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GLENN T. SEABORG

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Journal
of
GLENN T. SEABORG

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VOLUME 1

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Glenn, Peter, and Helen Seaborg, 836 Washington Street, Albany, California

September 15, 1946; picture taken by Herman Robinson

Preface

I have kept a journal since I was a boy, most of the time recording the events in an organized manner each day. For the rest of the time the events were recorded in a non-systematic manner, with the intention that all of the material could be organized at a later date into a systematic daily journal. These volumes, covering the period 1946-1958, from the time of my return to Berkeley from the University of Chicago's Metallurgical Laboratory until my start as Chancellor of the Berkeley campus of the University of California, fall into the latter category.

This portion of my journal is based on my notebook entries; memos covering phone calls, appointments, and meetings; minutes of meetings; my appointment calendars and correspondence files; the Radiation Laboratory Chemistry Division personnel files and travel vouchers; laboratory notebooks of my scientific colleagues and cyclotron bombardment logs; some catalogs and materials from the Bancroft Library and the University Archives; back issues of the campus newspaper the Daily Californian and clippings from S. F. Bay Area newspapers found in my scrapbook, etc. Helen was able to provide me with some of her appointment calendars, which helped clarify family and social activities. Many of these resources provided clear and detailed material. Other notes were made hastily and casually, using initials for people's names and rather cryptic abbreviations; however, when these were deciphered, they provided surprisingly complete information.

This portion of my journal consists of about a dozen volumes, starting with Volume 1 (May 19, 1946-December 31, 1947).

I am greatly indebted to Margie Hollander for her invaluable help during the eight-year period when this journal was assembled into publishable form.

Introduction

This portion of my journal covers the 12-year period during which I served as Director of the Division of Nuclear Chemistry of the Radiation Laboratory (now the Lawrence Berkeley Laboratory). The Division was created by Radiation Laboratory Director Ernest O. Lawrence upon my return to Berkeley from Chicago in the spring of 1946. The initial members were composed mostly of people I brought with me from Chicago (Albert Ghiorso, Stanley Thompson, Herman Robinson, Edgar Westrum, Walter Blaedel, Jerry Howland, and a number of graduate students), with a few who had worked at Berkeley during the wartime Manhattan Project days (Charles Prescott, Jr., Fred Reynolds, and Eugene Huffman). Of the initial members, Blaedel, Howland, Prescott, and Westrum did not remain as permanent staff. Amos Newton also joined us, on assignment from Eastman Kodak Company. The initial core leadership of the Division consisted of Associate Professor Isadore Perlman (Professor, 1949), Associate Director of the Division, and Assistant Professor Burris B. Cunningham (Professor, 1953), leader of the Division's inorganic chemistry research program, both of whom came with me from Chicago.

This was a period of great research activity, during which, I believe it can be said, this research group was the premier nuclear chemistry research group in the country and in the world. Many scientific papers were published, including over 100 of which I was author or co-author. The work centered around graduate student research. A total of 156 students earned their degrees during this period; I had 45 students obtain their Ph.D. degree, including a few who were in the process at the end of this period. [The following list includes name of student, date of entry into UC and date of degree, name of research director, and title of thesis.]

Albridge, Royal Gaines, Jr. (Fall 1955-June 1960), Perlman, "Nuclear Spectroscopic Studies in the Heaviest Element Region. II. An Accelerating Electron Spectrograph."

Altman, Lawrence Lothar (Fall 1956-term. July 1966), Perlman, [passed prelims, no thesis].

Appelman, Evan Hugh (Fall 1955-June 1960), Perlman (Hyde), "Chemical Properties of Astatine."

Asaro, Francesco (Frank) (June 1950-1953), Perlman, "The Complex Alpha Spectra of the Heavy Elements."

Asprey, Larned Brown (Autumn 1946-1949), Cunningham, "Equilibria in the Oxide Systems of Praseodymium and Americium."

Axe, John Donald (Fall 1955-September 1960), Cunningham, "The Electronic Structure of Octahedrally Coordinated Protactinium(IV)."

Bailey, Sylvia Mae (Fall 1953-June 1959), Perlman, "Independent Yields of Isomeric Pairs in Nuclear Reactions."

Barr, Donald Westwood (Autumn 1954-September 1957), Seaborg, "Nuclear Reactions of Copper Induced by 5.7-Bev Protons."

Barton, George Wendell, Jr. (1946-June 1950), Perlman, "An X-Ray Spectrometer for Use in Radioactivity Measurements."

Batzel, Roger Elwood (Fall 1948-June 1951), Seaborg, "Fission of Medium-Weight Elements."

Benioff, Paul Anthony (Fall 1951-September 1959), Perlman, "Nuclear Reactions of Low-Z Elements with 5.7-Bev Protons: Nuclear Structure and Simple Nuclear Reactions."

Biller, William Frederick (Autumn 1949-January 1953), Perlman, "Characteristics of Bismuth Fission Induced by 340-Mev Protons."

Blann, Herbert Marshall (Spring 1957-September 1960), Seaborg, "Fission of Gold with 112-Mev C^{12} Ions: A Yield-Mass and Charge-Distribution Study."

Brink, Gilbert Oscar (Autumn 1953-June 1957), Cunningham, "Nuclear Spins of Thallium-197, Thallium-198, Thallium-199 and Thallium-204."

Broido, Abraham (Autumn 1948-June 1950), Cunningham, "The Vapor Phase Hydrolysis of the Trichlorides of La, Pr, Sm, and Am."

Brooks, Daniel Phillip (Fall 1947-June 1949), Templeton, [M.S., thesis never submitted--called to active duty].

Browne, Charles Idol, Jr. (Summer 1950-June 1952), Perlman, "Precision Measurement of X-Rays and Gamma Rays in Radioactive Decay."

Bryan, William Phelan (Autumn 1953-January 1958), Cunningham, "The Reactions of Mixed Crystals of Rare Earth Fluorides with Fluorine Gas."

Carnahan, Chalon Lucius (Autumn 1955-January 1958), Seaborg, "Nuclear Reactions of Uranium Induced by 5.7-Bev Protons. Radiochemical Yields of Light Elements" [M.S.].

Carniglia, Stephen Charles (Autumn 1951-January 1954), Cunningham, "The Vapor Pressures of AmF_3 and Americium Metal."

Carr, Robert Joseph (Autumn 1951-September 1956), Seaborg, "Spallation-Fission Competition in the Nuclear Reactions of Plutonium Induced by Alpha Particles."

Carter, Giles Frederick (Autumn 1949-January 1953), Templeton, "Crystal Structures of Sodium Superoxide and Yttrium Trichloride."

Castner, Stanley Vernon (Spring 1948-January 1951), Templeton, "Some Neutron Deficient Strontium Isotopes" [M.S.].

Chapman, Captain Kenneth Richard (Autumn 1953-June 1954), Seaborg, no thesis [M.S.]

Chasman, Richard Roy (Fall 1955-September 1958), Rasmussen, "Theoretical Studies of the Alpha Decay of Deformed Nuclei."

Chetham-Strode, Alfred, Jr. (Autumn 1953-January 1957), Seaborg, "Light Isotopes of Berkelium and Californium."

Chubbuck, Lt. Colonel James Burney (Summer 1946-June 1948), Perlman, "Artificial Radioactive Isotopes of Cerium and Lanthanum" [M.S.].

Clark, Edward Shannon, Jr. (Autumn 1951-January 1956), Templeton, "The Crystal Structure of Gold (III) Chloride."

Coleman, Joseph Arthur (September 1956-June 1958), Seaborg, "Several Spallation Reactions of U^{238} plus He^{4}" [M.S.].

Crane, William Ward Turner (Autumn 1948-June 1951), Perlman, "Some Physical and Chemical Properties of Curium."

Crespo, Vitor Pereira (Spring 1958-January 1962), Perlman/Hyde, "Ejection of Large Fragments in High-Energy Nuclear Reactions."

Diamond, Richard Martin (Autumn 1948-September 1951), Seaborg, "Some Properties of the Actinides. An Ion Exchange Study of Hybridized 5f Bonding in the Actinides."

Dodge, Richard Patrick (Autumn 1954-June 1958), Templeton, "The Crystal Structure of Vanadyl Bisacetylacetonate."

Donovan, Paul Francis (September 1955-September 1958), Seaborg, "Nuclear Reaction Mechanisms in the Heavy Element Region."

Douthett, Major Elwood Moser (Summer 1948-June 1951), Templeton, "Ranges of Fragments from High Energy Fission of Uranium."

Dunlavey, Dean Carl (Autumn 1949-September 1952), Seaborg, "Investigations of Alpha Radioactivity Using Nuclear Emulsions."

Eads, Donald Leroy (September 1956-January 1959), Seaborg then Perlman, "Spallation Reactions of Plutonium-240 with Helium Ions and Plutonium-242 with Deuterons" [M.S.].

Eyring, LeRoy (Autumn 1943/military service-June 1949), Cunningham, "Thermochemical Studies of Oxides of Praseodymium and Americium, and the Calculation of the Pr^{+++}-Pr^{++++}, Am^{+++}-Am^{++++} Oxidation Potentials."

Faler, Kenneth Turner (Fall 1956-June 1959), Rasmussen, "Nuclear Decay Scheme Studies of Some Tantalum and Terbium Isotopes."

Feay, Darrell Charles (Autumn 1950-June 1954), Cunningham, "Some Chemical Properties of Curium."

Felber, Frank Frederick (Spring 1955-January 1957), Rasmussen, "Nuclear Decay Schemes of Some of the Isotopes of Tantalum" [M.S.].

Fink, Richard Walter (Autumn 1948-July 1949), Templeton, "Properties of Some Neutron Deficient Cesium Isotopes" [M.S.].

Fischer, Vera Kistiakowsky (Autumn 1948-January 1952), Seaborg, "A Study of the Isotopes of Promethium."

Flamm, Eileen Joy (Fall 1957-September 1960), Perlman, "Perturbation of Alpha-Gamma Angular Correlations in Transuranium Isotopes."

Fleming, Edward Homer, Jr. (Autumn 1949-June 1952), Cunningham, "The Specific Alpha Activities and Half-lives of U^{234}, U^{235}, and U^{236}."

Florence, Lt. Mitchell Garth (Summer 1953-January 1955), Perlman, no thesis [M.S.].

Folger, Robert Lancaster (Autumn 1948-June 1951), Seaborg, "High Energy Proton Fission-Spallation of Uranium."

Foreman, Bruce Milburn (Fall 1953-June 1958), Seaborg, "Spallation and Fission in Th^{232} and the Masses of the Heaviest Elements."

Fritsch, Arnold Rudolph (Autumn 1953-January 1957), Perlman, "Energy Levels of Neutron Deficient Lead Isotopes."

Fung, Si-Chang (Autumn 1949-January 1952), Perlman, "Nuclear Excitation and Recoil by High Energy Particles."

Futrell, Jean (September 1955-September 1958), Templeton, "The Radiation Chemistry of the Symmetrical Dichloroethylene."

Gallagher, Charles Joseph, Jr. (Autumn 1954-January 1958), Rasmussen, "Electron-Spectroscopic Studies of Neutron-Deficient Rhenium Isotopes."

Gibson, Lt. Walter Maxwell (February 1954-June 1957), Seaborg, "Fission and Spallation Competition from the Intermediate Nuclei Americium-241 and Neptunium-235."

Gilmore, John (Fall 1956-September 1960), Perlman, "The Effect of Angular Momentum on Fission Probability."

