, 435 (1939). There is, of course, the work of Güeben (Ann. Soc. Sci. Bruxelles B52, 60 (1932); B53, 115 (1933)) who claims to have discovered alpha branching in $MsTh_2$, which would lead to an isotope of element 87. However, since no radiation from the isotope of element 87 was observed, it seems unnecessary to give consideration to this work from the point of view of naming this element.

I go on to tell Paneth that I proposed "americium," symbol Am, and "curium," symbol Cm, as names for element 95 and 96 at the same symposium on nuclear chemistry at the meeting of the American Chemical Society.

I dictated several letters of recommendation today. I wrote to Dr. Bartky of the University of Chicago about John Malm who has applied for a fellowship at the Institute of Nuclear Studies, saying that Malm is of graduate student calibre. I then say to Professor G. L. Clark of the University of Illinois that I rate Ed Orlemann very highly but add that Orlemann has accepted a position on the staff of the University of California, where he is at present and where, I hope, he is well satisfied. In a letter to H. F. Jordan of the Research and Development Department of United States Rubber Company, I reiterate the remarks that I made to him at the ACS meeting in Atlantic City about Darrell Osborne. In reply to his concern about Osborne being a perfectionist, I say that I have seen no indication of severe attention to trivial detail but Osborne is critical of his own ideas and will certainly never be prone to have "wild" ideas.

Yesterday I received a copy of the script of the broadcast of John E. Pfeiffer of CBS about the Atlantic City meeting (the program was not heard in Chicago). Pfeiffer had this to say:

> The 100,000 chemists in Atlantic City had plenty to talk about. They discussed everything from a sardine-oil paint and why cold-storage eggs change color, to the latest news in atomic energy. An important story was revealed by Dr. Glenn Seaborg of the University of Chicago. He's worked on the Manhattan Project during the war. You remember that the State Department recently announced a way to prevent the misuse of atom-bomb explosives. The idea is to blend these potent substances with harmless chemicals. This would make the diluted mixture useful for peacetime research only. But Dr. Seaborg spiked this widely publicized "denaturing" theory. He revealed that scientists could, if they wanted to, extract the harmless denaturing chemicals from the mixture. That would leave pure atom-bomb explosives and defeat the entire purpose of the denaturing process. Denaturing by itself, in

other words, is not a sure-fire way to control atomic energy. And Dr. Seaborg, didn't hesitate to point out the need for UNO inspectors. Their job would be to make regular visits to atom scientists throughout the world--just to see that things were going according to Hoyle. Once we had inspectors, the denaturing process might help. It would delay any undercover stuff. It's no cinch to separate plutonium, for example, from the materials used to dilute it. The job would require a big chemical plant. That would stall any dirty work by one to three years. Besides, it's almost impossible to hide a big factory, and regular check-ups would be that much easier. There was an interesting slant on these discussions. The Army recently issued a new manual stating in black and white just what scientists could and couldn't talk about. Secrecy rules were relaxed--but only a bit. Dr. Seaborg told of two chemical elements that had been discovered as a result of the Manhattan Project (one of these elements by the way, was named curium at the suggestion of a CBS listener). Dr. Charles Coryell of Oak Ridge revealed that the splitting of uranium produced more than 150 types of radioactive atoms. The announcement gave people something to talk about. But behind every one of them, were a hundred unmentioned facts. Chemists at Atlantic City got a few crumbs of information--and I know that they came away hungry. They had many questions they wanted to ask but didn't. They knew the answers would come back in double talk. I had real respect for the way Manhattan Project scientists sidestepped the few potent questions. The real story wasn't told, and everyone knew it...

I wrote to Pfeiffer today, thanked him for the script, and answered a few questions he posed.

Don Stewart wrote to Iz Perlman to describe the division between Berkeley and Chicago of the various special materials and products now here in Chicago. Stewart gave Perlman a list and description of the items for which Perlman should made a formal request from his Area Engineer.

I also received a note from Nell A. Parkinson of C & E News yesterday asking if I would review Henry D. Smyth's book, Atomic Energy for Military Purposes. Today I replied affirmatively.

Administrative items received today included two memos from Branch announcing that daylight savings time becomes effective Sunday, April 28, and that Memorial Day, May 30, will be an authorized legal holiday. J. J. Nickson also repeated, by memo, the recommended procedure to be followed with wounds incurred while working with radioactive materials.

Saturday, April 27, 1946

I wrote a supportive letter for Norman Davidson to Professor R. K. Summerbell of Northwestern University. I say I rate Davidson as an exceptionally good chemist with an excellent grasp of both the theory and techniques of modern chemistry and full of original ideas.

Today I played 18 holes of golf at Cog Hill Course No. 2 with Hagemann, Ghiorso, and Manning. Hagemann and Ghiorso won a "low ball plus low total" match, 5 and 2. (FH-109, AG-102, GS-102, WM-116.)

After dinner Helen and I spent the evening with Leonard and Alice Katzin.

Sunday, April 28, 1946

Helen and I visited the Chicago Museum of Science and Industry.

There is a big article in the Chicago Sun this morning describing a supplement that will be carried in Friday's Sun about the "Atomic Age."

Monday, April 29, 1946

Burris Cunningham terminated today. He and his family will leave Chicago by train at 8:45 p.m. tonight for Berkeley.

I have been spending some time during the last few days preparing a paper, "The Transuranium Elements," to be used as the basis for a talk to a meeting of the American Physical Society in Chicago on June 21. Today I received a typed draft; I shall ask some members of our group to review it.

A letter arrived from Erle M. Billings of the ACS thanking me for my participation in the discussion relative to graduate training in chemistry at the ACS meeting in Atlantic City on April 5. I also received a questionnaire from Albert Noyes to be filled out about the present status of graduate work in our department at Berkeley. In addition Noyes asks for a letter describing what we (members of the ACS committee) feel should be emphasized in graduate training.

Helen went shopping and to the library today.

Tuesday, April 30, 1946

This morning I attended the meeting of the Heavy Isotopes Group along with Fineman, Florin, Ghiorso, Hindman, Hopkins, Hyde, Jaffey, Katzin, Kohman, Manning, Osborne, Scott, Stewart, Studier, R. Thompson, and Warshaw. We reviewed the writing situation, and Florin said he is completing his papers on the lower and higher plutonium fluorides. Hopkins is writing a memo for Stan Thompson on the use of fluosilicate in decontaminating americium and should be finished in a few days. He plans to make a literature survey on radiations accompanying fission products. Stewart said the chapter on purification has been revised and is ready for editing. Ghiorso will complete all of the writing on fissionability measurements by May 26. Kohman said he has not revised the chapter on the radiochemistry of neptunium and plutonium since only one additional completed paper was available. This chapter will be revised by putting back the material formerly removed at the request of the patent department. He is making an outline for the chapter on elements 95 and 96. Manning has not completed the paper on 95^{241} being done with Asprey, and Jaffey has nearly finished the chapter on measurements except for the description of the pulse analyzer and fission counting equipment. R. Thompson said the rough draft is ready for the chapter on working targets. I then said a new revised version of the "Table of Isotopes" and of the isotope chart will be issued by July 1. In order to give proper credit in this table for work done in the section, the various teams which have worked on targets should have their write-ups at least in draft form immediately. All of the chapters in volume 14A are apparently coming along quite nicely with the exception of the one by Seaborg and Manning on the fissionability of heavy nuclei. The printing of volume 14B will not be held up if all the papers are not in but will go to press whenever enough are available to complete the volume, if cleared by security.

I then commented on the bombardments and stated that I feel that future targets should be worked by the same teams as those in the past, that is, protactinium bombardments will be worked by R. Thompson and Osborne, and so on. The next targets will be deuterons on Q-metal and 37. These will be flown in on May 11 if arrangements can be made. The 37 target can probably be handled by Hindman alone as no effort will be made to check the short-lived isotopes. The Q-metal will be worked by Hopkins and someone else to be selected later. This target is being worked primarily to check the fissionability of Np^{238}. An attempt to check the fissionability of Np^{236} can be made although it is probably too short-lived to catch. The Pu^{238} plus Pu^{239} mixture in the plutonium fraction can be checked, but it might be doubtful as to whether the results can be interpreted to mean anything. Studier commented that more men should be placed on the teams working up these targets with a man to take over when the target comes in and to oversee the time-consuming milling and dissolving operations. The necessity for doing this leaves the man in poor shape when it is time to make the critical measurements.

I asked about the results of the processing on the protactinium target plus deuterons and the thorium target plus deuterons. Ghiorso first reported on the protactinium target. The uranium fraction when worked up had about 18,000 fission c/m. It has about the same cadmium ratio as U^{233}, so apparently it is due to slow neutrons. The fissionability of this fraction is decaying with a 70-day half-life so it cannot all be due to U^{230} although a portion must be. Osborne and Thompson said the yield from this target was some 8-10 times that of the last similar bombardment, whereas the amount of irradiation and quantity led them to expect only about twice as much. They are again getting a 4½-day Geiger activity through 2 grams of beryllium, assigned tentatively to U^{231}. With no absorber this started as 1½-day half-life which was gone in 12-24 hours and then tailed over to the 4½-day activity. Both the lead and aluminum absorption curves check on this 4½-day radiation and indicate x-rays. There is a very small amount of gamma-ray activity associated (through 3½ grams of lead). Osborne suggested the 4.5-day x-ray activity might be a nuclear isomer of U^{232}, and it was decided that a nuclear isomerism might be a feasible explanation. It will be checked later.

Hyde and Studier then reported on the thorium target. Two fractions were milled from the surface of this target. The top was three mils in thickness, and it was hoped to get all of the Pa^{230} from the d,4n reaction in this depth of metal with a minimum of other isotopes. The second sample was 22 mils in depth. Protactinium and uranium fractions were then separated in each sample. The uranium was cleaned out of the protactinium fractions by carrying protactinium on MnO_2 and extracting the protactinium by TTA from 3-4 N HNO_3 at periodical intervals. The fissionability was rechecked on both the purified protactinium and the uranium milked from it. Ghiorso said that 5 percent of the protactinium fraction from the bottom layer gave 15,000 fission c/m. The original decay of the fissionability of this material was larger than expected before U^{233} started growing in. The protactinium fractions from both layers showed fission. The fission in the fraction from the top layer decayed more slowly than that from the lower layer. If some Pa^{230} is present in the lower layer, or if Pa^{232} has an appreciable fission cross section (the latter is more probable), these data can be accounted for. The samples are still being followed although some minor difficulties during the measurements (such as beta decay arising from the reaction of neutrons on the gold used in making the plates) make it difficult to interpret the results. In repeating this experiment, one sample should be checked for beta decay at the beginning and left to stand to serve as a control (i.e., should not be put in the pile). Protactinium-230 and Pa^{232} would seem to undergo slow neutron fission with roughly the same cross section as far as can be told from these data at the present time. I asked if the n,γ cross section of Pa^{233} was rechecked and Ghiorso said that, as usual, the last measurements had to be rushed because the pile was to be used by other people. The results are not too satisfactory. It is thought that perhaps slight changes in the position of the counting chamber in the thermal column is a factor. Some 60 measurements were made in all, and the results cut the former value by a factor of 6 to something like 50 barns, but they are no more reliable than the former results which indicated 300 barns.

Kohman asked if the corresponding experiment had ever been done on Np^{239}. I said no, although it was once suggested to Allison as an explanation for the following: The Pu^{240}/Pu^{239} ratio in plutonium of various gt levels could not be directly correlated with the amount of neutron radiation received on the assumption of a simple n,γ reaction of Pu^{239}. A cross section of 1,000 barns for the reaction $Np^{239}(n,\gamma)$, however, would account for the effect observed at that time. The direct measurement of the $Np^{239}(n,\gamma)$ cross section would be a tough one to do. An easier one on another isotope, suggested by Manning, would consist of bombarding thorium at Hanford and to look for the UX_1 (Th^{234}) to get the neutron cross section of Th^{233}.

Manning announced that the Q-metal slug will be coming in from Hanford and should be worked fairly quickly to get the U^{240} present. The Np^{240} and Pu^{240} can also be obtained by milking. Someone with experience in decontamination is needed to work it up. It was decided that Hopkins should do it at the sacrifice of the literature search on fission product radiations.

Manning then brought up the problem of checking the dissolver solution samples from Hanford. It was decided that Fineman should try to get a few microliters from this solution to give to Britain for electroplating. Ghiorso can then check this on the pulse analyzer to see if he can get any data to explain high losses in the extraction step at Hanford.

Fineman reported that his last experiment on the growth of Em^{221} from Ra^{225} indicated that none is formed. The experiment will be repeated once more as a check.

Ghiorso talked about several check measurements made at Argonne. The americium plus curium sample has been repurified and the Cm^{242} would seem to have a fission cross section of about 1,000 barns, while the Am^{241} cross section is 2.7 barns this time instead of 2.9. Rechecks on radiothorium and actinium plates show them both still to have fission cross sections of about 1 barn. Anderson reported that he has now prepared the ammonium-rubidium and cesium mixed sulfate compounds with Pu(III). These have the characteristic formula of: $(NH_4,Rb,Cs)Pu(SO_4)_2 \cdot 4H_2O$. All of them are light lavender blue in color. At the present time work is being continued on the more complicated sodium and potassium compounds.

Another of the Project papers was completed today, "The First Use of Bismuth Phosphate for Separating Plutonium from Uranium and Fission Products," by Stanley G. Thompson and Glenn T. Seaborg.

I prepared and sent to Colonel A. V. Peterson the proposed chemistry program for our group at Berkeley which he requested. I include 14 topics: Nuclear Properties of Transuranium Elements, Chemistry of Americium, Chemistry of Curium, Search for Transuranium Elements, Chemistry of Plutonium, Chemistry of Neptunium, Chemistry of Protactinium, Spectroscopic Investigations, Nuclear Investigation in Region of Atomic Number 80-85, Identification of Reactions and Products Obtained as a Result of Use of New Acceleration Machines, Search for Transuranium Elements in Nature, Basic Chemistry of the Rare Earth Elements, Isotope Separation by Solvent Extraction, Special Separations.

Today I received a letter from Miss Inez L. Magill of Pittsburgh, a science teacher and former clinical laboratory technician, who recently has talked with someone who heard my lecture in Pittsburgh. She describes the case of a friend who has myelogenous leukemia and asks if I or any colleague would be interested in treating her friend with radioactive phosphorus.