Glass, Richard Alois (Autumn 1950-June 1954), Seaborg, "Studies in the Nuclear Chemistry of Pu, Am, and Cm, and the Masses of the Heaviest Elements."

Glenn, William Ellis, Jr. (Fall 1947-January 1952), L. C. Marshall, "Time of Flight Mass Spectrograph" (Ph.D. in Engineering).

Goeckermann, Robert Herman (Autumn 1946-January 1949), Perlman, "Characteristics of Bismuth Fission with High Energy Particles."

Gonzalez-Vidal, Jose (Autumn 1955-September 1958), Seaborg, "Survey of Tritium-Producing Nuclear Reactions."

Gordon, Glen Everett (Fall 1956-September 1960), Seaborg, "Fission and Spallation in Nuclear Reactions Induced by Heavy Ions."

Gray, Peter Rygaard (Fall 1952-January 1956), Seaborg, "Electron Capture and the Auger Effect in the Heaviest Elements."

Grover, James Robb (Autumn 1952-January 1958), Seaborg, "The Reactions of Tantalum with 5.7-Bev Protons."

Gruber, John Balsbaugh (Fall 1957-June 1961), Cunningham, "An Analysis of the Absorption Spectra of Tm(IV) and Am(VI)."

Gunn, Stuart Richard (Autumn 1950-June 1954), Cunningham, "Thermodynamics of Aqueous Ions of Americium."

Hall, Kenneth Lynn (Autumn 1949-July 1951), Templeton, "Counting Efficiency of Bismuth 205" [M.S.].

Hardgrove, George Lind (Autumn 1956-September 1959), Templeton, "The Crystal Structure of Several Cis-1,2 Dihalobenzocyclobetenes."

Hicks, Harry Gross (Autumn 1946-June 1949), Seaborg, "New Neutron-Deficient Radioactive Isotopes of the Rare Earth Region."

Higgins, Gary Hoyt (Autumn 1949-June 1952), Seaborg, "An Investigation of the Isotopes of Americium and Curium."

Hill, Max Wilmer (Fall 1955-January 1959), Perlman, "Nuclear Decay Studies of Protactinium Isotopes."

Hoff, Richard William (Autumn 1950-January 1954), Seaborg, "Orbital Electron Capture in the Heaviest Elements."

Hollander, Jack Marvin (Autumn 1948-September 1951), Perlman, "Nuclear Transformations Using Accelerated Carbon Ions."

Hopkins, Horace Herbert, Jr. (Autumn 1946-June 1949), Cunningham, "Spallation Products of Arsenic with 190 Mev Deuterons."

Hulet, Ervin Kenneth (Fall 1949-September 1953), Seaborg, "An Investigation of the Isotopes of Berkelium and Californium."

Hummel, John Philip (Autumn 1953-September 1956), Perlman, "Alpha Decay Studies in the Heavy Element Region."

Jaffe, Harold (Autumn 1951-June 1954), Perlman, "Electron Capture Studies in Shielded Nuclei."

James, Ralph Arthur (Fall 1946-June 1948), Seaborg, "Isotopes of the New Element Curium (Atomic Number 96)."

Johnson, Quintin Calvin (Fall 1957-January 1961), Templeton, "Some Problems in Crystallography."

Jones, Merle Eugene (Autumn 1949-September 1951), Cunningham, "Vapor Pressure of Americium Trifluoride."

Jones, Robert Edward, Jr. (Autumn 1953-January 1957), Templeton, "The Crystal Structure of Acetic Acid."

Juliano, Jose Ochoa (Summer 1954-June 1957), Perlman, "Coincidence Nuclear Spectrometry with Applications to Europium-154 and Europium-155."

Karraker, David George (Autumn 1947-June 1950), Templeton, "Isotopes of Rubidium, Polonium, and Bismuth."

Koch, Charles William (Autumn 1951-February 1954), Cunningham/Latimer, "Thermodynamics of the Trichlorides and Oxychlorides of the Lanthanide and Actinide Elements."

Kofstad, Per Kristen (Autumn 1950-September 1953), Seaborg, "Spallation and Fission of Silver."

Kyi, Ru-tao (Fall 1956-June 1960), Cunningham, "Paramagnetic Resonance of Tetravalent Protactinium."

La Chapelle, Theodore James (Autumn 1946-June 1948), Seaborg, "Neptunium (V) Chlorides and Oxychlorides" [M.S.].

La Salle (Shoaf), Mary Joan (Autumn 1953-January 1955), Seaborg, "The Chemistry and Thermodynamics of Vanadium (V)" [M.S.].

Lessler, Richard Marshall (Fall 1952/in military/-January 1959), Seaborg, "Spallation-Fission Competition in Neptunium Compound Systems. Decay Scheme Studies."

Levine, Charles Arthur (Spring 1948-January 1951), Seaborg, "A Study of Naturally Occurring Plutonium."

Levy, Harris Benjamin (Autumn 1950-September 1953), Perlman, "I. Isomeric States of Bismuth 210. II. Relative Yields in the Formation of Nuclear Isomers."

Lindner, Manfred (Spring 1946-September 1948), Perlman, "Nuclear Reactions in Antimony with High Energy Particles."

Lohr, Harold Russell (Autumn 1947-June 1950), Cunningham, "Heats of Formation of Some Aqueous Ions of Americium."

Lokken, Stanley Jerome (Autumn 1953-September 1954), Seaborg, no thesis, [M.S.].

Lovejoy [Westerdahl], Carolyn Ann (September 1957-September 1961), Rasmussen, "Nuclear Orientation of Some Rare Earth Isotopes".

Luoma, Ernie Victor (Autumn 1954-January 1957), Seaborg, "Deuteron-Induced Spallation and Fission Reactions in Plutonium Isotopes" [M.S.].

McDonell, William Robert (Autumn 1948-September 1951), Perlman, "Chemical Products of the Irradiation of Aliphatic Alcohols with High-Energy Nuclear Particles."

McLaughlin, Ralph Dexter (Autumn 1951-January 1954), Cunningham, "The Absorption Spectrum of PuF_3" [M.S.].

McWhan, Denis Bayman (Fall 1957-September 1961), Cunningham, "Crystal Structure and Physical Properties of Americium Metal."

Magnusson, Lawrence Bersell (Autumn 1946-June 1949), Seaborg, "Isotopes of Neptunium."

Marquez, Luis (Autumn 1947-June 1950), Perlman, "Abnormal Charge Increase in Nuclear Reaction."

Marshalek, Eugene Richard (September 1957-June 1962), Rasmussen, "Theory of Collective Vibrations of Even-Even Spheroidal Nuclei."

Marshall, Thomas Vincent (Fall 1955-September 1960), Rasmussen, I. "Beta-Spectroscopic Studies in the Promethium Region. II. The Coriolis Interaction in Deformed Nuclei."

Mathur, Hirdaya Behari (Spring 1952-January 1955), Seaborg, "Radiochemical and Spectrometer Studies of Some New Nuclear Isomers Prepared by Cyclotron Bombardment."

Meinke, William Wayne (Autumn 1947-January 1950), Seaborg, "High Energy Bombardment Products of Thorium."

Michel, Maynard Cornelius (Autumn 1950-September 1953), Templeton, "Separation and Assignment of Radioactive Isotopes."

Miller, Daniel Robert (Autumn 1946-September 1948), Seaborg, "High Energy Spallation Products of Copper."

Mollenauer, James Frederick (Fall 1957-September 1961), Rasmussen, "Effects of Angular Momentum on Gamma Ray Production in Compound Nucleus Reactions."

Momyer, Floyd Franklin (Spring 1950-January 1953), Perlman, "Studies of Neutron-Deficient Isotopes of Emanation, Francium, and Radium."

Morgan, Leon Owen (Early 1946-September 1947), Seaborg, "Isotopes of the New Element Americium (Atomic Number 95)."

Nervik, Walter Edward (Summer 1951-June 1954), Seaborg, "Tantalum Spallation and Fission Induced by 340-Mev Protons."

Nethaway, David Robert (September 1955-January 1957), Seaborg, "Excitation Functions for Reactions of Bev Protons on Indium" [M.S.]

Neumann, Henry Matthew (Autumn 1947-June 1950), Perlman, "Radioactive Isotopes of Bismuth."

O'Connor, Paul Radell (Spring 1946-September 1947), Seaborg, "The Chemical Identification of Isotopes Formed by the Bombardment of Uranium with High Energy Particles."

O'Kelley, Grover Davis (Autumn 1948-June 1951), Seaborg, "The Spectrometric Determination of Some Beta Particle and Conversion Electron Energies."

Orth, Donald Alfred (Autumn 1947-January 1951), Seaborg, "Isotopes of Neptunium and Plutonium."

Parsons, Lt. Russell Kenneth (Summer 1953-September 1954), Perlman, no thesis, [M.S.].

Passell, Thomas Oliver (Autumn 1951-June 1954), Perlman, "Internal Conversion of Gamma Radiation in the L Sub-Shells."

Pilger, Richard Christian (Autumn 1954-September 1957), Perlman, "Nuclear Decay Schemes in the Actinium Family."

Prohaska, Charles Anton (Autumn 1948-September 1951), Perlman, "Heavy Element Decay Schemes with Alpha-Gamma and Alpha-Electron Coincidental Counting."

Raby, Bruce Alan (Autumn 1952/Army-January 1954), worked with Hyde, degree in general chemistry [M.S.].

Rasmussen, John Oscar (Spring 1949-January 1952), Seaborg, "Alpha Radioactivity of Nuclides with Atomic Numbers Less than 83."

Reddoch, Allan Harvey (Fall 1954-June 1960), Cunningham, "Nuclear Quadrupole Resonance of Some Inorganic Chlorine Compounds."

Rhodes, (Coxworth) Ann (Autumn 1955-September 1957), Rasmussen, "Decay Studies of Some Neutron Deficient Isotopes of Antimony and Tellurium" [M.S.].

Ruiz, Carl Phillip (Autumn 1956-June 1961), Perlman, "Alpha Decay Studies in the Families of the Light Uranium Isotopes."

Schooley, James Frederick (Fall 1953/Air Force-January 1961), Rasmussen, "Some Low-Temperature Nuclear-Orientation Studies." [M.S. Jan 1955; Ph.D. Jan 1961]

Senko, Michael Edward (Autumn 1953-January 1957), Templeton, "Crystal Structure of a Triazole and Choline Chloride."

Sharma, Hari Dutta (Autumn 1948-September 1951), Seaborg, "Investigations of Some Unusual Nuclear Reactions and Study of Double Beta Decay."

Shirley, Virginia (Schultz) (Autumn 1955-January 1957), Rasmussen, "Decay Scheme Studies of Some Light Gadolinium Isotopes" [M.S.].

Shudde, Rex Hawkins (Spring 1952-September 1956), Seaborg, "Fission of Uranium with 5.7-Bev Protons."

Shuey, Richard Lyman (Autumn 1945-September 1950), L. W. Reukema (Electricial Engineering), "Instrumentation for Energy Determination of High Energy Particles."

Silva, Robert Joseph (Fall 1954-June 1959), Seaborg/Harvey, "Mechanisms of the (α,pn) Reaction."

Slater, Louis Maurice (Autumn 1949-June 1954), Seaborg, "High Energy (d,p) Reactions."

Smith, Warren G. (Autumn 1952-June 1955), Rasmussen, "I. Neutron-Deficient Isotopes in the Noble Metal Region, II. Conversion Electron Spectra of Some Heavy Elements."