The Physical Review has now published [69, 366-7 (1946)] our two letters that were submitted in 1941 but voluntarily withheld from publication: "Radioactive Element 94 from Deuterons on Uranium" by G. T. Seaborg, E. M. McMillan, J. W. Kennedy, and A. C. Wahl and "Radioactive Element 94 from Deuterons on Uranium" by G. T. Seaborg, A. C. Wahl, and J. W. Kennedy.

"Allies Order Suppression of German Science" is the heading of an article in this morning's paper. This decision of the Allied Control Council includes a prohibition on any research that might develop an atomic bomb.

Wednesday, May 1, 1946

Waldo Rall of H. L. Anderson's Light Isotopes Group terminated at the Met Lab in order to return full time to school at the University of Chicago.

A thank-you note arrived from John E. Pfeiffer, Science Director of CBS, for answering some questions he posed recently. He asked that I make a point of having lunch with him on my next visit to New York.

Helen went to a baseball game with Anne York. Later we were taken out to dinner by Anne and Foster.

King Gustav of Sweden has a great grandson. This is the first direct male heir to the throne of Sweden to be born in 40 years.

Thursday, May 2, 1946

The Solvent Extraction Group met today. The attendees were Ader, Davis, Evans, Hausman, Hyman, King, Lincoln, Leader, Manning, Nachtman, Osborne, Post, Schaffner, Schraidt, Sheft, and Wagner. After describing column runs made since the last meeting, Hyman summarized the process as it stands today: Plutonium and uranium recovery have been successfully achieved beyond any question. A final plant would be able to get plutonium recovery in excess of 99.5 percent with present conditions; perhaps 99.9 percent with minor changes which are still uncertain. Decontamination is less certain. While 10^7 has been achieved, it has not been realized as consistently as might be expected. The difficulty appears to be in the use of more strenuous oxidizing conditions than can be tolerated in the preparation of both IA and IIA feeds. As a result, ruthenium and cesium are found in high activities in the final product stream. Work is now in progress aimed toward more selective oxidation procedures. He said that one additional fission product that may be leaking through in the recent runs is iodine, which we failed to find in earlier fission product analysis on older material. Not enough information is known about what happens to iodine in the dissolver. It may go through as the iodide. We know free I_2 would be hexone soluble.

Davis described his work in attempting to learn what impurities are present in commercial hexone and what is the effect of these impurities on the elimination of zirconium and ultimately other fission product elements from the product. In order to obtain this information we desire to (1) study the effects of the most probable impurities deduced from the literature on the manufactured hexone, (2) determine directly by experiment the nature of the impurities in commercial grades available and those remaining in the pretreated hexone used in our columns, and (3) prepare a purer grade of hexone to find what separation of zirconium results from its use. Some probable impurities from the synthesis Shell employs to produce hexone will be mesityl oxide, methyl isobutyl carbinol, and butanol. The experiments which have been conducted up to this time involve the determination of the distribution ratios for zirconium between the aqueous phase containing various salts and different hexones. These results are summarized below:

Distribution Ratios Zirconium Aqueous/Hexone

Aqueous Solution	Pure Hexone	Pretreated Hexone	Shell Commercial	Carbide Commercial
8 M NH_4NO_3 0.5 M HNO_3	750	690	940	610
8 M NH_4NO_3 0.5 M HNO_3 0.1 M $Na_2Cr_2O_7$	690	700	700	290
10 M NH_4NO_3 0.5 M HNO_3 0.2 M $Na_2Cr_2O_7$	300	230	280	70
10 M NH_4NO_3 0.1 M $Cr(NO_3)_3$ 0.5 M HNO_3 0.1 M $Na_2Cr_2O_7$	550	650		80
0.2 M $Cr(NO_3)_3$ 10 M NH_4NO_3 0.5 M HNO_3				180

The "pure" hexone was a pretreated and carefully fractionated commercial hexone. It is seen from these results that the Carbon and Carbide hexone has an effect markedly different from the other hexones when equilibrated with salt solutions III and IV. Since (after equilibration with Carbide hexone) the green color of the chromic ion was observed in the aqueous phase originally containing no chromic ion (salt solution III), it is thought the low distribution ratio might be due to some oxidation product or products of the impurities in the hexone.

Further study will include the determination of distribution ratios of zirconium using oxidation reduction systems such as Ce(IV)-Ce(III) and using hexone containing mesityl oxide, methyl isobutyl carbinol, butanol, and oxidation products of these substances.

Ader reported on a set-up for a solvent extraction process to recover U^{235}. In connection with a power pile program being studied at Clinton, each day 160 g of uranium along with 6 kg of aluminum are expected to be processed.

The nomenclature employed in the proposed design follows that currently being used in the Redox Process.

Column IA Feed (200 l)	Scrub (50 l)	Extraction (100 l)
1.13 M $Al(NO_3)_3$ 0.0034 M UNH 0.5 M HNO_3	0.5-0.7 M $Al(NO_3)_3$ 0.1 M HNO_3	Hexone -0.5 M in HNO_3

Extractor portion - ca. 7 plates
Scrubber portion - ca. 3 plates
Estimated diameter of column - 2½-3 inches
Estimated height of column - 40 feet (±10 feet)

A decontamination factor of 10^3 is expected in this column. If the process is designed to recover Np^{237} also, 0.1 M dichromate will be included in the feed and scrub solutions. The neptunium will go along with the U^{235} and be separated from it in column IIA.

Column IB Feed (100 l)	Extractant (10 l)	Product (10 l)
Hexone from Col. IA	0.1 HNO_3	0.07 M UNH 1.0 M HNO_3 ca 0.01 M $Cr_2O_7^{=}$

Estimated diameter of column - 1½-2 inches
Estimated height of column - 10 feet (4 plates)

The decontamination factor expected through column IB is 10^4.

Column IIA Feed (15 l)	Extractant (25 l)	Scrub (10 l)
10 l of IBP made up to 1.25 M $Al(NO_3)_3$ 0.5 M HNO_3	Hexone - 0.5 M HNO_3	0.5 M $Al(NO_3)_3$ 0.1 M HNO_3

Extractor portion - ca. 4 plates
Scrubber portion - ca. 4 plates
Estimated diameter of column - 1 inch
Estimated height of column - 12 feet

The decontamination factor expected through column IIA is 10^6. If necessary, fluoride and a reducing agent may be included in both feed and scrub solution. A reducing agent will probably be included to separate uranium from neptunium and plutonium if both of the latter are to be recovered. The neptunium and plutonium are carried out in the IIAW stream whereas uranium goes with the IIAP to Column IIB.

Column IIB Feed (25 l)	Extractant (2 l)	Product (2 l)
Hexone from Col. IIA ca. 0.1 M HNO_3	0.1 M HNO_3	0.35 M UNH 1.0 M HNO_3

Estimated diameter of column - 1 inch
Estimated height of column - 10 feet (ca 5 plates)

The decontamination of the final product is expected to be about 10^7. A concentration factor of 100 is achieved but may be varied by varying the volume of aqueous extractant in Column IIB.

Actual laboratory batch experiments have given beta particle decontamination factors of 200-500 in Column IA, and 8 x 10^3 through Column IB (with and without dichromate having been used in Column IA).

Fission product analysis of the IBW (hexone) solution from runs which did and did not have dichromate in the IAF solution showed the following FPE to be present:

Without $Cr_2O_7^=$	With $Cr_2O_7^=$ (0.1 M at 50°C)
92% I	89% I
8% Ru	1.1% Ru
1.5% RE	1.4% RE
	0.2% Zr

The feeds for these batch runs were made from rather young slugs--slugs that had had but 35 days cooling before dissolution and processing. In the contemplated process the uranium rods will have at least a 60-day cooling period to permit decay of U^{237}. Thus eight-day iodine will not be as important a FP in process runs as it is in our batch experiments. An additional factor of 8 or 9 in Column IA will be achieved in the absence of this iodine. Since no FP analyses have been made on IBP solutions, we do not know if iodine comprises an appreciable amount of the FPE in this solution. Batch work in the second cycle has not been possible so far due to the low activity of our feed solution. A feed solution greatly concentrated in FP activity is being prepared by E. Hausman by ether extracting the uranium out of our regular concentrated hot UNH solutions. Distribution ratios of neptunium between $Al(NO_3)_3$ solutions and hexone are now being obtained. Preliminary results indicate the necessity of oxidizing neptunium to the VI state with dichromate in the IAF solution, if quantitative recovery is required.

D. C. Lincoln is repeating some of our batch decontamination runs using dibutyl cellosolve as solvent instead of hexone. Dibutyl cellosolve gave excellent results in the separation of uranium from thorium at Clinton. However, in our process this solvent possesses several disadvantages. It is a poor extractant for uranium and this requires rather high salting out concentration of $Al(NO_3)_3$, thereby bringing our aqueous solutions close to the freezing point and requiring smaller through-puts for the same size column and packing as compared with hexone. Furthermore, the higher boiling point of dibutyl cellosolve makes solvent recovery more difficult than with hexone.

S. C. Lind, Editor of The Journal of Physical Chemistry, replied to my letter of April 24 in which I told him that the majority of the speakers at the symposium in Atlantic City decided to publish in the news edition. Lind feels that this is a poor decision as he considers the news edition badly in need of revision and in no way worthy of ACS. He says that my paper is the first scientific contribution he has ever seen in the news edition worthy of citation.

R. K. Summerbell of Northwestern University wrote to thank me for my recommendation of Norman Davidson and asking if I can recommend a younger man in the field of inorganic chemistry to be brought in on the instructor's level.

Helen went to the library today.

"Atomic Energy May Be Key to Eternal Youth" says an article in this morning's newspaper. This was from a suggestion in C & E News. What C & E News actually said was that the adverse effects of radiation will cause acceleration of studies on longevity and problems of aging.

Friday, May 3, 1946

Frances V. Benner, a special assistant in the ACS whose duties include assisting local sections to arrange monthly programs, wrote to invite me to join one of the touring groups in New England during the 1946-47 year.

I received a note from Martin Matheson of John Wiley. He is in Chicago at the Palmer House and would like an appointment with me to discuss plans for publications in the fields of nuclear physics and radiochemistry. I immediately replied, suggesting next Wednesday afternoon or Thursday in my office and ask that he phone me before coming.

The report, "An Emanation Method for Radium Analysis," by Fineman, Weissbourd, Anderson, Sedlet, Ames, and Kohman was completed today.

Helen went to the library again today.

Today the Chicago Sun carried its big supplement, "The Atomic Future," by Howard W. Blakeslee. It is 25 pages long and shows where uranium and thorium are mined, gives a glossary of the languages, and discusses the future peaceful uses of atomic energy. Blakeslee says, "Harnessing the atom ranks with the greatest discoveries of man." He writes of the risks to their careers taken by scientists who worked on the atomic bomb and of the risks President Roosevelt took in this matter.

Saturday, May 4, 1946

Another letter arrived from Geoffrey Wilkinson today. He is proceeding with his plans to join our group in Berkeley at the beginning of September. Wilkinson says that he needs evidence that he has been accepted for study in order to satisfy the U.S. Immigration Department.

I played 18 holes of golf at Jackson Park with Hagemann, Ghiorso, and Manning. French and Al won a "low ball plus low total" match, 10 and 8. (FH-106, AG-97, GS-97, WM-107.)

Sunday, May 5, 1946

I spent several hours in my office at the lab today.

"Atom Power Plant Now in Blueprint" reads a headline in today's paper. This is an announcement by Dr. Farrington Daniels, who says the experimental plant will be constructed in Oak Ridge.

Monday, May 6, 1946

Kathleen Florin terminated today. George Bernstein, who served as an SED man with the Solvent Extraction Group before being transferred to Site Y, was hired today by the Met Lab as a Junior Chemist.

The Laboratory Council met today in New Chem, Room A-7.

An inquiry arrived for me from Harold H. Strain, Carnegie Institution of Washington at Stanford University. Strain is interested in finding an analytical chemist to participate in the cooperative preparation of a revised book on chromatographic adsorption analysis.

Report MUC-FDL-202, "Report for April, 1946," was issued today. The report of the work in Section C-I is that submitted by Manning to Daniels on April 20 (MUC-GTS-2290). The report on Section C-II includes the following information on High Temperature Piles. Although it has been found that beryllia is volatilized by steam at 1,500°C, volatility appears to be negligible at somewhat lower temperatures. The presence of 0.05 percent of steam in helium gas caused a loss of 0.7 percent of the uranium content when the gases were passed over a mixed fuel rod of BeO and 10 percent uranium oxide at 1,450°C for 50 hours.

The hot-pressed beryllium oxide bricks exhibit cracking when heated and cooled at rates greater than 220°/min., but it has been found that this cracking is eliminated by annealing the bricks at high temperatures (above 1,400°C). The Norton Company is investigating the possibility of changing their operations in such a way as to produce the annealed bricks which will not be subject to this cracking.

The crushing strength of the hot-pressed beryllia with a density of 2.9 g/cc^3 has been found to be 200,000 lb./sq. in. and the tensile strength of a 90 percent BeO-10 percent UO_2 tube having a density of about 3.1 g/cc^3 has been found to be 20,000 lb./sq. in.

Linear measurements and weights of 600 pieces of BeO and BeO-UO_2 and density determinations on 100 such pieces have been made, preparatory to irradiation tests in the Hanford pile. Following irradiation, thermal conductivity, elastic modulus, crushing-strength, and tensile-strength changes will be determined.

The mock-up unit for obtaining information on the operation of the high-temperature pile has been provided with a pre-heater for the helium coolant in order to make it possible to reproduce more closely the temperature gradients existing at various altitudes in the actual pile. It is also being equipped with a purifier for removing oxygen from the helium. The internal structure of the unit has been reconstructed to allow the installation of many more thermocouples so that more complete data on temperature distribution through the operating unit can be obtained.

Determinations of the heat-conductivity of 90 percent BeO-10 percent UO_2 rods at the Battelle Memorial Institute over the temperature range from 150° to 350° show values very close to those obtained on pure BeO samples of similar density, reported earlier.