Stephens, Frank Samuel (Autumn 1952-June 1955), Perlman, "Decay Schemes and Nuclear Spectroscopic States in the Heavy Element Region."

Stewart, Donald Charles (Autumn 1946-June 1950), Cunningham/Kirk, "Growth of Chick Fibroblasts in Vitro with Special Reference to the Role of Embryo in the Liquid Media."

Stoner, Allan Wilbur (Autumn 1953-September 1956), Perlman, "Nuclear Properties of Some Neutron-deficient Isotopes of Emanation, Polonium, and Astatine."

Stover, Betsy Jones (Autumn 1947-June 1950), Cunningham, "New Neutron Deficient Radioactive Isotopes of Rare Earths and Osmium."

Street, Kenneth, Jr. (Spring 1946-June 1949), Seaborg, "Isotopes of Americium and Curium."

Strieter, Frederick John (Fall 1956-January 1960), Templeton, "The Crystal Structures of Several Organic Compounds."

Strominger, Donald (Fall 1953-September 1956), Rasmussen, "Experimental Study of Nuclear Isomers in the Millimicrosecond Lifetime Range, II. Applications of Nilsson's Wave Functions for Deformed Nuclei."

Surls, Joseph Pleas, Jr. (Autumn 1951-January 1956), Seaborg, "Ion-Exchange Behavior of Actinides and Lanthanides."

Sweeney, Michael Patrick (Autumn 1953/Air Force-January 1955), Cunningham, "Radiation Chemistry of Isopropyl Compounds" [M.S.]. [later Ph.D.]

Templeton, David Henry (Early 1946-June 1947), Perlman, "Artificial Radioactive Isotopes of Polonium, Bismuth, and Lead."

Thomas, Thomas Darrah (Autumn 1954-September 1957), Seaborg, "Spallation-Fission Competition from the Compound System U^{233} Plus He^4."

Thompson, Stanley Gerald (Autumn 1946-September 1948), Seaborg, "Nuclear and Chemical Properties of Americium and Curium."

Toth, Kenneth Stephen (September 1954-June 1958), Rasmussen, "Nuclear Studies in the Rare Earth Region."

Unik, John Peter (Fall 1956-June 1960), Rasmussen, "Coincidence Measurements in Nuclear Decay Scheme Studies."

Valyosick, Ernest William (Fall 1956-September 1959), Perlman, "Range and Range Straggling of Heavy Recoil Nuclei" [M.S.].

Vandenbosch, Robert (Autumn 1954-September 1957), Seaborg, "Fission and Spallation Competition in Ra^{226}, Th^{230}, U^{235}, and Np^{237}."

Vandenbosch, Susanne Elaine (Ritsema) (Autumn 1954-January 1956), Seaborg, "Fission and Spallation Excitation Functions of Uranium" [M.S.].

Viola, Victor Emanuel, Jr. (Autumn 1957-June 1961), Seaborg, "Angular Distribution from Heavy Ion Induced Fission."

Wallmann, James Caswell (Summer 1947-June 1951), Cunningham, "The Specific Activity and Half-life of Various Isotopes of Plutonium."

Werner, Louis Bernard (Autumn 1940/Early 1946-September 1948), Perlman, "Isolation and Properties of Curium."

Werning, Joseph Robert (September 1955-January 1959), Templeton, "Thermal Ionization at Hot Metal Surfaces."

Wolfe, Col. Richard Duncan (Summer 1946-June 1948), Seaborg, "Radioactivities Produced in the Platinum Group by Bombardment of U with 400 Mev He Ions" [M.S.].

Worthington, John Trelfa (Autumn 1953-January 1955), Perlman, no thesis [M.S.].

Worthington, Captain William Jacob, Jr. (Summer 1950-January 1952), Seaborg, "High Energy Spallation Products of Zinc" [M.S.].

Young, Gifford Alan (Spring 1948-June 1949), Templeton, no thesis (M.S.].

Zalkin, Allan (Autumn 1948-June 1951), Templeton, "Crystal Structure of Lanthanide and Actinide Borides and Fluorides."

Some of the other graduate students associated with our group during this period included Howard W. Anderson (1956-1958, Chem E), Robert S. Brown (1954-1959, Chem E), Richard P. Burns (1954-1955), Amado Y. Cabezas (1957-1961), William B. Carter (1957-1960, Chem E), Lung-wen (Linda) Chiao (1957-1961, Rasmussen), Yung-Yee Chu (1956-1960, Templeton), Henry Cheung (1953-1958, Chem E), Denny L. Condotta (1952-1953, Chem E), Harry L. Conley (1957-1960), Marshall W. Cook (1950-1954, Chem E), Alberto Cortes (1956-1962 Helmholz/Harvey), John E. Cotter (1957-1959, Chem E, EE), Milton W. Davis (1946-1951, Chem E), Walter Dong (1952-1956, Chem E), John H. Duffin (1954-1959, Chem E), Robert J. Fallat (1954-1959, Chem E, Medicine), John D. Faust (1958-1963, Physics), Eugene J. Fenech (1953-1960, Chem E), J. Leonard Fick (1949-1954, Chem E), Stanley D. Furrow (1956-1957), Hugh Garvin (Physics, 1957), Elwood H. Gift (1954-1955, Chem E), Paul W. Gilles (1946-1947), James R. Griffith (1954), John B. Gruber (1957-1961, Cunningham), James N. Haag (1957-1962, Templeton), Rodney E. Harrington (1956), William S. Harris (1953-1958, Chem E), Russell G. Herron (Autumn 1954-June 1955, M.S. [non-UC degree, research done at UCRL with F. Reynolds]), Carol H. Hewitt (1953-1954), Thomas E. Hicks (1946-1949, Chem E), Owen G. Holmes (1950-1955, with McClure), Robert H. Houston (1954-1958, Chem E), Jens L. Hov (1953-1959, Chem E), Luc

Huang (1954-1955, Chem E), Charles D. Hunt (1947-1955, Chem E), Gabriel L. Jacques (1954-1958, Chem E), John Jost (1949-1950, Chem E), Wilbur V. Johnson (1953-1954), Herbert R. Johnston (1953-1955, EE), James S. Kane (1952-1955), William J. Knox (Physics, 1946-1947), L. C. Lavering (1951), Victor L. Ledesma (1957-1963, Chem E), Hugh R. Lehman (1948-1952, Chem E), Nian-Tze (Norman) Li (1957-1960, Chem E), David Z. Lippmann (1948-1953), Robert E. Lundin (1950-1955), Edward J. Lynch (1949-1953, Chem E), John L. Maier (fall 1953), Arturo Maimoni (1950-1956, Chem E), Richard Marrus (1957-1959, Physics), Robert E. Meredith (1956-1959, Chem E), Eugene I. Motte (1952-1954, Chem E), Robert F. Nickerson (1953-1958), Ronald Odum (1954), Chem E), Alfred W. Petersen (1952-1954, Chem E), John E. Powers (1951-1954, Chem E), Homer E. Rea, Jr. (1949-1953, Chem E), Albert J. Rothman (1950-1954, Chem E), Barney Rubin (1947-1951, Chem E), Russell H. Sanborn (1952-1955), Sidney D. Skirvin (1951-1952), Harry E. Spencer (1950-1954/Connick), Peter O. Strom, Jr. (1955-1956), Carol C. Sweeney (1957-1958), Archie B. Treadwell (1954-1955), John H. Vanderveen (1954-1961, Chem E), William R. Wilcox (1956-1960, Chem E), Joseph Winocur (1955-1960, Chem E), Peter Yankwich (1948), Thomas J. Ypsilantis (Physics, 1949-1950), Rene D. Zentner (1954-/law).

Three of our graduate students showed enough promise to be appointed as members of the Department of Chemistry faculty--David Templeton in 1947, Kenneth Street, Jr., in 1949, John O. Rasmussen, Jr., in 1952,-- and for temporary appointments, Jack M. Hollander (1951-1953), James A. Cobble (1953), Richard A. Glass (1954), T. Darrah Thomas (1957-1959). They helped administer the Division, along with Earl Hyde, who joined us in 1949 from Argonne National laboratory. Other UC Chemistry/Chemical Engineering faculty with whom we often collaborated included Leo Brewer, LeRoy Bromley, Robert E. Connick, Donald N. Hanson, Chester T. O'Konski, Wendell M. Latimer, Bruce McGarvey, Edwin F. Orlemann, Charles W. Tobias, Theodore Vermeulen, Charles Wilke and Physics Department faculty William A. Nierenberg and Carson D. Jeffries.

In addition to students and staff, we frequently had participating guests, including postdoctoral appointments, for varying periods of time. These included, for example: John M. Alexander (1957-1958), Saadia Amiel (1956-1957), Nathan E. Ballou (1947-1948), Christiane Baltzinger (1957-1958), Ingmar Bergström (September-October 1953), Norman A. Bonner (summer 1948), Fred L. Canavan (1955-1956), Albert A. Caretto, Jr. (1956-57), Andre Chesne (1957-1958), Gregory F. Choppin (1953-1956, summer 1957), Maung Cho Cho (June-July 1957), T. C. Chu, James W. Cobble (1952-1954), Alexander Cosmatos (1958), Ugo Croatto (1951-1952), Lloyd Currie (1957), Richard M. Diamond (summer 1957), Harold W. Dodgen (summer 1949, summer 1950) Peter Fong (summer 1956), Kenneth W. Ford (December 1957), Wilhelm Forsling (1955), Sherman Fried (July 1952), Gerhart Friedlander (September 1952), Bernard Fries (1947), Jean Fuger (1956-1957), Tor Ragnar Gerholm (June 1958), Lawrence Glendenin (1952), Richard C. Hoff (1958), Peter Graf (1955-1956), Dieter Gruen (1955-1956), Barun C. Haldar (1951), Rolfe H. Herber (1958), Thomas Hicks (1950), Peggy Hoffman (Lehigh University) (summer 1956), Lennart W. Holm (1956), Hans Ihle (1957), Ralph James (summer 1949), William A. Jenkins (1950-52), Marvin Kalkstein (1951-1956), Berta Karlik (May 1956), Joe Katz (July 1947, October 1956, other occasions), Richard W. King (June 1958), Truman Kohman (summer 1949), Russell A. Kurtz (du Pont, summer 1952), Inge-Maria Ladenbauer (1957-1958), Walter J. Laird (du Pont, summmer 1952), Lamberto Malatesta (1950), J. D. McCullough (1947, summer 1948 and 1949), Hugh McManus (summer 1957), Lars Melander (1949), John Mihelich (1955), J. Malcolm Miller (summer

1949), Ben Mottelson (September 1956), M. Luis Muga (1957-1960), Helmut Munzel (May 1958), Keiji Naito (1958-1959), Robert Naumann (August 1952), John Newton (1956-1958), Sven-Gösta Nilsson (1956-1957), Ivar Olovsson (1957-1958), William C. Orr (1948-1949), Brian D. Pate (May 1958), Rupert Patzelt (1958), Arthur M. Poskanzer (1958), Mangipudi V. Ramaniah (spring 1953), Lewis E. J. Roberts (March 1955), Mario Rollier (1951-1952), Maurice E. Rose (September 1956), Frank S. Rowland (summer 1957), Gösta Rudstam (1950), Jan Rydberg (spring 1951), Jagdish Shankar, Torbjörn Sikkeland 1956-1957), Frederic C. Schmidt (1954-1955), Mark D. Snyder (du Pont, 1952), Ellis Steinberg (1952), Peter Cooper Stevenson (1949-1951), Nathan Sugarman (May 1958), Sigvard Thulin (1955), Edward R. Tompkins (1950-1951), William W. True (summer 1957), Anthony Turkevich (summer 1956), Helge Tyrén (1951-1952), William H. Wade (1955-1958), John R. Walton (1957-), A. H. Wapstra (March-April 1957), Fritz Weigel (1956-1957), Edwin O. Wiig (August 1948), Geoffrey Wilkinson (1947-1951), Lester Winsberg (1955-), Karl-Erik Zimen (June 1948).

Occasionally undergraduate research students (and even high school students) worked with some of our staff, sometimes for only the summer: Richard Borg (1953), Dwight Conway (1952), Glenn M. Cook (1957), David D. Cudaback (summer 1948, summer 1949), John W. Eastman (1957), Bruce M. Foreman (1953-1954), Warren Heiman (1948), Paul Lasky (1956), Robert A. Naumann (1948-1949), Neil L. Nininger, (summer 1956, summer 1957), Ann Pitzer (summer, 1957), John A. Reed (1957). Also, during some summers a few high school teachers worked with various staff people.

Throughout this period of time I presided (or Perlman in my absence) over weekly group meetings to review progress in our research program. Starting sometimes at 8 a.m., but more often at 8:30 a.m., and lasting up to 10 a.m., they were held on Thursdays (on Tuesdays the first year), first in a conference room on the hill in a temporary building (quonset hut), then on the campus in the Old Chemistry Building in a room above the stock room, finally in the conference room (Room 191) of Building 70. There was no planned agenda for these meetings, at which the graduate students were called on to make progress reports on and discuss plans for their research; staff members also made reports on the same unstructured basis.

We had weekly bag lunch planning meetings at Monday noon, first in my office in Building 5 and then in my office in Building 70 for the senior staff. (My office always had a huge complete chart of isotopes and a periodic table on the wall and was equipped with a blackboard.) Perlman presided in my absence and sometimes another senior staff member when we both were absent. The attendees served as a sort of an informal Executive Committee for our Division, and included other faculty members, Burris Cunningham, and when they assumed this status, David Templeton, Kenneth Street, and John Rasmussen, and Earl Hyde, in his administrative role--and other senior staff members such as Stanley Thompson, Albert Ghiorso, Bernard Harvey, Herman Robinson, Fred Reynolds, Jack Hollander, Frank Asaro, Maynard Michel, Frank Stephens, Darrah Thomas, John Conway, and Administrative Assistant Gertrude Steel. Additional senior staff members included Walter J. Blaedel (1946-1947), Thomas E. Hicks (1949-1952), (Eugene Huffman (1946-), John C. Hubbs (1955-1959), Jerome Howland (1946-1949), Charles Prescott, Jr. (1946-1948), Wladyslaw Swiatecki (1957-), James C. Wallmann (1953-), Edgar Westrum (1946-1947; summers 1948, 1949).

During almost all of this period my secretary was Doral Buchholz,

following the early, short tenure of Mary (Millard) Bender. Some of the other staff, support, and office personnel at this time included: Ursula Abed (1957-), Dorothy Abrams, A. E. Bazell, Bernadine Bertink, L. J. Beaufait, Richard L. Boegner (1953), Harry R. Bowman (1951-), A. Bratenahl, Edith T. Bryan, Margot Carlson (-1955), Eileen Carson (1955-), La Rae B. Chatelain (1948), Homer Conzett, Charles A. Corum (1957-), Elwin H. Covey (1946-1949), Lucille Cox, Shirley Daney, Carol (Hewitt) Dauben, Mildred Davis, Herbert Di Grazia, Roger Dorr, Eileen Doyle, Lucy C. Edwards, Forrest Fairbrother, Jr. (1950), Ward Ferris, Antoine Frank, M. Fran Gallagher, Roberta Garrett, Raymond Gatti (1956-), Lilly Y. Goda, Ellen J. Grahlman (1946-1947), Lorraine Hanna, Al Hartzell, Virginia C. Hempel, Winifred Heppler (1946-), Margie (Schnarr) Hollander (1948-1955), Pat Howard (1956-), Glen I. Iddings, Alice Israel, Edward Jeung (1956), Frances Jewell, Hildred Jensen, Stephen Kahn, Louise Kalm, George W. Kilian, Charles W. Koch, Almon E. Larsh (1950-), Robert Latimer (June 1958-), Eugene Lee (-1957), Jeanne (Wheelock) Lilly, Robert C. Lilly, Pat (Maguire) McLaughlin, Jeannette Mahoney, Karl Marhenke (1957-1958), Donald F. Martin, Ralph McLaughlin, Docia McKennon, Helen (Vaughn) Michel (1956-), Mary Misak, Bobby Ann Mohler, Milton Moore, Duane F. Mosier, James B. Niday (1952-1953), Carol V. Oakes (195-1958), Robert F. Osborne, Robert L. Oswalt, Tom Parsons, G. Donald Paxon, Lorraine Petch, Llad Phillips (1956), Terry Pionteki, Elinor Potter (1952-), Jean Rees (1956-), Carolann Rossi (1957-), Helena Ruben (1950-), Jean Samson, Aldo F. Sciamanna, George V. Shalimoff (1953-), Donald Simkin, Dorothy (Bockhop) Stewart, Doris (Heisig) Terwilliger, Yoshiko Uchida, Helene Voyer (1947), Jane Waite, Lawrence A. Williams, Jane Wulf, Mary Wyld, Al Wydler, Gifford Young, M. Charles Zeitz, et al.

We had helpful collaboration from the men operating the accelerators: Joseph Hamilton, W. Bart Jones, Bernard Rossi, Tom Putnam at the 60-inch cyclotron; Jimmy Vale, Lloyd Hauser, at the 184-inch cyclotron; Ed (Edward J.) Lofgren, assisted by Bill (William A.) Wenzel, Bruce B. Cork, and Walter Hartzell, at the Bevatron; Ed Hubbard, Edward G. Hartwig, Robert M. Main, et al at the Hilac.

Essential for assurance of the safe operation of our research program was the support work and radiation monitoring of the Health Chemistry Group under the capable leadership of Nels Garden (and his assistant Rosemary Barrett); the group included such people as Dale Alloway, Aldo Azzalini, Bill Bennett, Herman J. Bradley, Howard Browne, Herb Cantelow, Max Chapman, Leonard Deckard, Jim Haley, John Gifford, Red Gordon, E. Kenneth Hulet, Ruth Mary Larimer, Francis McCarthy, O. L. (Dusty) Meadors, Elmer Nielsen, Ray O'Dea, John Peck, Will Phillips, William Ruehle, Al Salo, Mike Thaxter, Manley Wu, Jenson Young.

At the beginning our entire group was housed in Building 4, the two-story chemistry building that was built and used during World War II on the Radiation Laboratory project for development of the electromagnetic method for the production of enriched uranium-235. We were very cramped for space but received some relief the following year (1948) when the neighboring Building 5 was constructed, with financial help from the Rockefeller Foundation. Although this was primarily a "hot lab," Perlman and I had our offices there; this also served as the headquarters for Ghiorso, Cunningham, and Thompson. As our requirements for space increased a number of our graduate students were accommodated, beginning in 1949, on the top floor of Building 50, the physics building. We all came together in our new chemistry building, Building 70, in 1955. As we neared the end of this period, in 1958, we obtained approval for

construction of an adjoining chemistry building (Building 70A) at a cost of $2,000,000.

The 60-inch cyclotron, (furnishing 20 Mev deuterons and 40 Mev helium ions) in Crocker Laboratory on the campus, served as our mainstay for irradiations of our targets during the entire period. The new 184-inch cyclotron (furnishing 340 Mev protons, 180 Mev deuterons, and 360 Mev helium ions) became available in 1947 and the bevatron (6 Bev protons) in 1953. The heavy ion linear accelerator (HILAC), furnishing heavy ions up to neon with an energy of 10 Mev per nucleon and operated as a part of our Division, became available in 1957. By 1958 we had received approval for the construction on the hill of an 88-inch spiral ridge cyclotron (60 Mev deuterons, 120 Mev helium ions, and 60 Mev protons) at a cost of $5 million for operation by our Division.

The interval 1946-1958 was a period when six new transuranium elements were synthesized and identified, i.e., discovered, by this research group (together with colleagues from other laboratories in some instances) --berkelium (atomic number 97) in 1949, californium (98) in 1950, einsteinium (99) in 1952, fermium (100) in 1953, mendelevium (101) in 1955, and nobelium (102) in 1958--an average of one every other year. In the 1950's, a program of production by intensive neutron bombardment in high flux reactors of weighable amounts of elements up to fermium (100) was carried out which also led to the determination of the (n,γ) and (n,fission) cross sections of numerous isotopes of these elements.

Curium (96) was the first to be isolated in weighable amount in 1947, berkelium (97) and californium (98), in 1958. During this decade the chemical properties of berkelium, californium, einsteinium, fermium and mendelevium, as well as astatine, were defined by using the tracer technique; macroscopic quantities of neptunium, plutonium, americium and curium were used for the production and determination of the properties, including crystal structures (via x-ray diffraction), of numerous compounds and metallic forms of these elements. Such work was also done with many of the rare earth elements. The involvement of 5f electrons in bond hydridization in the actinide elements was demonstrated in the early 1950s. The efficiency of separation of these elements by the ion exchange adsorption-elution method took a huge leap forward when ammonium alpha-hydroxy-isobutyrate was discovered in 1956 for use as an eluant.

The bombardment in 1947 of a wide range of elements with 180 Mev deuterons and 360 Mev helium ions furnished by the newly operating 184-inch synchrocyclotron led to the first observation of nuclear reactions which we termed as "spallation" reactions. Also in 1947 such bombardment led to the observation of the nuclear fission reaction in elements well below the thorium-uranium region, ranging from tantalum (73) to bismuth (83); it was shown that such high energy fission has a tendency to be symmetrical. The fission of medium weight elements such as copper with 60-70 Mev protons was demonstrated in 1950. Bombardment of thorium with 80-150 Mev deuterons and 100-200 Mev helium ions in 1948 led to the observation of extensive radioactive decay chains collateral to the thorium (4n), neptunium (4n+1), uranium (4n+2), and actinium (4n+3) families.

During 1948 and 1949 great advances were made in the alpha-decay systematics of the heavy elements--both in the relation between alpha particle

energy and mass number and atomic number and the relation between half-life and energy--which led to continued forefront advances in this field during the next decade. Data on alpha radioactivity accumulated in our laboratory had a strong influence on the development of the single, collective, and unified particle nuclear models. In 1949 alpha emitters in a neutron deficient rare earth region were first observed as a result of the bombardment of rare earths with 200 Mev protons in the 184-inch cyclotron. By 1951 substantial progress had been made in understanding the systematics of spontaneous fission, and by 1954, the nuclear thermodynamics of the heaviest elements had been well-formulated. Evidence for an important nuclear subshell at N=152 was also recognized in 1954. Double coulomb excitation was discovered at the Hilac in 1958 using oxygen ions on tungsten.

About 25 radioactive isotopes of transplutonium elements and more than 100 isotopes throughout the rest of the elements were discovered during this period with the 60-inch cyclotron, along with a similar large number with the 184-inch cyclotron. Nuclear spin assignments, by molecular beam methods, were made on about 45 radioactive isotopes produced by the 60-inch cyclotron.