Design work on loading and unloading mechanisms for the high-temperature pile is continuing. At the same time, tests are being carried out on creep strength of metals at elevated temperatures, the determination of pressure drops in fuel-rod channels as a function of fuel-rod design, and on proposed means for removal of oxygen from pile coolant gases.

The use of the polarograph in the analysis of beryllium-containing compounds is being investigated. Well defined waves have been obtained for thallium, cadmium, and lead in the presence of beryllium.

About 300 grams of extremely pure beryllia oxide have been prepared by the sublimation of basic beryllium acetate. Twenty grams of this material was sent to Clinton for cross section measurements, and 20 grams to MIT for use in preparing standards. New standards are being prepared in this laboratory.

The Physics and Metallurgy Division reports new crystal-structure results for AmO_2 and $AmCl_3$ and a description of the preparation of AmO_2 and $AmCl_3$. The melting point of $NpCl_4$ is reported as 538°C.

The Mass Spectroscopy section made the following mass assignments of radioactive isotopes: europium 9-hour activity at mass 152, iridium 19-hour activity at 194, iridium 60-day activity at 192.

For April the percent of divisional effort in chemistry devoted to various problems is as follows:

Section	Program Area	% Effort
C-I (1)	Nuclear and chemical properties of heavy elements	16
C-I (2)	Control analysis of uranium ore	4
C-I (3)	Recovery of product	3
C-I (4)	Redox solvent-extraction process	16
C-II (1)	High-temperature piles	14
C-II (2)	Radiation chemistry	3
C-II (3)	Fission-product studies	3
C-II (4)	Analytical chemistry	2
	Writing for Plutonium Project Report	37
	Total	98

The distribution of Metallurgical Laboratory employees as of April 30, 1946 (including 21 SED men) is:

	Academic	Non-Academic	Total	SED
Argonne Laboratory	32	12	44	2
Chemistry Division	88	32	120	14
Health Division	68	113	181	1
Physics and Metallurgy Division	19	7	26	2
Opacity Section	5	1	6	1
Services and Development Division	26	195	221	1
				21
Scientific Administration	4	4	8	
Information Division	15	17	32	
Patents	2	12	14	
Associated Sites (Manhattan District Advisor, Evergreen)	2	2	2	
	261	395	656	
Security and Safety			145	
Administration			365	
			1166	

The Board of Governors of Argonne National Laboratory is meeting today.

Tuesday, May 7, 1946

I received a letter dated May 1 from Maurice E. Nahmias, Seine, France, who as a Rockefeller Fellow spent several months in Berkeley in 1937. Nahmias asks for reprints, a photograph of me, and a copy of the Bulletin of the Atomic Scientists of Chicago. He then asks me to convey his greetings to Professor Lawrence, Dr. Cooksey, Ed McMillan, Bill Libby, and all others whom he met when he was in Berkeley.

Wally Reynolds from Berkeley had lunch in our apartment with Helen and me.

In the late afternoon I took the train from Chicago to Milwaukee (seat 1, car 24) where I had dinner with the American

Association of Brewing Chemists. I gave the after-dinner address, "Atomic Energy." After the talk I immediately left by train to return to Chicago.

Helen had dinner with Frances Chilson, who spent the night with us.

<u>Wednesday, May 8, 1946</u>

I insisted that I must return to Chicago last night after my talk in Milwaukee because of the limited time remaining before our departure for Berkeley. It was a rather foolish move because this morning I awoke with a cold and spent the morning at home.

At the lab in the afternoon I found a notice of a meeting of the Atomic Scientists of Chicago tonight at 8:00 p.m. There will be a policy discussion on "Where do we go from here? What shall be the future policy of the Atomic Scientists of Chicago?"

Donald A. McPherson and Brad Wiley of John Wiley & Sons arranged to have an appointment with me today. They are very anxious to publish a book on radiochemistry which they would like me to write.

The Power Shortage Committee (Furney, Branch, Captain C. A. Tidd, and Henry Solem) issued the following memo:

> The coal and electrical power shortage is, as you know, becoming very acute. The Metallurgical Laboratory has been asked to participate in a program designed to affect a considerable savings of critical supplies. If the emergency continues, hospitals, food agencies, and sewage disposal facilities will receive first priority for electrical service. You are, therefore, requested to perform your duties in such a manner as to conserve as much electricity as is possible. Your wholehearted cooperation in this serious matter is of utmost importance.

H. B. Hass of the Department of Chemistry of Purdue University wrote to me asking for a recommendation for Joe Katz.

Frances Chilson had dinner with us at our home and left for Plymouth, Iowa (her family home) late in the evening.

This morning's paper quotes Professor Alexander Ananoff as saying that, in 25 years, atomic energy will transport passengers from Paris to the moon in 3 hours and 27 minutes.

Thursday, May 9, 1946

Donald Ames, one of our SED men, is being transferred to Site Y for a month.

Ruth Rogers sent Cyril Smith a list of data about neptunium, plutonium, americium, and curium that I put together this morning.

The following Plutonium Project Record reports have now been issued: "Microscale Testing of Refractories for the Production of Plutonium Metal" (CT-3896), R. Frank, M. Gerstein, N. N. Hellman, Z. V. Jasaitis, H. P. Robinson, and E. F. Westrum, Jr.; "Microscale Studies of Sulfide Refractories for Plutonium Metal Production" (CT-3897), E. F. Westrum, Jr. and N. N. Hellman; "The Preparation of Plutonium Metal on the Ultra-Microscale" (CT-3898), Sherman Fried; "Technical and Experimental Aspects of Metal Production on the Microgram and Milligram Scale" (CT-3899), H. L. Baumbach, S. Fried, Z. V. Jasaitis, P. L. Kirk, H. P. Robinson, R. S. Rosenfels, and E. F. Westrum, Jr.

My cold is still bad so I came home in the middle of the day.

Marjorie Osborne visited Helen during the morning.

Friday, May 10, 1946

I attended the meeting of the Heavy Isotopes Group this morning. The following other people were present: Anderson, Elson, Fineman, Hagemann, Hildebrandt, Hindman, Hopkins, Jaffey, Katzin, Kohman, Manning, Osborne, Scott, Sedlet, Stewart, Studier, R. Thompson, and Weissbourd. Anderson reported on his preparation of trivalent plutonium compounds of the rare earth type. He has prepared compounds of (NH_4, Rb, and Cs)$Pu(SO_4)_2 \cdot 4H_2O$. He assigns a coordination number of 4 (neglecting the H_2O molecules) to this type of compound. He has prepared the compound $K_5Pu(SO_4)_4$ and assigns a coordination number of 8. $K_3Pu(SO_4)_3 \cdot H_2O$ has not been identified. A discussion then ensued as to the possibility of obtaining pure crystalline forms of these compounds for x-ray pattern studies. Anderson thought that this would not be feasible because of the rate of oxidation of the Pu(III) to Pu(IV) and only one perfect crystal of this type has ever been grown.

I announced that Dempster has phoned me and said that he is prepared to measure with his alpha-ray spectrograph the alpha ranges of those isotopes whose alpha ranges differ by small amounts. Weissbourd suggested that Pu^{239} and Pu^{240} alpha-range measurements be made.

I then asked if everyone concerned with the incoming targets knew that they (the targets) are to be here at 10:00 p.m. Saturday and asked if all arrangements have been made for immediate processing. Stewart said that Hartford will do the milling this time and that everything is arranged. These are the Q-metal plus deuterons and the Np^{237} plus deuterons targets. I then asked Hindman what is to be done with the Np^{237} bombardment, and he replied that the slow neutron fission cross section of 94^{236} is to be determined. Studier, in reporting about the Th^{232} plus deuterons target, said that the pulse analysis plots of the 3-mil layer showed that the neutron-induced fission decay resolved itself into two components corresponding to that of Pa^{230} with a half-life of 17 days and Pa^{232} with a half-life of 1.4 days. The 22-mil layer showed a fission decay corresponding to the decay of Pa^{232} with a half-life of 1.38 days, indicating the absence of Pa^{230} (formed by the d,4n reaction) in this deep layer.

Roy Thompson discussed the latest findings of his protactinium solubility work which was done with the oxide.

Hopkins said that there was nothing definite to report on the Q-metal target from the Hanford pile. He is continuing to follow the uranium fraction and periodically milking it for the neptunium fraction seeking evidence for U^{240}. The neptunium fraction absorption and decay curves were similar to the Np^{239} curves. He thinks that it will be necessary to follow it for a longer period of time in order to resolve the curves more accurately.

Fineman and Weissbourd then reported on the progress of the pulse analysis of the dissolver solution from Hanford but gave no results.

Stewart made several general announcements: the plate and card punch is now in room D-4; and the section has received 20 gram allotments of neodymium, praseodymium, columbium, ruthenium, and osmium. Their purity is unknown at the time, but attempts are being made to ascertain it. There is not germanium nor gadolinium in the allowance.

Weissbourd mentioned the book reviewed by W. F. Libby in the Review of Scientific Instruments, 17, No. 4, April 1946: The Crystalcounter: A New Instrument in Nuclear Physics, by P. J. van Heerden. Weissbourd wanted to know if it might be possible for someone in the section to grow pure AgCl crystals of the proper size (1 mm x 4 mm) or to obtain them commercially. If so, experimentation along these lines could probably be done in the laboratory with the present equipment. Detection of beta and gamma-radiation in crystals will be an efficient way to count them at a high geometry. The author claims that the pulses he gets are proportional to the energy of the radiations. If true, it means that, as with the pulse analyzer, we could get Fermi plots of the energy distribution and thus do away with absorption curves. The energy of the beta is expended in the crystal and an ionization corresponding to 1-2 Mev is obtained which is comparable with alpha particles in a gas.

Scott then stated that he has found and proven a 185 kev gamma-ray in enriched U^{235} samples. He measured the gamma-rays from two different samples, i.e., different in U^{235}/U^{234} ratios. The intensity apparently follows the U^{235} content. The ratio is approximately 1 gamma-ray/3 alpha particles.

Virginia Meschke was hired as a technician in the Solvent Extraction Group today.

Manning sent James R. Gibson the Performance Review Report for Kathleen B. Florin, who is terminating because her husband, Alan, is transferring to Los Alamos. Manning states that a merit increase is not recommended only because of this reason.

The "Summary Report for April, 1946" (CC-3492) for Section C-II was issued today. There are three sections--High Temperature Pile, Special Problems, and Analytical.

I am still not recovered from my cold, so I went home to have lunch with Helen and to rest for a short time.

I received a copy of CP-3483, "Sixty-inch Cyclotron Activities - March 1945" by Joe Hamilton. Hamilton reports a second attempt to produce 132 Mev carbon ions, using CO_2 rather than acetylene, as the source of carbon. The number of carbon ions observed was somewhat less than expected. Bombardments described for March include helium ions on U^{238} and lanthanum for us in Chicago and helium ions on U^{233}, Pb^{208}, and Pb^{206} for Perlman in Berkeley.

My introductory remarks and paper, "The Heavy Elements," for the ACS Symposium in Atlantic City appeared in today's issue of <u>C & E News</u> [<u>24</u>, 1192 (1946)].

In Berkeley Stan Thompson is scheduled to go on the Radiation Laboratory payroll today.

King Victor Emmanuel of Italy has formally abdicated in favor of his son, according to today's paper.

<u>Saturday, May 11, 1946</u>

I caught up with some of my correspondence today. I wrote a letter of recommendation for Joe Katz to H. B. Hass at Purdue, saying that Katz is a versatile scientist with a good understanding of inorganic, organic, and physical chemistry; he has a very pleasant personality and would be a good staff member for a university.

I replied to Harold H. Strain's request of April 30 for names of analytical chemists who possibly might be interested in collaborating on a book. I suggest Dr. Arthur W. Adamson, now at USC, and J. Schubert here at the Met Lab. Both men have worked with G. E. Boyd.

To R. K. Summerbell of Purdue, I say that I know no good men on the instructor's level but perhaps some name will come up in the near future.

I answered a letter from Jim Gibson of Ellensburg, Washington, and tell him that I consider the two choices of schools for training in nuclear physics to be the Institute for Nuclear Studies at the University of Chicago and the University of California with its Radiation Laboratory. I say I chose the University of California for my graduate work.

To Jacob Sacks who is preparing a review article on the use of radioactive isotopes in biology, I suggest the names of Cole, Jacobson, Nickson, Curtis, Cohn, Hamilton, and Melvin Calvin as men to contact about biological work on the project.

Other letters went to Geoffrey Wilkinson, to whom I explain that he should write to Professor Latimer about the matter of admission to the University of California; to Miss Frances Benner of the ACS, to whom I say that I am about to return to the University of California and cannot accept the ACS lecture tour this year; to R. Sergeson of Detroit, for whom I describe the problems about the alleged discovery of the element illinium and suggest that he contact Coryell for more information; to Miss Magill of Pittsburgh, who wrote about the treatment of leukemia, and to whom I explain that the work is only in the experimental stage and suggest that she write to John Lawrence in Berkeley; and finally to Ernest Manfred of New York, to whom I sent the photograph and autograph he requested.

I went home and had lunch with Helen today. Alice Katzin dropped by to see her later.

Sunday, May 12, 1946

At 1:30 p.m. Helen and I had Sunday dinner with the Royal Smiths. In the late afternoon we visited the Quiz Kids radio show and then had dinner with the John Lewellens in Glen Ellen. There we met the Neil Hamiltons. Hamilton, a star of many silent movies, will narrate with me a record about atomic energy for Lewellen.

Monday, May 13, 1946

Today Karl K. Darrow wrote me a note to say that he will schedule my talk to the Chicago meeting of the APS on June 22 at 2:15 p.m.

I wrote a letter of recommendation for Norman R. Davidson to Robbin C. Anderson at the University of Texas. The letter is very similar to the one I wrote to Summerbell at Northwestern University recently.

A courier left Chicago today for Berkeley with many of the samples that we agreed to share.

In the afternoon I went to the recording studio to participate with Neil Hamilton in the production, for John Lewellen (Lewellen's Club Productions), of a popular recording describing atomic energy.

In Berkeley Burris Cunningham is scheduled to go on the Radiation Laboratory payroll at $450 per month.

"10-1 Vote in City Opposes Giving A-Bomb to Reds" reads the title of an article on the front page today. People in Chicago were surveyed and said they did not feel the Soviet Union could be trusted with such a weapon.