The mechanisms of nuclear reactions induced over a range of energies by protons, deuterons, and helium ions with targets throughout the periodic table were investigated. A systematic study of nuclear fission-spallation competition in the heaviest elements using deuterons and helium ions in the 60-inch cyclotron was carried out beginning in the middle 1950's. The yields of nuclides formed as spallation and fission products by bombardments with 340-Mev protons in the 184-inch cyclotron and 6-Bev protons in the bevatron were determined; such a program using heavy ions furnished by the HILAC was begun in 1957.

During this entire period of time there was a substantial program in the field of radiation chemistry using deuterons and helium ions at the 60-inch cyclotron.

Tables of Isotopes that became the standard reference source throughout the world were published in 1948, 1953, and 1958.

In order to pursue our nuclear chemical research at the increased level that our results seemed to warrant, new facilities would be needed, both at the national and the local level. I wrote to AEC Chairman Lewis Strauss on October 24, 1957, about the need for a "very high flux reactor" and a two-fold program to (1) irradiate Pu^{239} in a high flux production-type reactor to produce Cm^{244} and (2) irradiate the curium in the "very high flux reactor" to produce berkelium, californium and einsteinium in substantial quantities. This led to the national Transplutonium Production Program. I testified in February 1958 at a hearing before the Joint Committee on Atomic Energy (JCAE) on the need for support of basic research and this led, through the help of California Congressman and JCAE member Craig Hosmer, to the authorization and funding for our new 88-inch cyclotron and our new chemistry building (Building 70A).

In 1950 Ernest Lawrence launched a new project at the site of the wartime air base in Livermore for which he brought in the California Research Corportation (a subsidiary of the Standard Oil Company of California) as the operating contractor with the support of the Atomic Energy Commission. A number of people from our Nuclear Chemistry Division accepted employment in

this effort. The objective of the project was to build a large linear accelerator, with the camouflaged name of Materials Testing Reactor (MTA), for the acceleration of protons to bombard a target to yield copious quantities of neutrons. These neutrons were to irradiate uranium for the production of plutonium more efficiently than could be done with nuclear reactors. The plutonium would be chemically separated from the uranium and fission products. This rate of production was to be used to alleviate the perceived shortage of available uranium. However, it was soon apparent that the supply of uranium would be adequate to fuel the needed number of plutonium production nuclear reactors and thus this project was terminated.

However, in 1952, a second nuclear weapons laboratory (a perceived needed competitor to the Los Alamos Laboratory) was established at this Livermore site upon the urging of Ernest Lawrence and Edward Teller. At the beginning we in the Nuclear Chemistry Division assumed the responsibility for the chemical work in this new laboratory. Many of our graduating Ph.D. chemists accepted employment there and other members of our Division and of the College of Chemistry on campus worked to insure the success of the new venture. At my suggestion Kenneth Street was soon placed in charge of the chemical program. At times the Livermore chemists attended the Thursday morning research meetings of our Chemistry Division. Gradually, the Livermore chemistry effort assumed a position of independence.

I round out this summary by including some description of my personal life and some activities outside of the laboratory.

In May 1946 my wife Helen and I returned to California by train from Chicago, where I spent the war years and a little more, working at the University of Chicago's Metallurgical Laboratory. (The immediately preceding four years, 1942-1946, are covered in the preceding four volumes of my journal.) We occupied a small rented house on Washington Avenue on the west side of the hill in Albany, where we were joined by my sister Jeanette who had offered to provide Helen with help in the care of our soon-due child. The "child" arrived very prematurely at the end of May (May 31) as twins, Paulette Jeanne and Peter Glenn, of which only Peter Glenn survived.

About a year later we bought a relatively large, two-story house on Ellsworth Street in south Berkeley, which was immediately occupied by the four of us. Our daughter Lynne Annette arrived within a couple of months, on September 6 of 1947, and soon thereafter (October) Jeanette returned to Southern California where she was married on December 6, 1947 (and another marriage on June 3, 1957). Our second son, David Michael, was born on April 22, 1949; in this case I performed the obstetrical services for Helen, who was unable to proceed beyond the front steps of our house before the birth took place. Also in 1949 we purchased a lot in Lafayette, and arranged with a contractor to build a one story, U-shaped, redwood house to our specifications (limited in size by our finances), which we occupied in July 1951. Our third son, Stephen Keith, was born the following month (August 14). With our financial situation augmented by my Nobel Prize, the following spring we added bedrooms to our Lafayette house. It was built largely during a two-month stay (May-July) of our family in Chicago, where I served as a visiting scientist at the Argonne National Laboratory.

Our fourth son, John Eric (who was named after his great grandfather and uses the name Eric), was born on November 17, 1954. (I narrowly escaped

having also to deliver Eric at his birth.) We purchased the two empty lots adjoining our property on the south, which became our "field" upon which we laid out a baseball field and built a tennis court, for use by our kids and their friends. Soon thereafter, we joined with our neighbors to the north, the Isadore Perlmans and Albert Alexanders, to build, in the Perlmans' back yard, a swimming pool for joint use by our three families.

Helen and I hosted each year a cocktail party, first at our Berkeley home and backyard and then at our Lafayette home and patio. This was attended by members (and their spouses and some children) of our Nuclear Chemistry Division, a few other people from other groups of the Rad Lab, some members of the campus College of Chemistry and other campus departments, and the Livermore Lab. The number of attendees grew, and the total reached nearly 300 toward the end of the 1950s.

Near the end of 1946, President Harry Truman appointed me as a member of the nine-person General Advisory Committee (GAC) of the newly established and appointed Atomic Energy Commission (AEC). The initial members of the GAC were an awesome group--J. Robert Oppenheimer (who served as Chairman), Enrico Fermi, James B. Conant, Isidor I. Rabi, Lee A. Du Bridge, Cyril S. Smith, and industrialists Hood Worthington and Hartley Rowe. With such a membership the GAC exerted a tremendous influence on the initial Commissioners of the AEC--David E. Lilienthal (Chairman), Lewis L. Strauss, Robert F. Bacher, Sumner T. Pike and William W. Waymack. The first meeting of the GAC was held in Washington on January 3, 1947, and we met on the average of every other month until the end of my term, August 1, 1950. We advised the AEC, in a very influential manner, on the rehabilitation of the Los Alamos Weapons Laboratory (which had become somewhat disorganized after the end of the war), the operation of the AEC facilities for the production of fissionable material, the diminishing role of secrecy in the operation of the AEC, the distribution of radioactive isotopes produced in the AEC facilities, the instigation of the AEC's marvelous program of support of basic research in U.S. universities and colleges, the operation of the national laboratories, the direction of the emerging civilian nuclear power program, the AEC organizational structure, and many other areas where we thought our advice, sought or unsought, would be helpful.

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

I made five visits to Europe during this period of time. The first of these, which was also my first-ever visit to Europe, occurred in October and November 1949. I accepted an invitation from the Swedish Royal Academy of Sciences to visit Sweden to give a series of lectures on the transuranium elements in Stockholm, Göteborg, Lund, and Uppsala. On this occasion it was a great thrill for me to meet, for the first time, a number of my Swedish relatives, on both my mother's and my father's side, in such places as Stockholm, Västerås and Kopparberg. I was particularly interested to meet my

mother's brother, Karl Adolfsson, and his family in Kopparberg, and he took me to visit my mother's childhood home in nearby Grängesberg, which she left at the age of 17 in 1904 to emigrate to Ishpeming, Michigan. My father was born in Ishpeming of parents born in Sweden.

On the way home from Sweden I visited England to make a tour of a number of their nuclear laboratories and production facilities at the request of the AEC, which wanted my evaluation of their competence in order to decide whether it would be worthwhile for the United States to initiate a cooperative program with them; I gave a favorable report to the AEC after my return home.

In December 1951, my wife Helen accompanied me on a trip to Stockholm, where I was awarded the Nobel Prize in Chemistry, together with Edwin M. McMillan for our investigations on the chemistry of the transuranium elements. Visiting Stockholm at the same time were Edwin and Elsie McMillan, and, also, Ernest and Molly Lawrence, in order that Ernest might give his Nobel Address, which he was prevented from doing when he won the Nobel Prize in Physics in 1939 during World War II. We three couples were an especially close group as Molly and Elsie are sisters and Helen served as Ernest's secretary from 1938 until 1942. The memorable events included the afternoon Nobel Ceremony in the Concert Hall on December 10th, the gala Nobel Banquet that evening in the Town Hall, the banquet, the following evening in the Royal Castle with the King and Queen, our three Nobel Lectures on December 12th, and the numerous receptions and dinners preceding the Nobel Ceremony. Also during this visit, preceding the Nobel Ceremony, the Lawrences and Seaborgs travelled by train to nearby Uppsala to see The Svedberg and his colleagues and to participate in the dedication of his newly completed cyclotron. Also during this visit I had the pleasure of crowning the Lucia Queen of Stockholm at the traditional Lucia ceremony in the Town Hall on December 13th. On the way home Helen and I stopped in Paris where I visited Madame Irene Joliot-Curie at the Institute of Radium, Marie and Pierre Curie's old laboratories, and Frederic Joliot at the nearby College de France.

In September 1955, I attended the first International Conference on the Peaceful Uses of Atomic Energy, where I gave one of the Plenary Lectures. Here I was excited to meet for the first time and learn of the experimental results of a number of Soviet nuclear chemists and nuclear physicists, to meet Otto Hahn, the co-discoverer of nuclear fission, and to meet and talk to many other famous nuclear scientists. Ernest Lawrence became acquainted with many of these scientists at evening dinners, and one result of this was a visit of Otto Hahn, at our invitation, to the United States and Berkeley later that fall. Al Ghiorso, Stan Thompson, and I took an automobile tour of Switzerland during the middle weekend of the Conference.

My fourth visit was to England in June 1956, to give the Centenary Lectures at the invitation of The Chemical Society in Liverpool and in London on the subject of the transuranium elements. It was a thrill to give the London lecture in Burlington House, in the same room where Michael Faraday gave his famous series of lectures. Also during this visit I visited the Harwell Research Establishment, England's outstanding nuclear research laboratory.

My last visit to Europe during the period covered by this portion of my journal occurred in July 1957, when I accepted an invitation to attend and speak at the XVIth International Congress of Pure and Applied Chemistry and

the XIXth Conference of the International Union of Pure and Applied Chemistry in Paris. In order to take her mind off my father's death, which had occurred the previous February, I persuaded my 70-year-old mother to accompany me on a visit to Sweden, which I could conveniently visit before and after my visit to Paris. Soon after our arrival in Stockholm, I visited the Nobel Institute for Physics, which coincided with the news conference, in which I participated, at which the Swedish-British-American international team announced the discovery of the element with atomic number 102. (This was later shown to be wrong by our research team at the Radiation Laboratory in Berkeley.) We visited my mother's relatives in Stockholm, Kopparberg (her brother Karl Adolfsson) and the Dalarna district (where she was born, in Grängesberg), and also a cousin on the Seaborg side, Carl Tersmeden. I left my mother to continue her visits while I flew to Paris, where I gave my talk, "Recent Research on the Actinide Elements," attended a number of ceremonial functions and dinners, visited the Saclay Laboratory of the French Atomic Energy Commission, and visited a number of my friends including my wartime French colleague, Bertrand Goldschmidt. I flew back to Stockholm, joined my mother, visited some more with my relatives, and returned with her to Stockholm in order to fly home.