Tuesday, May 14, 1946

Alan E. Florin terminated at the Met Lab today. He is transferring to Los Alamos. A technician, Erwin Roy John, began working in the Heavy Isotopes Group under Darrell Osborne.

Ralph Lapp prepared an interesting memorandum about Operation Crossroads for Farrington Daniels which Daniels issued today:

> You have undoubtedly heard that the Metallurgical Laboratory is sending several groups of scientific personnel to the South Pacific to participate in the Atom Bomb Tests to be held this summer. This bulletin is being issued so that, as members of this Laboratory, you may have additional information about the operations.
>
> Bikini atoll, where the bomb tests will take place, is one of the northern most of the Marshall Islands, which were wrested from the Japanese about two years ago. Bikini is a long way from most anywhere; for example, it is 1,573 miles from Guam, 2,096 from Honolulu, and 4,150 from San Francisco. The lagoon, where the test will occur, is about 14 x 20 miles in dimension and is ringed by a narrow fringe of low lying land, opening on one side to the ocean.
>
> Some of our scientists will make the trip to Bikini aboard naval ships while others will travel in C-54 transports. They will leave the west coast for Pearl Harbor and then will continue on to Kwajalein. Kwajalein lies 170 miles from Bikini

and it is a base of operation for certain of the bomb tests crews inasmuch as it is the nearest air field to Bikini. It is from this point that a lone B-29 will take off early on a July morning to drop the fourth Atom Bomb ever to be detonated. This will be an overwater drop and it will explode hundreds of feet above the surface of the lagoon.

In the lagoon will be a fleet of ships arranged radially from a centrally placed target ship. This target ship will be surrounded by a fleet of 47 major ships including two aircraft carriers, four battleships, two cruisers, 16 destroyers, eight submarines, and 15 U.S. transports. In addition, there will be other ships of lighter class and there will also be ships from the Japanese and German fleets. The prize heavy cruiser of the 19,500 ton class, the Prinz Eugen, reputed to have sunk the Hood, will be exposed to the blast. The light cruiser, Sakawa, along with the 32,700 ton battleship Nagato, the flagship of Admiral Yamamoto, will be in the target fleet. Several famous U.S. ships will form part of the fleet; these include the oldest U.S. carrier, the Saratoga, the cruiser Salt Lake City, as well as the battleships Pennsylvania and Nevada, both of which were victims to the Japanese attack at Pearl Harbor.

Most of our scientists will be stationed aboard ships lying about 20 miles from the target ship and will be equipped with special goggles to view the explosion. The bomb to be detonated is of the same type as that which was dropped on Nagasaki last August.

Prior to the explosion, our men will have placed instruments aboard the target fleet and immediately after the shot, they will set out in small craft to survey the lagoon prior to boarding the ships. During this phase of the operation, the men in the survey craft will operate a series of electronic instruments developed to measure the radioactivity which will remain in the lagoon. They will be in touch with a central control station by means of two-way radio and no one will be allowed to enter any area found to exceed our allowable activity level. After careful surveys have been made, the ships will be boarded and instruments and animals removed therefrom. Then the real work of the scientists will begin, for then the data must be collected and analyzed.

The exact date for Shot A, as it is called, has not been set and it will depend upon weather conditions. It should, however, take place during the first three weeks of July. Early in August, again depending upon weather conditions, a second shot (Shot B) will take place. This will be an on-the-surface shot. Some of our scientists will witness both Shots A and B, while others will participate in only one of the shots.

Upon their return to Chicago (late in July and again early in September) our scientists may be allowed to give their personal impressions of the tests at a meeting for all laboratory personnel.

We wish these scientists the best of luck, a safe journey, and good results.

John W. Tietz of DeWitt Clinton High School in the Bronx wrote asking whether my Pittsburgh paper on the use of tracers would be published. I replied today saying that it would not appear in print, but enclosed a mimeographed copy of my talk for his use.

W. G. Young and Saul Winstein invited me to be Phi Lambda Upsilon's Morgan Memorial Lecturer of 1946. Yesterday I received a formal invitation from Ernest Grunwald, President of Alpha Kappa Chapter at UCLA. The Morgan Lecture is usually given at the end of May or early in June. Today I wrote Grunwald, accepting the invitation and suggesting a date such as June 5. I say the title of my talk will be "Atomic Energy and the Transuranium Elements."

I also wrote to Miss Nell A. Parkinson of C & E News to tell her that I shall be glad to review for C & E News the State Department Committee's "Report on the International Control of Atomic Energy." I also apologize for the delay in sending her my review of the Smyth Report.

In another note I tell Stewart Kurtz, Jr., Chairman of the Gibson Island meeting, that I shall arrive on the morning of June 20 and leave on the morning of June 21. I say that I must attend meetings in Chicago both the beginning and the end of the week and will be unable to get in the few rounds of golf on Gibson Island during the week that I had anticipated.

Branch announced, by memo, that the last round trip of the station wagon between Argonne and the Met Lab will be eliminated on May 20 because of lack of use.

Helen had a 1:00 p.m. appointment with Dr. Davis today.

Wednesday, May 15, 1946

I attended my last meeting of the Heavy Isotopes Group this morning. Manning conducted the meeting which was attended by Anderson, Elson, Erway, Fried, Hagemann, Hildebrandt, Hindman, Hyde, Jaffey, Katz, Katzin, Kohman, Leader, Manning, Osborne, Scott, Sedlet, Simpson, Stewart, Warshaw, and Weissbourd. Manning reviewed the heavy isotopes program. The problems under this are briefly summarized as follows:

(1) To work up the high gt plutonium samples known as CT-4 to obtain material high in Pu^{240} for total neutron absorption coefficient measurements. (2) To obtain and study the properties of U^{236}. This would be done by milking the uranium from the highest gt plutonium available which is that from the last neptunium concentration run. This should yield about 10 percent U^{236}. (3) To milk Th^{234} from U^{238} and to let this thorium decay to U^{234}. The fission cross section of U^{234} should be checked, since the only value obtained for it so far may possibly be in error, as a possibility existed of traces of U^{235} being present in the original experiment. There was some discussion of this point with Katz pointing out that Mallinckrodt should have some U^{234} available. Katzin commented that this was not pure enough and had already been considered. Kohman commented that the Mallinckrodt residues might be a good source of $Th^{234}(UX_1)$ which could be highly purified and allowed to decay to U^{234}. (4) To bombard some Pa^{231} at Hanford primarily to produce U^{232} and also to get the neutron capture cross section of Pa^{232}. (5) There are several samples now being irradiated at Clinton to prepare material for rechecks of fission cross sections. One of these is a sample of americium to produce 62 for recheck and the other is of protactinium to give U^{232} for recheck. (6) To milk the large stocks of high gt plutonium for americium from time to time. (7) To bombard some highly purified neptunium at Clinton to produce Pu^{238} for a fission cross section check. (8) To work up the thallium now being bombarded at Clinton to produce the three year beta-emitting isotope. There is a faint possibility that this isotope may be fissionable, and it should be checked. It will also be a good source of a pure lead isotope as it decays.

Simpson asked about the supplies of americium in the section, as Erway is now working on the determination of vapor pressure of americium metal by using high gt plutonium. The availability of 5 mg of americium metal would permit a direct determination to be made. Manning replied that at the present time the only americium available will be that growing into the plutonium cows. A more accurate determination of the rate of growth of 51 in plutonium should permit more accurate estimates of the vapor pressure using the method now being used by Erway. Simpson agreed that this is possible and stated that milking to determine this figure might be done by a physical method by boiling out the 51 from plutonium at periodic intervals, since the vapor pressures would appear to differ by a factor of a thousand.

Jaffey then suggested several other problems that might be considered under the heavy isotopes classification. The first is a more careful determination of the decay schemes and half-lives of such isotopes as UY, Pa^{232}, and Np^{238}. This could be done by the use of a coincidence counter. Such an instrument has been constructed but has not yet been put into operation. A second problem is to consider the energy dependence of alpha-ray backscattering. The construction of a beta-ray spectrograph will probably have to be delayed until more manpower is available. Katzin commented that a number of counters for various jobs can be used particularly in getting more details on such things as the decay products of the 4n + 1 series and Pa^{232}. Weissbourd pointed out that another pulse analyzer will be needed for this work, one that could be reserved entirely for research on specific problems rather than as a service instrument for the section. Fundamental studies, such as a consideration of the mechanism of ion collection in counting chambers could also be tackled with such an instrument. Simpson stated that Willard's section has need of a beta-ray spectrograph. Freedman is interested in constructing one of the lens type. This led into the question as to whether two separate groups of instruments should be maintained in the division. Manning asked Jaffey to investigate the possibility of coordinating instrument maintenance and development between the two sections. It was pointed out that the Ryerson instrument group might be made part of this coordination plan and that the development of many instruments for special problems might be left to them.

Katz and Fried then reviewed some of the problems of interest in dry chemistry. Among these were mentioned: (1) the preparation of halides, sulfides, and oxides of elements 89, 91, 93, 94, 95; (2) another preparation of neptunium hexafluoride and redetermination of its lattice constants; (3) the reactions of NpF_3 and NpF_4 with oxygen; (4) the preparation of borohydrides of neptunium; (5) a consideration of the neptunates; (6) a systematic investigation of the uranium-oxygen system from the point of view of the fundamental mechanisms involved, including an extension of the present knowledge of uranic chemistry; (7) a study of the various crystal modifications of U_3O_8 particularly considering their phase relations and comparative reactions; (8) a study of the hydrolysis of uranium carbide; (9) preparation of heavy metal hydrides by Grignard reagents at low temperatures.

Hindman enumerated some problems in wet chemistry. Among these are a study of oxygenation of neptunium ion in solution; elucidation of the hydrolytic behavior of U(IV); a study of the complex ions of Np(V); and the construction with Tomkins of a recording spectrophotometer. Further work on the thermodynamics of some of the heavy ions in solution is also indicated. This will probably require a specialist of Ph.D. rank.

Osborne noted that along the line of thermodynamic studies, the determination of high temperature and low temperature heat capacities should be started and another calorimeter should be constructed to replace the one shipped to Berkeley.

Anderson said he is planning to continue the preparation of plutonium compounds, about 24 of which have already been made. The valence of three plutonium combinations with sulfate and other ions is being worked on at the present time.

Simpson mentioned he wants to start a program on the determination of magnetic moments. This will probably require at least two men of Ph.D. grade; he has one experienced man in mind who may be available. The first problem will be the determination of the electronic moment of plutonium by the standard molecular beam method. After the electronic moment has been determined, work will be done on the nuclear moment. After this, the same sort of thing could be done on other elements using their radioactivity as a means of locating the beam. Work with some of the fission products may be possible in this fashion. High vacuum experiments of the type now being made by Erway will also be continued. Hindman asked about the determination of ionization potentials, and Simpson said this again is a matter of manpower. The problem has been under consideration for some time.

Hindman asked if we are interested in C^{14} and Katzin said it might be used in preparing solvents for studying the mechanism of solvent extraction. The amount of solvation of the extracted ion and the extent of the solubility of the organic and aqueous phases in each other might be determined by this means. Kohman suggested that radioactive carbon could also be used in studying the decomposition products of solvents. Simpson commented that perhaps this C^{14} problem might best be developed in its primary stages in universities having strong organic chemistry departments.

Osborne noted that, as an additional problem, the recovery and chemistry of protactinium should be further studied. Manning then said the development of a method of isolating ionium from some of the St. Louis raffinates is also something which should be done. The actual processing could be farmed out. Sedlet said he might be interested in this problem. He is now finishing Ames' work on the half-life of radium.

Katzin named the study of the 1.8 centimeter alpha-emitting isotope in mine ores and more work on the members of the 4n + 1 and Pa^{230} series as problems for study, in addition to the consideration of the mechanism of solvent extraction already described.

Manning said that there are also some 80 pounds of thorium being irradiated in the Hanford pile to produce large quantities of U^{233} and we may want to work up some of these slugs here. He then said that any further expression of opinions should be turned in within the next few days to permit him tentatively to schedule manpower distribution. He noted that now is the time to make suggestions for special provisions to be made in the new chemical building at Argonne since plans for its construction are still in the formative stage.

A telegram and a letter arrived from J. C. Dart of Standard Oil of New Jersey asking me to address a joint meeting of the Baton Rouge Section of the American Institute of Chemical Engineers, the American

Chemical Society, and Sigma Xi during the last week of May on a topic such as "Social Significance of Atomic Energy." When I replied that I could not do it because of other commitments, Dart asked for help in securing another speaker. I wired him today suggesting Winston Manning.

Frank Belletire, a former technician who resigned to enter the Army, returned to our payroll today.

Ed Westrum wrote from Berkeley that he has sent two rough copies of the bulk of the plutonium fission report to Al Ghiorso, saying that he has left for Ghiorso the matters of handling the early history, instrumentation, and explanation of the poor quality of the work. Westrum mentions that Hindman has had the specific activity draft for about two weeks. He then asks if there is any hope of obtaining a mass spectrographic report from Site Y on plutonium sample 49NH, and he then notes that his laboratory at Berkeley is 85 percent complete.

I resubmitted to Hoylande Young a paper to be used as the basis for my talk to the APS meeting in Chicago on June 22 and other talks in June and July. I state that, according to the Declassification Guide of March 30, 1946, the items previously deleted are now declassifiable.

Helen went downtown and had lunch with Dorothy Paul. She and I then spent the evening with Darrell and Marjorie Osborne.

Thursday, May 16, 1946

Roy Thompson, Jr., who is transferring to Berkeley, terminated work here in Chicago today.

When I was in Oak Ridge in March, Elwin Covey told me of his desire to return to school in Berkeley, but he will need a part-time job to do this since he now has a wife and son to support. Today I called Spof English to tell him that I am leaving for Berkeley tomorrow. I asked him to check with Elwin about when he (Elwin) would be able to leave Oak Ridge and whether he would like to work full time during the summer.

Don Stewart sent a revised description of the samples of plutonium, americium, neptunium, protactinium, ionium, polonium, and radium sent to Berkeley on Monday. Stewart mentions that he has submitted Perlman's sample of 171 gt 49 for spectrographic analysis but there will be a delay before the samples can be run. He then says that he plans to be in Berkeley June 10-11 enroute to Bikini on his Operation Crossroads trip.

M. W. Welch of W. M. Welch Manufacturing Company called to discuss my chart of isotopes which they are going to publish.

I submitted an additional paper for declassification to Hoylande D. Young, describing the sections of the Guide applicable and stating that I want to submit the paper to Physical Review.

I also wrote to Dr. Elizabeth Rona in Washington, D.C., who has requested to work with our group in Berkeley. I say that we are hopelessly short of space in Berkeley for a year or two and suggest that she contact Winston Manning as laboratory space here is better situated.

A thank-you note arrived from Robert Sergeson for the information I sent him on May 11 about the element illinium.

Helen and I had dinner at the home of my secretary, Ruth Rogers.

The Army and Navy are looking for underground hideouts for war industries in the future. They expressed doubts that safe places could be found in the atomic age, according to this morning's newspapers.

Friday, May 17, 1946

Don Stewart wrote to F. H. Spedding at Ames to ask if he would again arrange for the production of U^{238} metal cylinders. He also asks whether Spedding could make the finished targets as we experienced metal losses during fabrication here. Stewart additionally requests that Spedding arrange for the preparation of similar thorium (low uranium content) targets.

W. H Sullivan sent me, in a letter which arrived today, data on tungsten and rhenium isotopes, suitable for inclusion in the "Table of Isotopes."

I terminated at the Met Lab today--just over four years since I first arrive here for this project.

I find that I have 16 days of accrued leave--leave that I was unable to take because of the press of work. I hope that I will be reimbursed for this time.

The furniture which we rented from McDougalls for use during our stay since the fall of 1943 at our apartment at 5418 Woodlawn Avenue was picked up this morning. Helen and I left Chicago in the late afternoon on the "City of San Francisco."

Saturday, May 18, 1946

Helen and I are enroute to Berkeley on the "City of San Francisco." I am suffering from a migraine headache.

Sunday, May 19, 1946

Helen and I arrived in Berkeley this morning and went to our new home at 836 Washington Street in Albany. Jo Owen and Lee Perlman came by to greet us.

I am ready to resume my academic position, professorial level, in the Department of Chemistry, University of California, Berkeley, and to start my position as head of the newly formed Nuclear Chemistry Division in the Radiation Laboratory.

* * *

Miscellaneous pictures covering the entire Met Lab period, not available in time for inclusion in the earlier volumes, are shown in Figures 60 through 75.

On July 14, 1979, a reunion of many Met Lab colleagues was held at our home at 1154 Glen Road, Lafayette, California. Pictures of these people taken at that time are seen in Figures 76 through 118.

XBB 790-16596

Figure 60. Jerry Howland with his niece, Carol Sorenson in Janesville, Wisconsin, Summer 1943. [At 21 Carol won the British Women's Amateur Golf championship while in England as a member of the U.S. Curtis Cup team.]

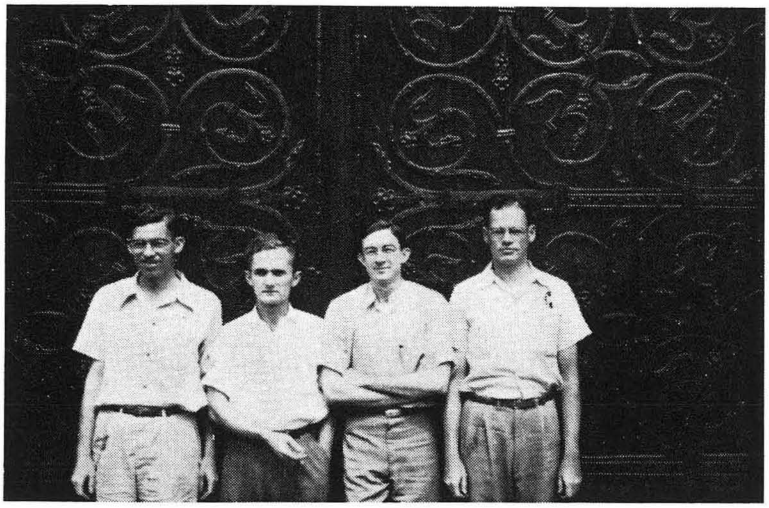


Figure 61. David Templeton, Tom Morgan, Fowler Yett, Roy Thompson, Summer 1944.

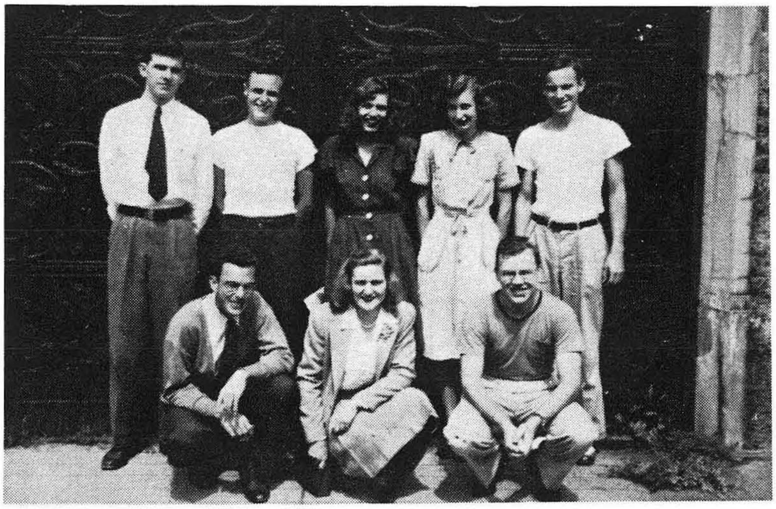


Figure 62. Top row: Walter Beard, Leonard Niedrach, Shirley Nyden, Edrey Smith, Horace Hopkins. Bottom row: Jonathan Dixon, Ruth Casler, Bob Goeckermann, Summer 1944.

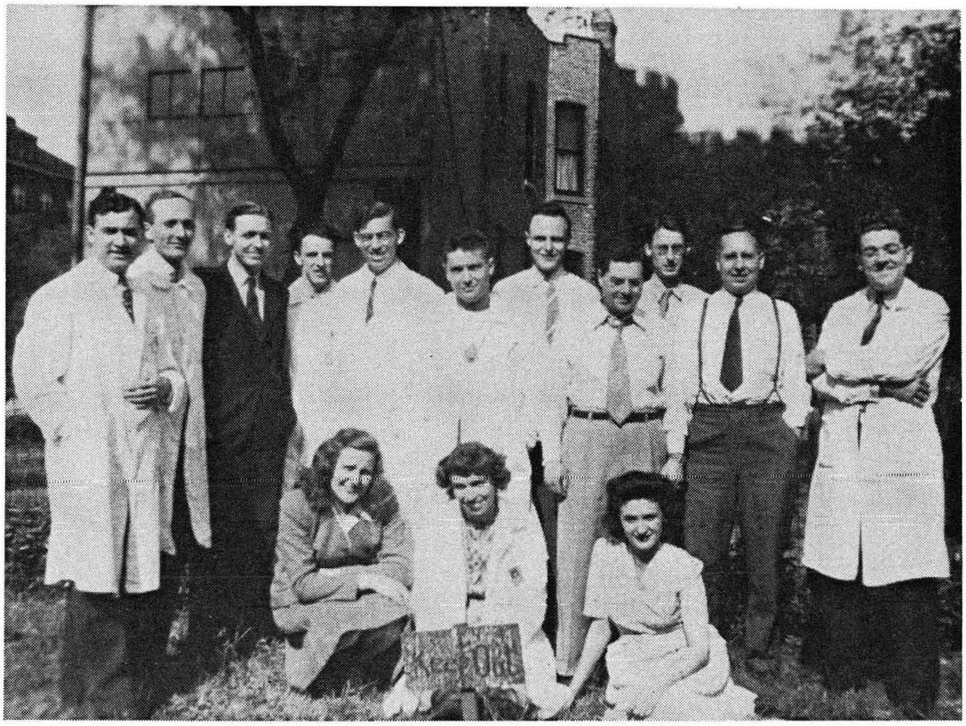


Figure 63. Section C-IV, Spring 1945. Top row, left to right: David Revinson, Stanley Rasmussen, James Watters, Ralph Bane, David Templeton, Kenneth Jensen, Leonard Niedrach, Mark Fred, Beverley Lewis, Sigurd Sheel, Ralph Telford. Front row, left to right: Ruth Casler, Rosanna Carter, Irene Corvin.

Figure 64. Section C-II, 1944. Top row, left to right: Augustine Allen, Sheffield Gordon, Boris Leaf, Robert Platzman, Aaron Novick, Milton Burton, Remson Schenck, Francis Safford. Second row: Robert Penneman, Robert Livingston, Alexander Van Dyken, Ellsworth Smith, Clarence Hochanadel, John Ghormley. Third row: Robert Pairs, Theodore Neubert, Louise Weil, Arild Miller, Gertrude Steel, John Flanagan, Joseph Royal. Fourth row: Melvin Bowman, Christine Bane, Norene Mann, Lucille McCormick, Eleanor Phillips, Ruth Boe, Leonard Treiman.

Figure 65. New Chem, Ingleside Avenue,
looking toward 56th Street, 1946?

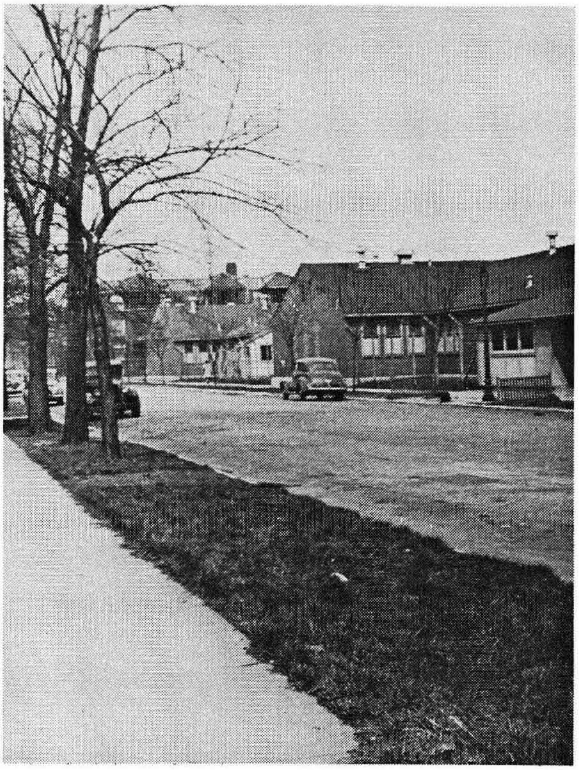


Figure 66. New Chem, Ingleside Avenue,
looking toward 56th Street, 1944-45?

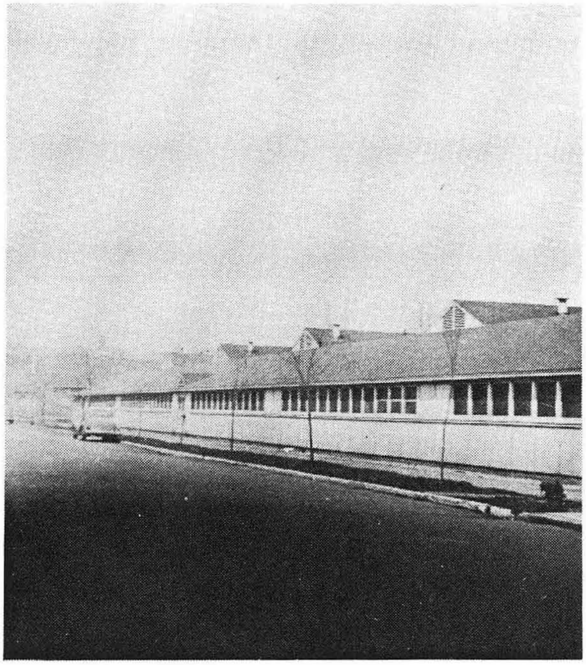


Figure 67. Chem Annex, Ingleside Avenue, 1945?

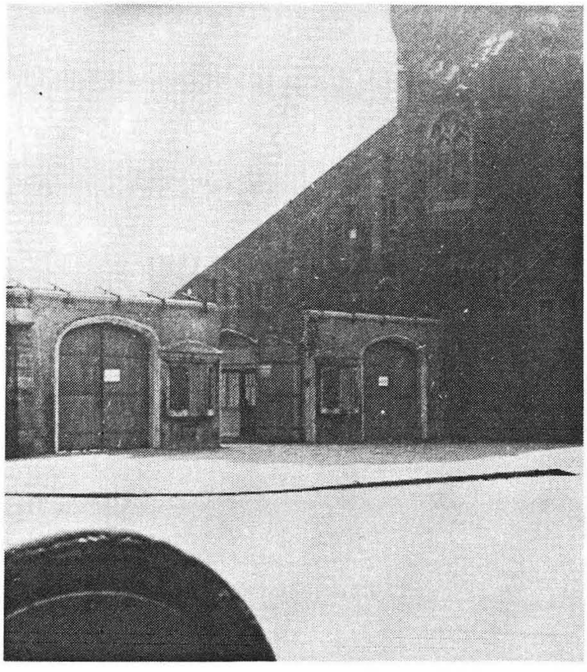


Figure 68. West Stands, Spring 1946.

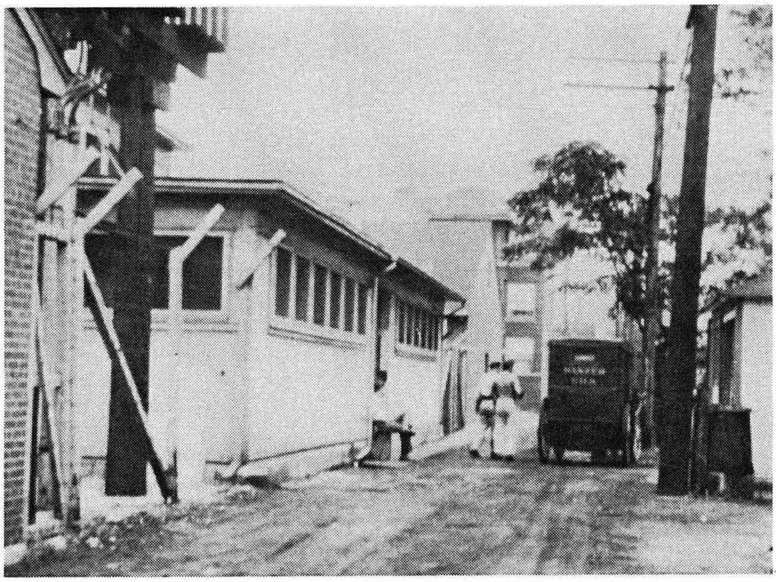


Figure 69. Alley in back of New Chem, 1945?