During this period I gave hundreds of talks at meetings of scientific societies such as the American Chemical Society and the American Physical Society, colleges, universities, etc. I was chosen to give a number of name lectures or series of lectures: the William Conger Morgan Memorial Lecture on the Transuranium Elements at UCLA in June 1946; the annual Harrison Howe Memorial Lecture of the Rochester Section of the American Chemical Society ("Plutonium and Other Transuranium Elements") in November 1946; a "Frontiers in Chemistry" lecture at Western Reserve University ("The Transuranium Elements") in March 1947; the Twentieth Annual Faraday Lecture in Pasadena ("New Elements and How to Make Them") in April 1947; the Nieuwland Lectures at Notre Dame University ("The Transuranium Elements") in November 1947; the annual Sigma Xi Lecture at the AAAS meeting in Chicago ("The Eight New Synthetic Elements") in December 1947; the Foster Lectures at Buffalo University (five lectures on nuclear chemistry and our Berkeley research program) in April 1951; the Third Annual Phi Lambda Upsilon Lectures at Ohio State University (two lectures on the "Transuranium Elements") in March 1952; the William Pyle Philips Lecture at Haverford College ("The Transuranium Elements") in October 1953; the Sigma Xi Lecture at Berkeley ("The Present Status of the Transuranium Elements") in November 1953; the third annual Gilbert N. Lewis Memorial Lecture at Berkeley ("The Future Synthetic Elements") in October, 1956; the ninth annual E. C. Franklin Memorial Lecture at the University of Kansas ("Recent Research on the Transuranium Elements") in April 1957; the Silliman Lectures at Yale University (four lectures on "The Transuranium Elements") in April-May 1957; the first Joseph W. Kennedy Memorial Lecture at Washington University, St. Louis, ("The Future Synthetic Elements") in April 1958.

I made appearances on numerous radio and television programs. Perhaps the most notable of these is the series of ten half-hour films on the chemical elements made by San Francisco educational television station KQED for the national Educational Television and Radio Center. This series was filmed in 1956 in the "Cave Room" of the Radiation Laboratory, an underground section of our nuclear chemistry building (Building 70). This series was shown nationwide on educational television stations beginning in 1957. The producer was Evans G. (Red) Valens; the chief writer was Daniel M. Wilkes; Radiation Laboratory chemist Bernard G. Harvey appeared with me as a co-host throughout,

and guest appearances were made by Melvin Calvin (explaining organic chemistry), Ernest O. Lawrence (his only filmed appearance showing him explaining his invention and the operation of the cyclotron), Emilio Segrè (explaining his discovery in 1937 of the first synthetic element technetium, atomic number 43), Edwin M. McMillan (explaining his discovery in 1940 of the first transuranium element neptunium, atomic number 93), Albert Ghiorso and Stanley G. Thompson (recreating their discovery in 1955 of the element mendelevium, atomic number 101), and Otto Struve (describing the creation of the chemical elements in cosmic processes involving nuclear reactions).

I served as author, co-author or co-editor of a number of books: in 1953, Volume One of Comprehensive Inorganic Chemistry with co-authors W. N. Lipscomb and P. R. O'Connor (one of my earlier graduate students), the first of an intended series edited by M. Cannon Sneed, J. Lewis Maynard and Robert C. Brasted; in 1949, the two-volume, The Transuranium Elements: Research Papers, with co-editors Joseph J. Katz and Winston M. Manning, Volume 14B of the Plutonium Project Record (PPR) of the National Nuclear Energy Series (NNES); in 1954, The Actinide Elements, with co-editor Joseph J. Katz, Volume 14A (a survey volume) of the PPR of the NNES; in 1957, The Chemistry of the Actinide Elements, with co-author Katz; in 1958, The Transuranium Elements, based on my four Silliman Lectures at Yale University in the spring of 1957; and also in 1958, Elements of the Universe, with co-author Evans G. (Red) Valens, based on my ten half-hour educational television programs on the chemical elements filmed in 1956 and first broadcast nationwide on educational television stations in 1957.

At the end of 1952 and the beginning of 1953, I embarked on a new adventure, a result of my devotion to athletics. I had followed closely the triumph of the U.C. Berkeley baseball team in the College World Series in 1947; watched with admiration Coach Lynn "Pappy" Waldorf's successful football teams in 1948, 1949, 1950 (three successive Rose Bowl teams) and attended the Rose Bowl games in Pasadena on January 2, 1950 and January 1, 1951. (I had missed the Rose Bowl game on January 1, 1949 because it was thought inadvisable for Helen to travel at that time due to her pregnancy.) Because he had learned that I was a regular attendee at U.C. Berkeley intercollegiate athletic contests, newly appointed Berkeley Chancellor Clark Kerr in the fall of 1952 asked me to serve as the Berkeley Faculty Athletic Representative. (Stanley Freeborn was leaving this post to become Chief Administrative Officer of the Davis Campus.) I accepted the offer, attended the next meeting of the Pacific Coast Intercollegiate Athletic Conference (PCIAC or simply PCC) with Freeborn in Pasadena in December 1952, and assumed my official duties the next month. My duties as Faculty Athletic Representative included supervising the academic aspects of the participation of student athletes in major and minor intercollegiate sports (ruling on student athletic eligibility, compliance with entrance requirements, conformance with the PCC Athletic Code, etc.), attendance as the Berkeley representative at the semiannual and special meetings of the PCC, attendance at the annual meetings of the National Collegiate Athletic Association (NCAA), and, fortunately, attendance at intercollegiate athletic contests. This brought Helen and me into a new social circle, which we enjoyed very much. It also brought me into close contact with Chancellor Kerr, which led, I believe, to my appointment as Chancellor in the summer of 1958 when he assumed the presidency of the University of California (my two and one-half years as Chancellor are covered in the next three volumes of my journal).

In 1956 I was appointed press spokesman for the PCC, which meant that I had the responsibility for conducting the press conferences following the sessions at the PCC meetings. These press conferences soon became exciting events (facing the most newspaper and radio reporters and TV cameramen that I had ever faced) when the cheating scandals erupted at several member institutions. Disclosure of illegal financial aid to athletes led to penalties and loss of eligibility of football players at USC, UCLA and the University of Washington and lesser penalties, for less serious violations, at U.C. Berkeley. This led to the withdrawal of USC, UCLA and Berkeley from the PCC in 1957, and the University of Washington, in 1958. I then played a leading role in putting together in 1958 a new athletic association, the Athletic Association of Western Universities (AAWU), consisting of USC, UCLA, Berkeley and the University of Washington, soon to be joined by Stanford University.

Throughout this period I received a number of honors: named one of "America's Ten Outstanding Young Men of 1946" by the U.S. Chamber of Commerce in 1947, the American Chemical Society Award in Pure Chemistry in September 1947, the John Ericsson Medal of the American Society of Swedish Engineers in February 1948, the Nichols Medal of the New York Section of the American Chemical Society in March 1948, the "Alumnus of the Year" Award from the University of California at Berkeley in 1948, my first honorary doctor's degree (D.Sc.) from the University of Denver in 1951, the Nobel Prize in Chemistry in December 1951, Honorary Membership in the American Institute of Chemists in September 1952, the Dickson Achievement Award, also "Alumnus of the Year," from the UCLA Alumni Association in 1953, the John Scott Award and Medal of the City of Philadelphia in 1953, honorary doctor's degree (D.Sc.) from Gustavus Adolphus College in May 1954, honorary doctor's degree (D.Sc.) from Northwestern University in June 1954, the Perkin Medal of the Society of Chemical Industry in January 1957, honorary doctor's degree (LL.D.) at the University of Michigan in June 1958.

I made my first major address on a non-scientific (i.e., sociological) subject on March 23, 1953--the Charter Day Address at the University of California, Riverside, on the subject, "Dawn of the Nuclear Age." I worked hard on the preparation of this address, trying to make my first venture into this arena a success. (I had, however, made a number of talks on atomic power to lay audiences, including participation in President Robert Gordon Sproul's Alumni Tour throughout the state south of the Bay Area in February 1952.) I gave the Commencement Address at my alma mater, David Starr Jordan High School in the Watts district of Los Angeles, on June 19, 1953.

My first college level Commencement Address was given at Gustavus Adolphus College on May 30, 1954. I also spoke to a lay audience upon my visit to my hometown of Ishpeming, Michigan, in July 1954, on the occasion of its Centennial Celebration. I presented the awards to the winners of the first San Francisco Bay Area Science Fair in April 1955, and addressed the National Science Fair participants in Los Angeles in May 1957, on "The Making of a Scientist." One talk I gave that attracted wide attention was "The Role of Basic Research," given to the joint meeting of the Atomic Industrial Forum and the Stanford Research Institute in San Francisco in April of 1955. I made my first address to the Commonwealth Club in San Francisco in August 1957 on "Atomic Energy and You." In 1957 and 1958 I spoke extensively on the problems of pre-college education, especially in the areas of science and mathematics. This included a rather pre-eminent talk on "Education in Our Age: Let's

Define the Problem" at the Conference on Science and Mathematics Education in the Public Schools at Sacramento in February 1958 and my Commencement Address, "Education Today" at San Francisco State College in June 1958. Another talk which attracted a good deal of attention, in April 1958, was entitled "Nuclear Power--Its Scientific Basis, Its Current Status, and Some Conclusions," which I gave at the Asilomar Conference on the International Atom sponsored by the World Affairs Council of Northern California.

These years marked a period of great scientific productivity and exciting landmarks in my personal life.

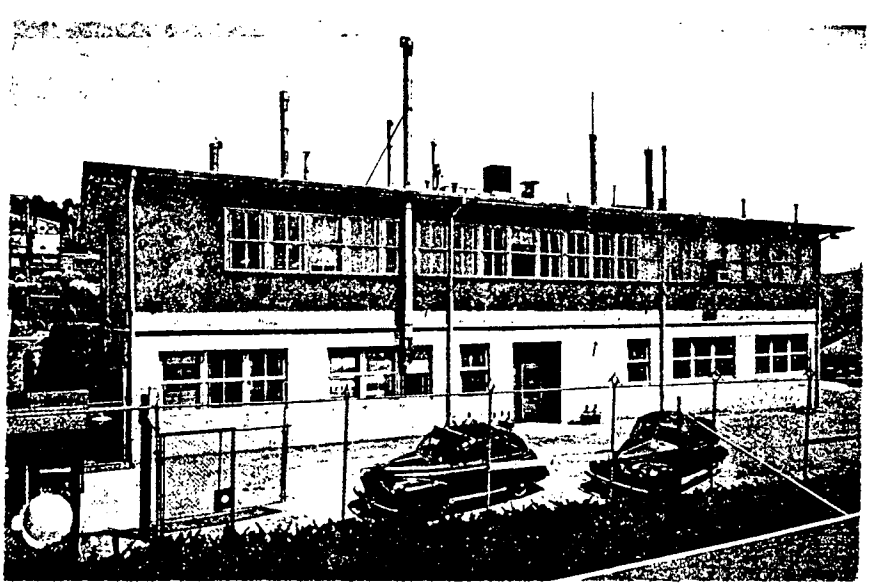
Sunday, May 19, 1946

Helen and I arrived in Berkeley from Chicago this morning after travelling since early Friday evening on the streamliner, "City of San Francisco." (I spent yesterday confined to our roomette with a migraine headache.) Iz Perlman and my sister Jeanette met and drove us to our new home at 836 Washington Street in Albany, which we are renting from Sophia Ryan, a high school English teacher who delves in real estate. Miss Ryan also owns the house at 527 Colusa Avenue, Berkeley, in which the Perlmans are living. Through this connection with Miss Ryan, Iz located the house for us. Although it is rather small--a living room, a kitchen with a breakfast room, one bedroom, a bathroom, and a small sleeping porch--Helen and I decided that it is much better for us than the attic apartment that we had rented from Rebekah L. Young, a secretary in the Department of Physics. Jeanette will be living with us and will help take care of our baby, who is expected in early July.