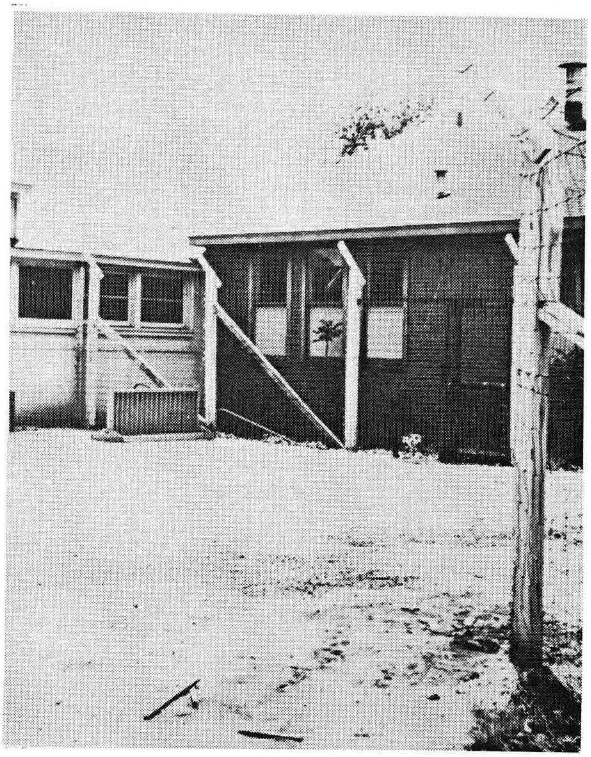
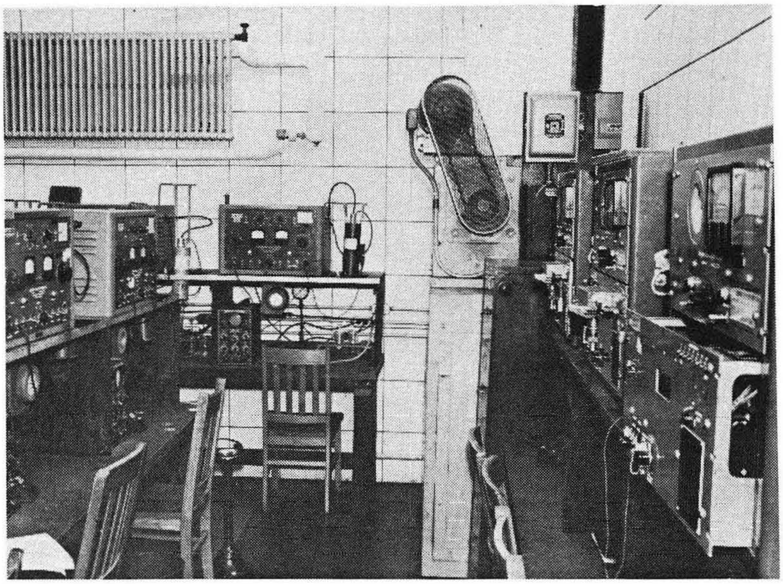


Figure 70. Behind New Chem, 1945?



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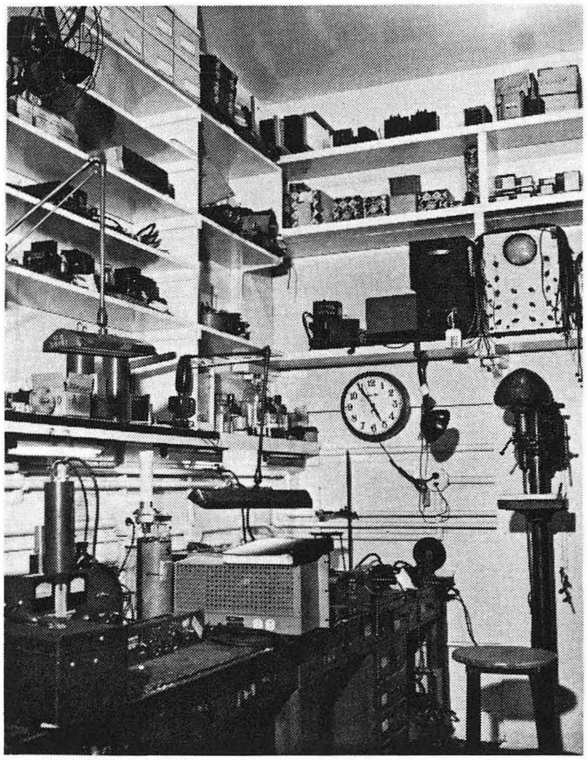

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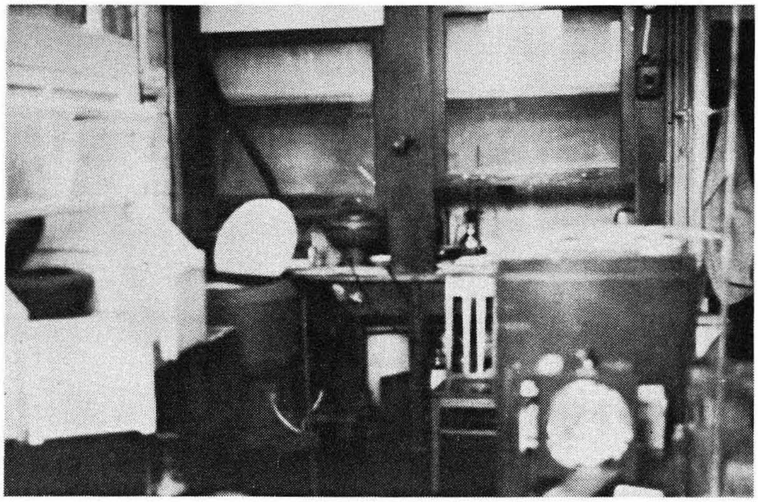
Figure 72. Electronics Shop (Room 19), Northwest corner, New Chem, 1945.



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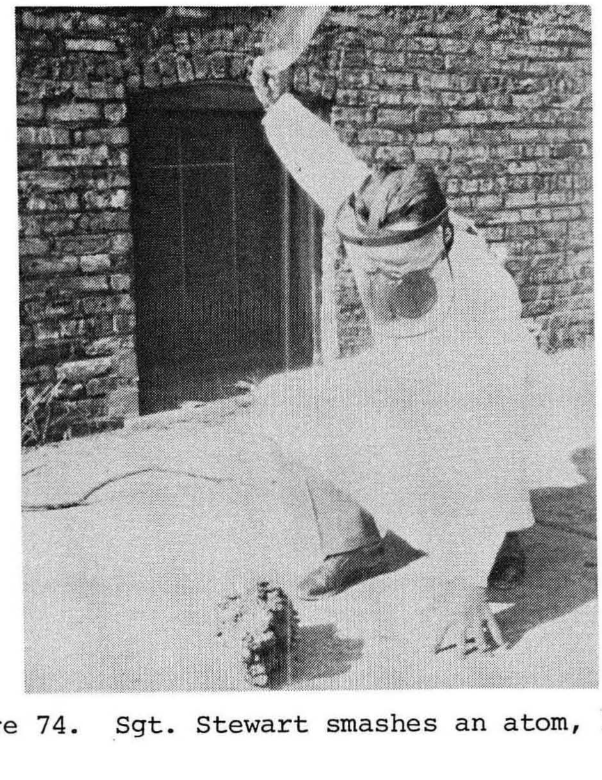


Figure 74. Sgt. Stewart smashes an atom, 1945.

Figure 75. Olga and Phillip Fineman's Wedding Celebration Dinner Party, Bryn Mawr Bowling Lanes Restaurant, 2015 E. 71st Street, Chicago, Fall 1946. Left side, foreground: Louis Kaplan, Caroline Kennedy, Howard Littland, Guest of Howard Littland, George Bernstein, Anita Tobias, John Schraidt, Phyllis Schraidt, Betty King, James King, Matt Walling, Rachel Whelan, Sydney Gaarder, Guest of Homer Tyler, Homer Tyler. Head table, left to right: Joseph Jacobsen, Guest of Joseph Jacobsen, William Walters, Phillip Fineman, Olga Giacchetti Fineman, Lester Coleman, Beatrice Davenport, Lillian Guadagna. Right side, foreground: Roberta Wagner, Betty Grossman, Roy Post, Becky Post, Ray Meschke, Virginia Meschke, John Natale, Jean Natale, Lee Gaumer, Seymour Vogler, Sherman Greenberg, Milton Klein, Winston Manning, Ruth Hyman, Herbert Hyman, Milton Ader.

Figure 76. Fred and Edrey Albaugh

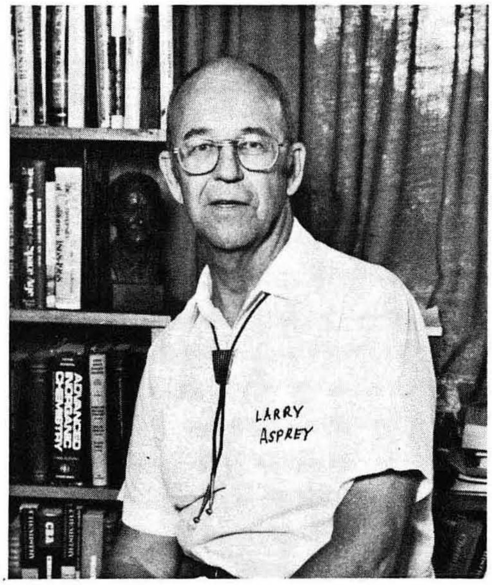


Figure 77. Larry Asprey

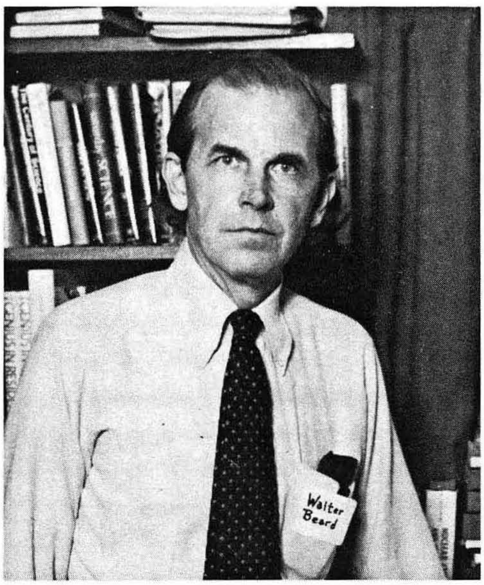


Figure 78. Walter Beard

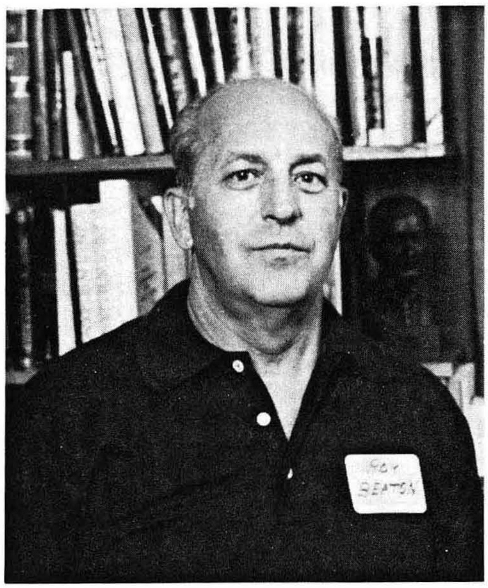


Figure 79. Roy Beaton

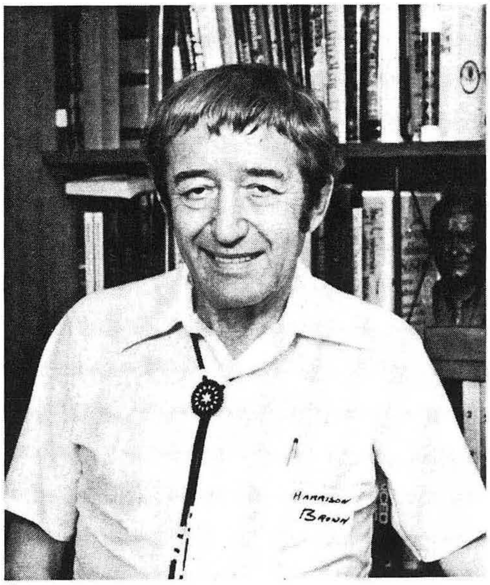


Figure 80. Harrison Brown

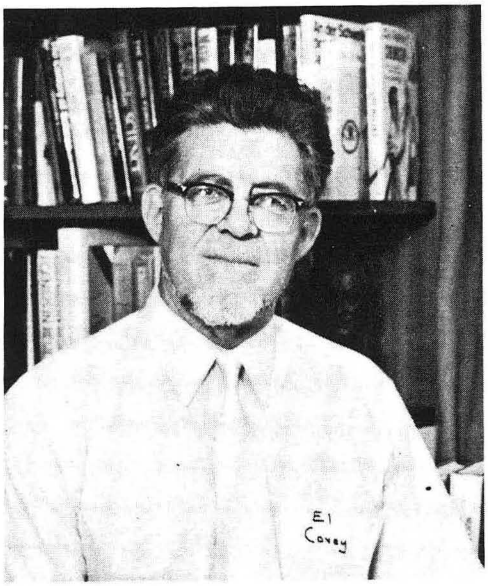


Figure 81. Elwin Covey

Figure 82. Lorraine Crawford

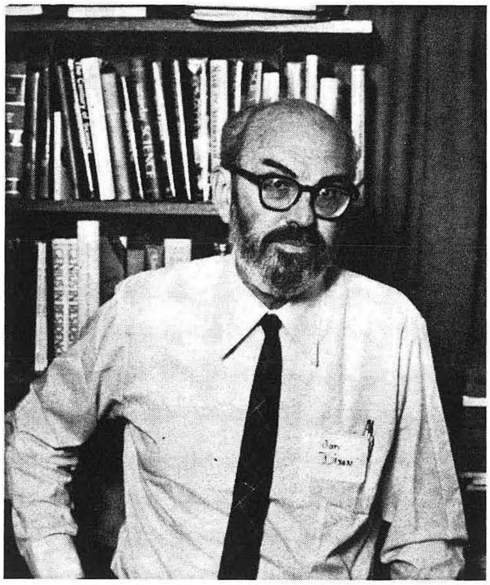


Figure 83. Jonathan Dixon

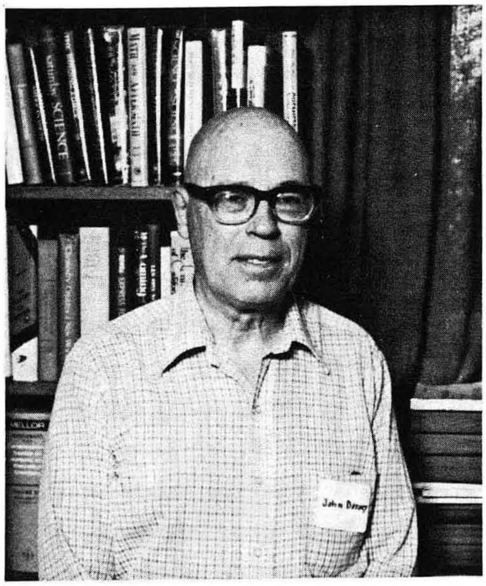


Figure 84. John Dorsey

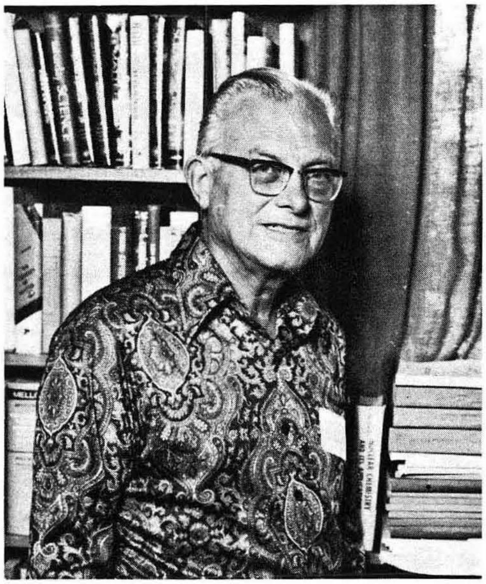
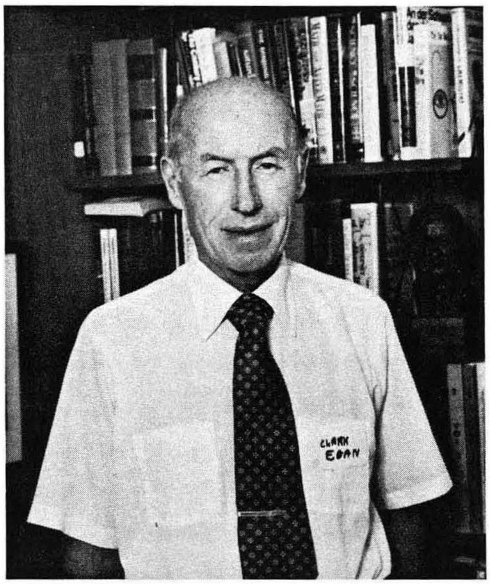


Figure 85. Leonard Dreher



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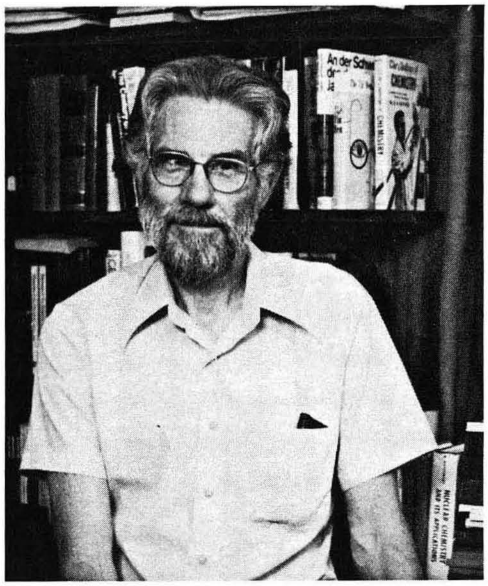

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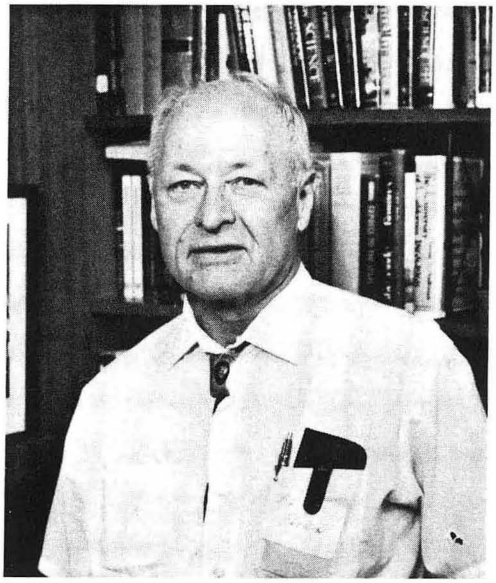

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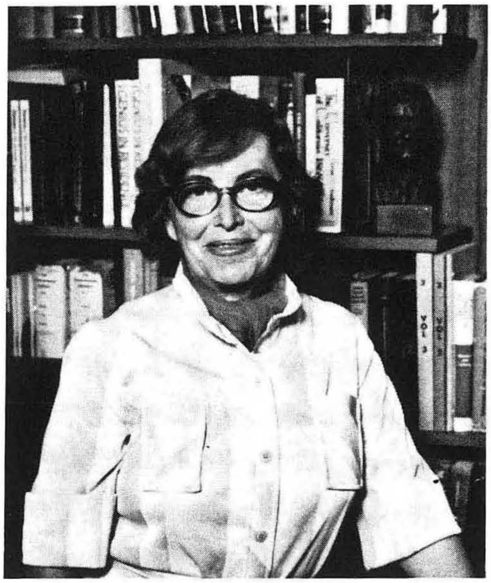

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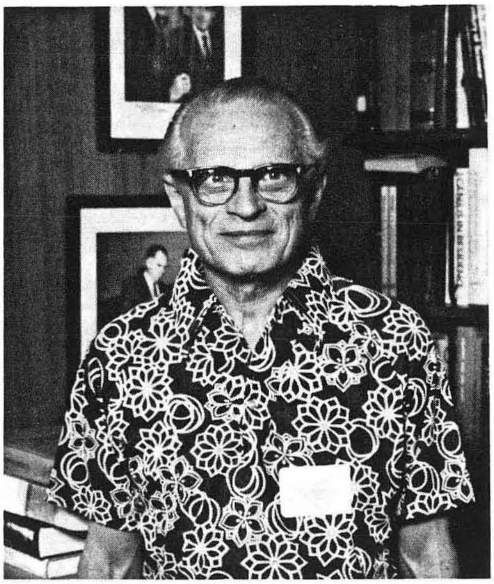


Figure 90. Bernard Fries

Figure 91. Al Ghiorso

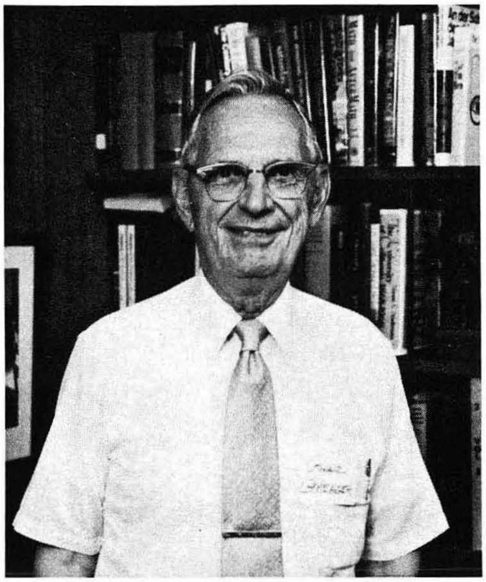


Figure 92. Oswald Greager

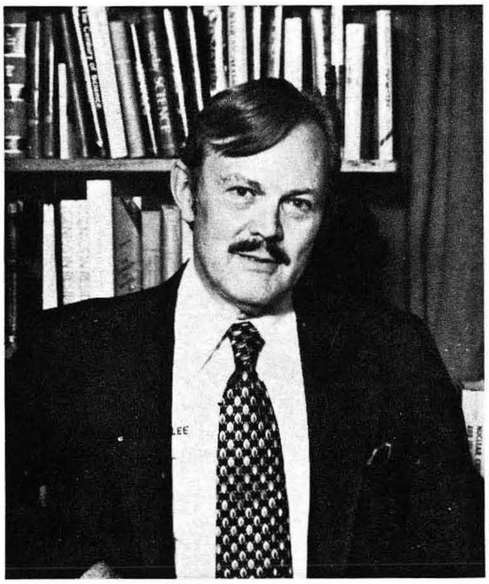


Figure 93. Roy Greenlee

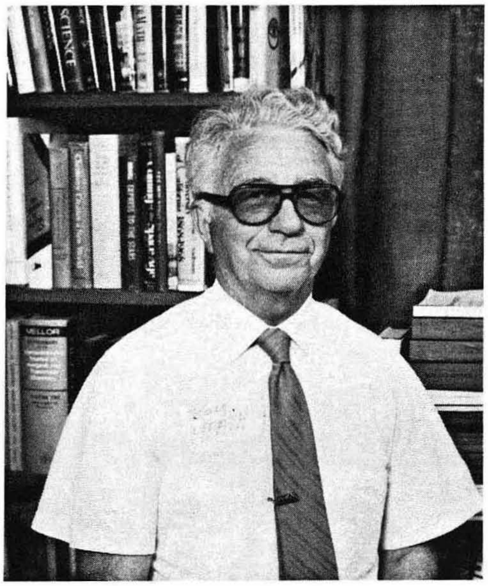


Figure 94. Roy Heath

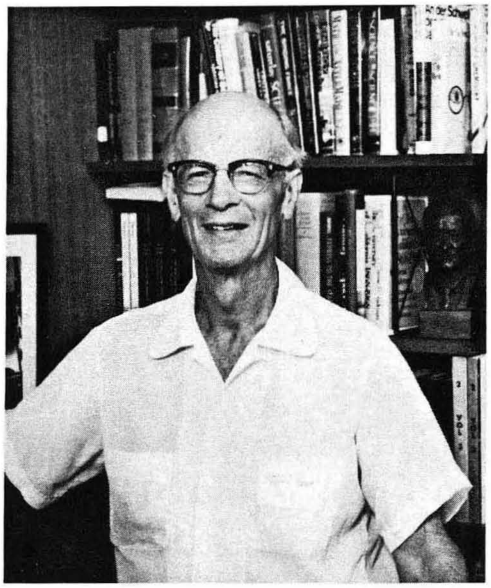
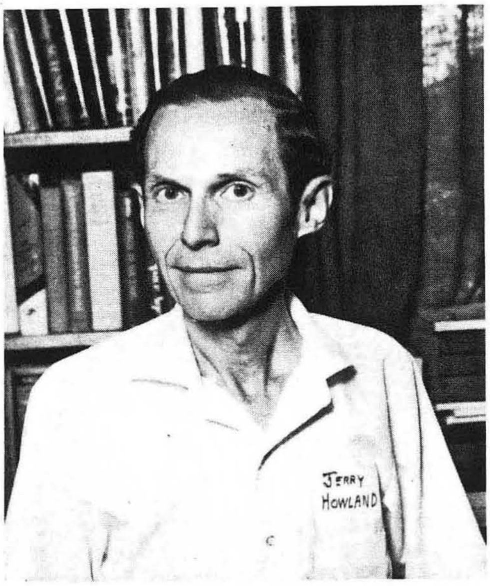


Figure 95. John Howe



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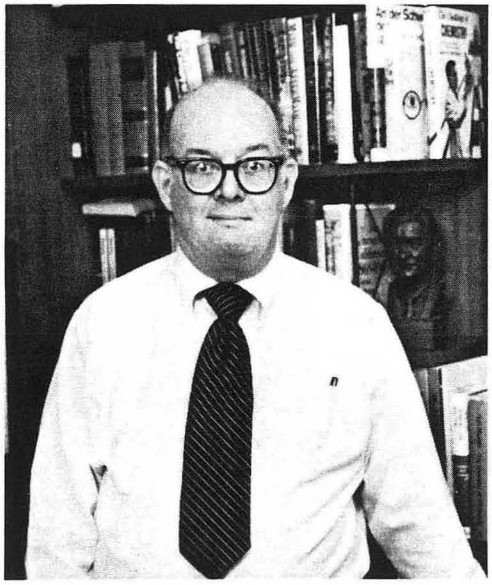
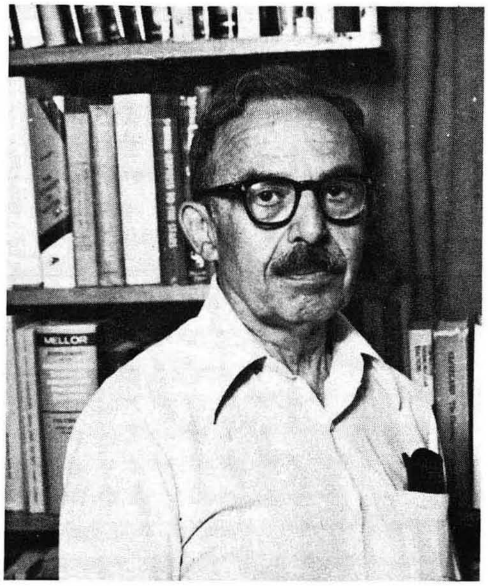