Jo Owen, a friend of Helen's from college days, and Lee Perlman came by in the afternoon to welcome us. Later we telephoned my parents and Helen's mother in Southern California to let them know that we arrived safely.

Monday, May 20, 1946

I drove to the campus in Jeanette's car (a 1939 Dodge sedan), which she will allow me to use until I can buy a car, a difficult thing to do these days. In Gilman Hall I checked in with Dean Wendell M. Latimer: I am ready to resume my academic position, professorial level, in the Department of Chemistry of the University of California. I greeted others in the College of Chemistry. I then went up the hill to the Radiation Laboratory to see Professor Ernest O. Lawrence (Director) and others. After checking in with the Personnel Department of the Radiation Laboratory (I am being paid $650/month for a forty-one hour week as Executive), I began my position as Head of the newly formed Nuclear Chemistry Division. In Building 4, the wartime Chemistry Building of the Radiation Laboratory, I became better acquainted with Mary E. Millard, who serves as my secretary.



Building 4

Several members of the Chicago group are already working in Berkeley. Isadore Perlman, Louis B. Werner, and Herman P. Robinson started on December 3; Leon O. (Tom) Morgan began on December 4; Paul R. O'Connor on January 2; Jerome J. Howland, Jr. (via Monsanto) on January 28; David H. Templeton on February 25; Edgar F. Westrum, Jr. on March 4; Robert H. Goeckermann and Larned B. Asprey on approximately April 1; Walter J. Blaedel, William J. Knox, Jr., and Lawrence B. Magnusson on April 15; Daniel R. Miller (via Oak Ridge) on approximately April 20; Ralph A. James on approximately May 1; Stanley G. Thompson on May 10; Burris B. Cunningham on May 13; and Theodore J. La Chapelle on May 16. [Albert Ghiorso is expected within a few days.]

Many of these men will be working on their doctorates: James, Morgan, O'Connor, La Chapelle, Thompson, Miller, and Magnusson with me; Goeckermann, Knox, Werner, Templeton, and Manfred Lindner (via Hanford, and already in graduate school with a teaching assistantship) with Perlman (newly appointed as a Research Associate Professor of Chemistry); LeRoy Eyring (already in graduate school) and Asprey with Cunningham (newly appointed as a Research Assistant Professor of Chemistry). Other staff members who did not come with us from Chicago include: Dorothy Bockhop, Norman W. Gregory (who will leave in the fall), Thomas E. Hicks (hired May 7, 1946), Eugene H. Huffman (in charge of an analytical chemistry group), Robert C. Lilly, George E. MacWood (who is leaving soon to go to Ohio State), Mary E. Millard (secretary, hired December 7, 1945), Charles H. Prescott, Jr., and Fred L. Reynolds. Amos S. Newton, who worked at the Ames Laboratory in Iowa during the war, is here on assignment from the Eastman Kodak Company. Robert E. Connick and Leo Brewer, who worked in Berkeley on the Plutonium Project during the war, now hold faculty positions in the Chemistry Department, and have offices and laboratories in Gilman Hall. Although not members of our group, they, along with their graduate students (Le Roy A. Bromley, Paul W. Gilles, Edward L. King, William H. McVey, et al.), will attend our group meetings.

Helen did some shopping. Mrs. Wallace M. Webb, a neighbor across the street, stopped in to welcome her with a bouquet of flowers.

Tuesday, May 21, 1946

This morning I presided at a meeting of the chemistry group, attended by Asprey, Blaedel, Bromley, Connick, Cunningham, Eyring, Gilles, Goeckermann, Howland, Huffman, James, Knox, La Chapelle, Lindner, Magnusson, Miller, Morgan, Newton, O'Connor, Perlman, Reynolds, Robinson, Templeton, S. Thompson, Werner, and Westrum. I talked mainly about the future research program of the group. Those who are graduate students will begin work on their thesis problems. We plan to try to make some pure curium so that we can study its properties, and we will try to develop a separation procedure in order to study the general chemical properties of americium and curium. Attempts will be made to make transcurium elements by the reaction of helium ions on Am^{241}. In addition, we have two samples (2 and 4 mg) of Am^{241} in the Hanford pile for bombardments. Later we plan to attempt bombardments with heavier particles, such as carbon ions. We want to extend the study of the dry chemistry and metallurgy of protactinium and neptunium. Blaedel may study the hydrolysis of plutonium. Westrum and Eyring will work on the

metallurgy of americium, protactinium, and neptunium. Ghiorso will continue his work on slow neutron fissionability. We want someone to study the chemical properties of the rare earths, in particular the oxidation states of such elements as praseodymium, neodymium, and terbium. The new accelerators will enable us to look for new types of reactions and radioactivities; of particular interest will be the possible splitting of lighter elements. We also still have several problems in the heavy region to be solved. Perlman mentioned that another problem is light mass isotopes that would be formed by bombarding Tl^{203} with high energy deuterons and helium ions when these become available in the 184-inch cyclotron. Perlman also talked about changes in the working space--nearly everything not requiring good shielding or good hoods will have to be done in the big laboratory upstairs.

* * * * *

In the mail awaiting me upon my arrival in Berkeley yesterday, I found a notice from the Graduate Division stating that I, along with F. A. Jenkins, R. B. Brode, L. W. Alvarez, and E. D. Kane, form the committee in charge of Edward Joseph Lofgren's research, dissertation, and final examination. This notice was dated May 3, 1946. I also had a letter from M. W. Welch of the W. M. Welch Manufacturing Company, which confirmed our arrangements for the publication of a chart of isotopes. Stewart S. Kurtz, Jr., wrote that he is sorry that I will be unable to stay for the Gibson Island dinner but he is happy that I can come at all. Other mail was received from Donald A. McPherson of John Wiley & Sons who, along with Brad Wiley, has discussed the possibility of my writing a book for them to publish; and from Alexander Silverman of the University of Pittsburgh, who asked for information on new elements that might be included on a periodic table. Robert Campbell of Life magazine sent me a copy of the layout and text of the article, "Plutonium Laboratory," scheduled for publication in Life; this is a story of the work of our group at the Metallurgical Laboratory and includes photographs taken there.

The Research Conference Meeting, which was held on Tuesday afternoon in prewar days, is now held at 7:30 p.m. on Tuesday evenings in Room 102, Gilman Hall. Tonight's meeting, conducted by Dean Latimer, had the same format as before--a student reporting on a paper in the literature and a staff member reporting on his own research--but lacked the flavor and spice of the old prewar meetings presided over by Professor Lewis.

Helen had tea at Glatha (Mrs. Wendell M.) Latimer's home at 810 Euclid Avenue.

Wednesday, May 22, 1946

I made the rounds of the laboratories in Building 4 to bring myself up to date on the progress of the research of the various men.

I read a copy of the following telegram sent out today by Albert Einstein to prominent Americans who have supported progressive causes:

OUR WORLD FACES CRISIS AS YET UNPERCEIVED BY THOSE POSSESSING POWER TO MAKE GREAT DECISIONS FOR GOOD OR EVIL. THE UNLEASHED POWER OF THE

ATOM HAS CHANGED EVERYTHING SAVE OUR MODES OF THINKING AND WE THUS DRIFT TOWARD UNPARALLELED CATASTROPHE. WE SCIENTISTS WHO RELEASED THIS IMMENSE POWER HAVE OVERWHELMING RESPONSIBILITY IN THIS WORLD LIFE AND DEATH STRUGGLE TO HARNESS ATOM FOR BENEFIT OF MANKIND AND NOT FOR HUMANITY'S DESTRUCTION. BETHE, CONDON, SZILARD, UREY AND FEDERATION OF AMERICAN SCIENTISTS JOIN ME IN THIS APPEAL. WE BEG YOU TO SUPPORT OUR EFFORTS TO BRING REALIZATION TO AMERICA THAT MANKIND'S DESTINY IS BEING DECIDED TODAY NOW THIS MOMENT. WE NEED TWO HUNDRED THOUSAND DOLLARS AT ONCE FOR NATIONWIDE CAMPAIGN TO LET THE PEOPLE KNOW THAT NEW TYPE OF THINKING ESSENTIAL IF MANKIND IS TO SURVIVE AND MOVE TOWARD HIGHER LEVELS. THIS APPEAL SENT YOU ONLY AFTER LONG CONSIDERATION IMMENSE CRISIS WE FACE. URGENTLY REQUEST YOU SEND IMMEDIATE CHECK TO ME AS CHAIRMAN, EMERGENCY COMMITTEE OF ATOMIC SCIENTISTS, PRINCETON, NEW JERSEY. WE ASK YOU HELP IN THIS FATEFUL MOMENT AS SIGN THAT WE SCIENTISTS ARE NOT ALONE.

Helen went shopping in downtown Oakland; her friend Jo Owen visited with her in the afternoon.

Thursday, May 23, 1946

I stopped in First Aid (near 184-inch cyclotron) for lab tests (blood count and urinalysis), preparatory to my re-entry physical examination.

After talking with the fellows in the various laboratories about their research, I accepted the May 14 invitation from Ernest Grunwald, President of Alpha Kappa Chapter of Phi Lambda Upsilon at UCLA, to deliver the Morgan Memorial Lecture on June 5. In my letter to him I included a draft copy of "The Heavy Elements" and suggested other material that he might use for publicity.

Helen and I had dinner at the home of Leonard and Dagmar Dreher, who live at 140 San Carlos St. in El Cerrito. [Leonard was a fellow chemistry major at UCLA and worked in my group at the Met Lab; I also knew Dagmar (Lundgren) at UCLA.]

Friday, May 24, 1946

A report is being issued in Chicago today (addressed to Farrington Daniels) by A. Ghiorso, W. M. Manning, and G. T. Seaborg. It describes the slow neutron fissionability of heavy nuclei of the odd-odd type. The neutron source was the thermal column of the Argonne heavy water pile. Protactinium-230, prepared in the Berkeley 60-inch cyclotron, was found to undergo fission with slow neutrons with a cross section of about 18,000 barns. Incomplete experiments have yielded preliminary values of 700 barns for the fission cross section of Pa^{232} and 1000-500 barns for Np^{238}, also prepared in the 60-inch cyclotron.

I returned to Robert Campbell (Life magazine) the material that he recently sent me, along with comments and corrections, and with the suggestions of Stan Thompson, Burris Cunningham, and others. I added the comment of Dean Latimer and Daniel M. Wilkes, newswriter from the University of California Office of Public Information, who suggested that recognition should be made that many of the men mentioned in the article are now members of the University of California plutonium laboratory.

I had my re-entry physical examination today, given me by the Rad Lab physician, Anton H. Schaefer, which I passed--my lab tests were ok; however, I was found to weigh only 132 pounds (considerably underweight), a consequence, I suppose, of my hectic schedule over the past several years at the Metallurgical Laboratory. My blood pressure was also quite low--90/62.

Helen spent part of the afternoon with Genevieve (Gen) Calvin; then she and I, along with the Francis Jenkinses, had dinner at the Calvin home, at 1431 LeRoy Ave., with Melvin and Gen. [Francis Jenkins, whom Helen and I knew before the war, is a professor in the Department of Physics; I have known Melvin since 1937, and Helen and I have known Gen since the Calvins were married in 1942.]

Saturday, May 25, 1946

I spent the morning in my Radiation Laboratory office and laboratories.