Figure 99. Harry Kamack



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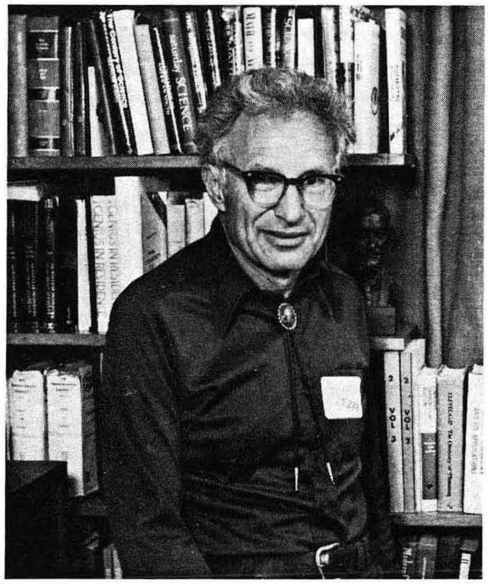

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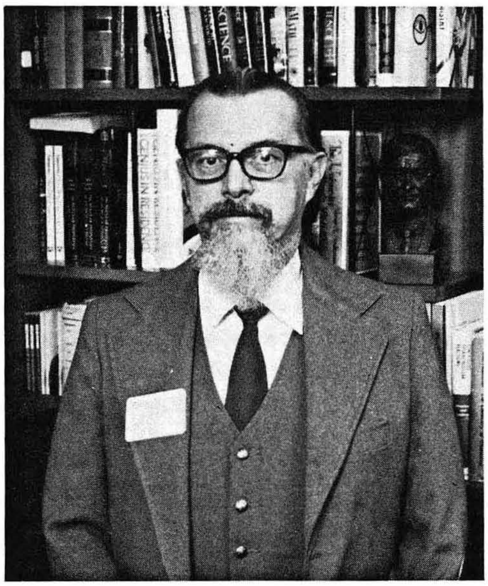
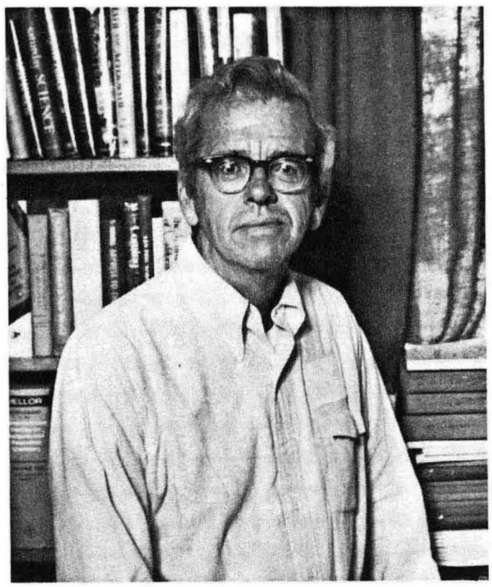


Figure 102. Alec Kelley



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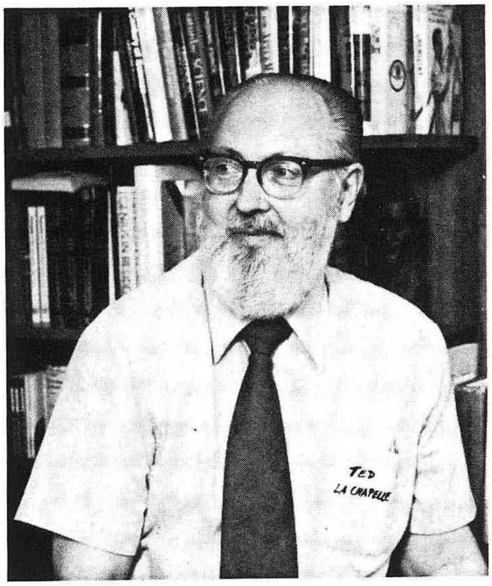

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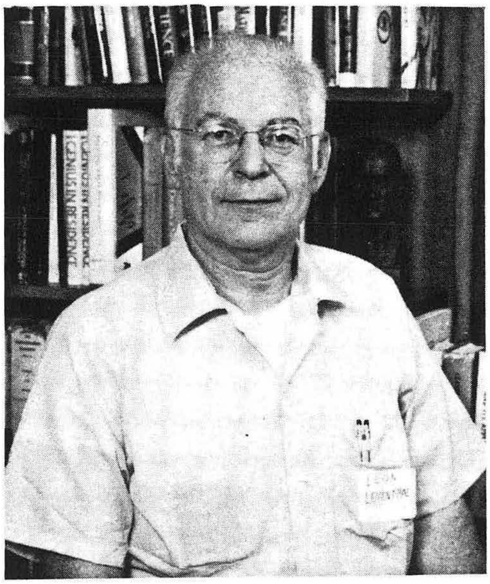

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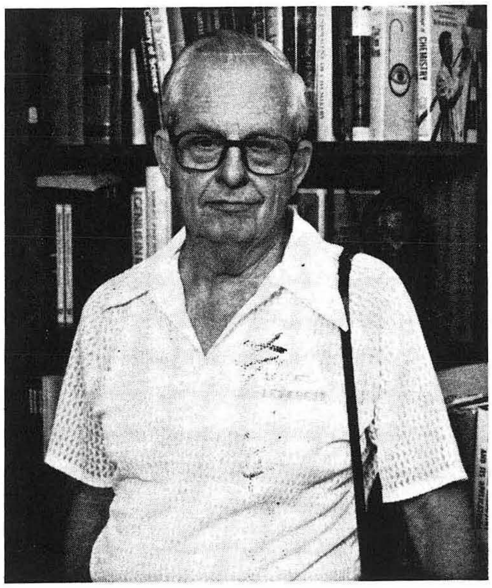

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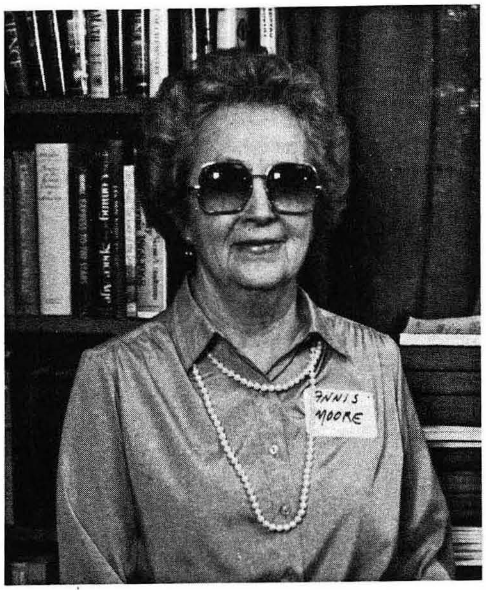

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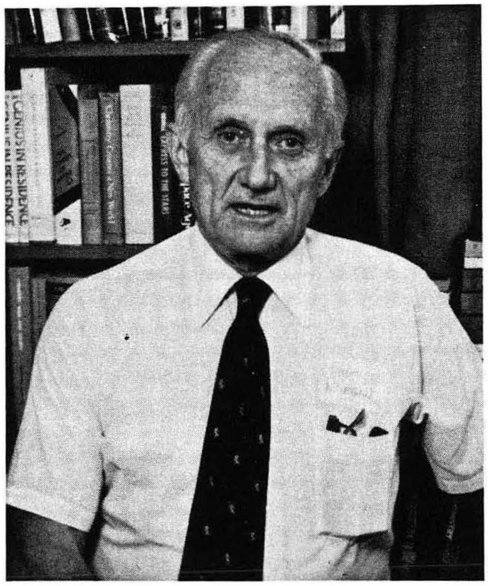


Figure 109. Tom Morgan

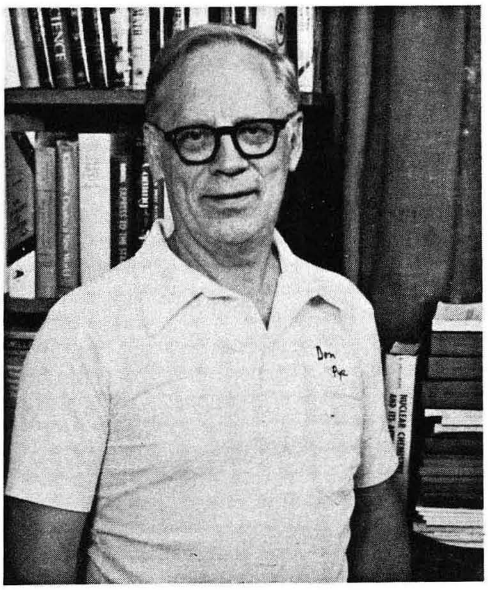
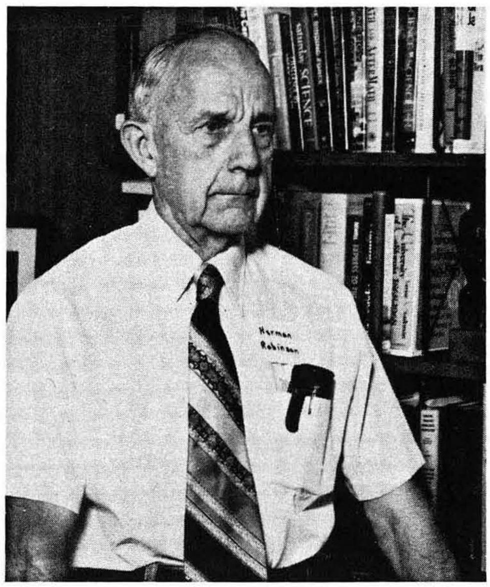


Figure 110. Donald Pye



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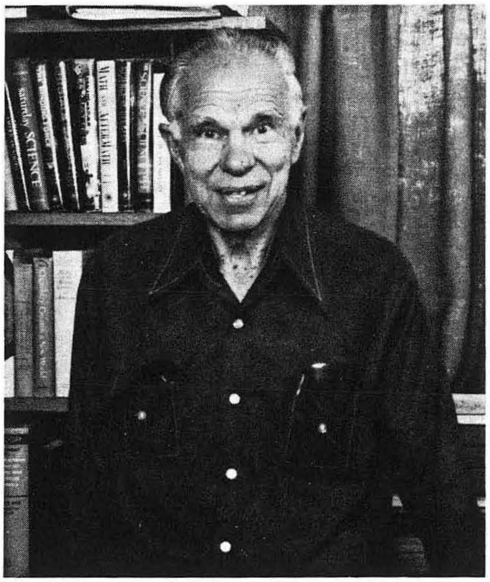

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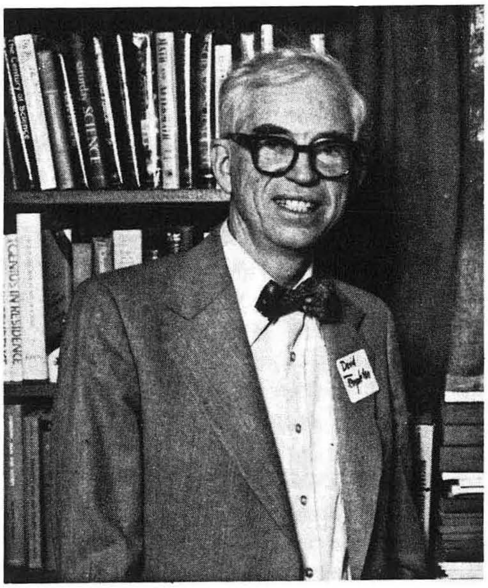

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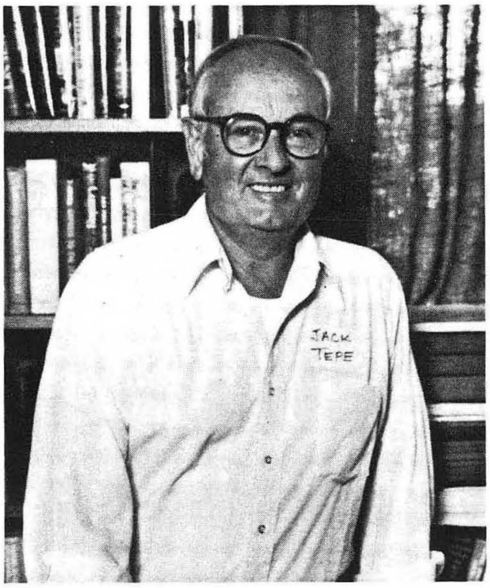

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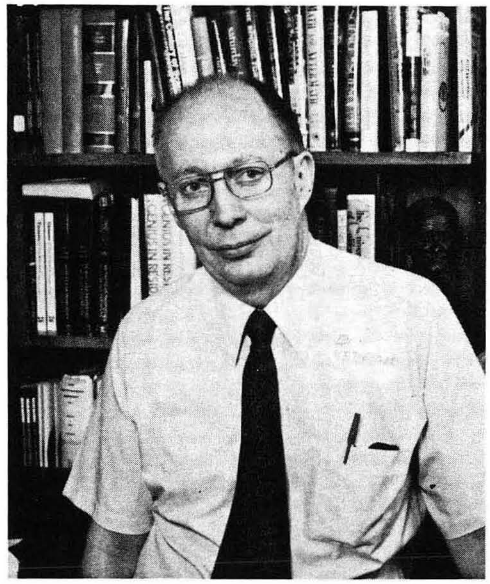
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Figure 118. Met Lab Section C-I Reunion, July 14, 1979. Standing, left to right: Clark Egan, Stephen Lawroski, William Knox, Herman Robinson, Fred Albaugh, Edrey Albaugh, Harrison Brown, John Howe, J. Leonard Dreher, Leonard Katzin, Roy Greenlee (back), John Dorsey (front), Earl Hyde, Larned Asprey, Donald Wetlaufer, David Templeton, Walter Beard, Donald Pye, Harry Kamack, Sonia Katz, Lorraine Crawford, Ralph Firminhac, Quentin Van Winkle, Jack Tepe, Zene Jasaitis, Jonathan Dixon (back), Ted La Chapelle. Middle row: Al Ghiorso, Nancy Leverett, Marilyn Howe, Annis Moore, Marge Asprey, Helen Seaborg, Glenn Seaborg, Bernard Fries, Roy Beaton, Leon Leventhal, Roy Heath, Mary Ellen La Chapelle, Marguerite Dorsey. Front row: Alec Kelley, Miles Leverett, Al Florin, Kay Florin, Elwin Covey, Joe Katz, Elton Turk, Tom Morgan, Jerome Howland, John (Osty) Ostapowicz.

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