John Lewellen of the "Quiz Kids" wrote, in a letter dated May 23, about how pleased everyone is with our "atomic" phonograph record. Lewellen suggests, if I am willing, that I make another (a second) record when I visit Chicago in June. Lewellen mentioned that we had a couple of boners on the first record.

A telegram arrived from W. A. Higinbotham (Chairman, Federation of American Scientists), reading:

> MOST IMPORTANT PHASE OF OUR WORK IS NOW POLITICAL EDUCATION CARRIED ON PRIMARILY THROUGH THE NATIONAL COMMITTEE ON ATOMIC INFORMATION. WOULD APPRECIATE YOUR ASSISTANCE TO RAISE FUNDS FOR THIS VITAL ACTIVITY IN RESPONSE TO TELEGRAM FROM EINSTEIN.

I spent the afternoon at home with Helen and Jeanette.

Sunday, May 26, 1946

After a casual Sunday morning, Helen, Jeanette, and I took a drive to Pleasanton.

Monday, May 27, 1946

Much of my day was taken up with reading and answering my mail, by dictating to Mary Millard. I received a May 18 letter, forwarded from Chicago, from Inez L. Magill, thanking me for information about John H. Lawrence's work in leukemia. John W. Tietz (from De Witt Clinton High School, Bronx) thanked me for the reprint I sent him. Nell A. Parkinson of Chemical and Engineering News wrote, in a letter dated May 21, that she is sending me a copy of the State Department Board of Consultants' "Report on the International Control of Atomic Energy" for review. To Professor Alexander Silverman who asked for information for use in a periodic table, I explained that the information on elements 43, 61, 85, and 87 of the type to be used in a periodic table is lacking at the present time--I did give him the half-lives of the longest-lived isotopes.

John Herman Ross of St. Louis, Missouri, in a letter dated May 19, asked some thoughtful questions about the properties of neptunium, plutonium, americium, and curium. Although I replied that all that the military has permitted to be said has appeared in C & E News and the Smyth Report, I enclosed a reprint of my article "The Chemical and Radioactive Properties of the Heavy Elements," from the December 10, 1945 issue of C & E News.

I also answered a May 8 letter from Dr. Adrian Ziegler of New York, saying that I believe it will be possible to prepare more elements than the 96 now known. I included the names and symbols for the transuranium elements.

I sent Professor Frank A. Long (a friend from graduate school days) at Cornell University in Ithaca, New York, the names of Dr. Raymond Stoughton and Dr. Norman R. Davidson as possible radiochemists for a position he has available at Cornell. Finally, I wrote to Ruth Rogers in Chicago (my last secretary at the Met Lab), asking her for a copy of the letter of recommendation I wrote for Dr. N. R. Davidson, as well as asking about the transfer of the laboratory notebooks of our people who have moved to Berkeley from Chicago.

This afternoon Helen had an appointment with Dr. Herbert Traut, an obstetrician at U.C. Hospital in San Francisco. Dr. Traut was highly recommended by Dr. E. M. Davis, Helen's obstetrician at the University of Chicago Lying-In Hospital. We took his suggestion despite the inconvenient distance to Traut's office.

Esther Wells, a cousin of my mother, and her son Lawrence Risem visited us during the evening.

I presided at the group meeting beginning at 8:58 a.m. In attendance were Blaedel, Connick, Cunningham, Eyring, Goeckermann, Howland, James, Knox, La Chapelle, Lindner, Magnusson, Miller, Newton, Perlman, Robinson, Templeton, S. Thompson, Werner, and Westrum. I made some general announcements, including the fact the Joe Hamilton would like to know how much longer to bombard the U^{238} target (now has 300 μah) and what other bombardments we want for the next six months. Suggestions were: Am^{241} plus helium ions; plutonium plus deuterons to settle the situation regarding Am^{240} and Am^{239}; U^{233} plus deuterons to prepare Np^{234} for fission measurements; protactinium plus deuterons to confirm the formation of U^{229} by a d,4n reaction; and plutonium plus helium ions to prepare Cm^{240} and Cm^{241} to test their fissionability. Reports today included Templeton's description of Lofgren's results on the proton bombardment of enriched Pb^{208}; Werner announced that, as a result of his recent experiments, he has concluded that TTA shows very little promise as a reagent for the separation of 95 and 96; Knox reported cross sections for some products produced from U^{235} plus helium ion bombardment; and Blaedel described his method of analysis of pulse analyzer curves.

* * * * *

My review of Henry D. Smyth's Atomic Energy for Military Purposes was mailed to Nell A. Parkinson of C & E News. I said in part,

As a description of the physics of the atomic bomb development, this report must be rated as an excellent, even brilliant, account. However, if it was intended as a complete account of all the scientific development in this enterprise, it must be recorded that it falls short of this objective and, in particular, the description of the role which chemistry played is far from adequate.

I answered John Lewellen's letter of May 23, saying that I am willing to try another recording session when I am in Chicago. I mentioned that my only free time will be the afternoon on June 14.

Also, I received a thank-you note, dated May 25, from L. Reed Brantley (a UCLA friend) of Occidental College in Los Angeles, thanking me for the material I sent for a talk he will make on atomic energy.

Don Cooksey, Associate Director of the Radiation Laboratory, requested, at my urging, in a memorandum to the Office of the Area Engineer, that I be maintained on the personnel list of the Metallurgical Laboratory to receive, here in Berkeley, the same reports that I received in Chicago.

Al Ghiorso has arrived in Berkeley and was placed on our payroll today.

Helen's friend, Jo Owen, spent the evening with her while I attended the Chemistry Department's weekly Research Conference meeting.

Wednesday, May 29, 1946

After talking with some of the fellows, I replied to a May 21 letter from Edwin O. Wiig at the University of Rochester, saying that Monday, November 18, 1946, is satisfactory as a date for my Harrison Howe Lecture. To Donald A. McPherson of John Wiley and Sons, I suggested that he contact me at the Windermere East Hotel in Chicago between June 13 and June 22. I also dropped a note to Saul Winstein, asking that he arrange for someone to pick me up at my South Gate address (9237 San Antonio Avenue) on June 5 (date of Morgan lecture) for I have a fair-sized box of slides to carry.

I declined a recent invitation from Prof. Dr. R. Delaby to attend a meeting of the Union Internationale de Chimie on the 24th to 27th of July in London.

Margaret M. Lamm began working as a clerk ($165/month) for our group today.

MUC-WMM-3, dated May 20, arrived from Donald C. Stewart at the Metallurgical Laboratory. Addressed to me, this is a report by L. B. Asprey and D. C. Stewart, summarizing the carrying and solvent extraction experiments (tracer scale) with americium that were carried out during the winter of 1945-46 in Chicago.

Mary Millard, our secretary, wrote to Ruth Rogers to request a copy of the press release that I worked on for the Lilienthal Board. She

mentioned that she has not yet found the outline of Volume 14A.

Thursday, May 30, 1946--Memorial Day

Since this was a holiday, Helen, Jeanette, and I drove north to look over additional golf courses on which I might play. The local courses include Mira Vista Country Club, Tilden Regional Park, and Contra Costa Golf Club. Although we drove as far as the Sonoma Golf Course, we found few additional suitable courses. By the time we returned home Helen felt rather ill. She tried to go to bed early, but an upset stomach kept her up.

Friday, May 31, 1946

Helen's condition did not improve during the evening; by 1 a.m. this morning, we concluded that she was in premature labor. I reached Dr. Traut at 1:30 a.m. at his home in San Francisco, but, to my dismay, he showed little interest in our problem. Helen remembered that Genevieve Calvin's obstetrician was Dr. Josephine E. Borson, whom I managed to phone at about 2 a.m. I somewhat frantically introduced myself, described the situation, and pleaded with her to serve as Helen's obstetrician. Dr. Borson arranged for an ambulance to pick up Helen and me and take us to Alta Bates Hospital. Helen soon went into the delivery room while I waited nervously in the waiting room. At about 4 a.m. the nurse came by to show me my very small (3 pounds, 7 ounces) daughter, who was then taken to the nursery to be placed in an incubator. I noticed continued activity near the delivery room, and soon I was shown my little son (4 pounds, 7 ounces, born at 4:14 a.m.), who was also taken to an incubator in the nursery. Dr. Borson, who thought I looked overly excited, gave me some sleeping pills and sent me home with Jeanette, who had driven to the hospital in the meantime, with orders to go to bed. Later I got up and drove back to the hospital to see Helen and the twins.

I then went to my Rad Lab office and found a confirmatory letter, dated May 25, to last Saturday's telegram from W. A. Higinbotham requesting my support for the "Emergency Committee of Atomic Scientists." I also heard from Ruth Rogers, who wrote that the personnel folders were mailed on May 27 and that the laboratory notebooks were also sent. Ruth also said that she has arranged for a roomette for me on the Broadway Limited to New York on September 21. Ruth reported that Winston Manning has finally had his reservations confirmed for his trip to Berkeley next week. In another letter, dated May 30, Ruth said that she would mail two copies of A-33 ("Properties of 94^{239}") on May 31. She also commented on the status of other items I have requested.

Saturday, June 1, 1946

When I arrived on campus this morning, I met Professor Giauque and learned that the news of our twins has already gotten around the Chemistry Department.

At the Radiation Laboratory I found a letter from a John M. Novak of Vallejo, who is concerned that an explosion of an atomic bomb on the surface of the ocean will continue until all of the hydrogen in the ocean will change to helium and all the nitrogen in the atmosphere will change to carbon. He concluded with "This is not a request for information but merely for assurance that you fellows know what you are doing."

Roy C. Thompson, Jr., a Ph.D. from our Chicago group, arrived and began working today.

Amasa S. Bishop called me today and asked that I serve as a sponsor for the "Committee for Foreign Correspondence" (sponsored by the Federation of American Scientists). Its purpose is to stimulate the establishment of direct contact by mail between scientists in this country and those in other parts of the world, to convince foreign scientists of our strong desire to work toward world peace and cooperation, and of our determination to avoid an atomic armament race. I asked Bishop if Dr. Oppenheimer was a sponsor, and he said he would let me know.

I visited Helen at Alta Bates Hospital and met Miss Boynton, the nurse in charge of the nursery. Helen showed me the gardenias that the Cookseys sent her and a bouquet that she received from the Calvins.

Sunday, June 2, 1946

I worked on the manuscript of my talk "The Transuranium Elements," to be given on June 21 at the Chicago meeting of the American Physical Society. This must be declassified before it can be published.

Helen and I decided, during my visit with her today, that we shall name the twins Paulette and Peter. Jo Owen, Helen's friend, visited her during the evening and brought a bouquet of sweet peas and roses.

Monday, June 3, 1946

I was assured, when I called the hospital as I do every morning, that the twins are doing fine.

Winston M. Manning is visiting our laboratory today and tomorrow. His trip was delayed because of difficulties with reservations.

Mary Millard received another letter from Ruth Rogers about the materials she is sending us. Ruth said to Mary, "When this tornado finally blows over, you and I should get together in some central spot where we can tear our hair and scream in unison." Ruth also asked about the reprinting of the "Table of Isotopes," for Miss Young (Hoylande D.) has been unable to find the plates.

Ernest Grunwald sent me a letter, which I received today, describing the final arrangements for the Morgan Lecture.

Later I visited Helen at Alta Bates Hospital.

Tuesday, June 4, 1946

Winston Manning, who is visiting from Chicago, attended our meeting this morning, over which I presided as usual. Others in attendance were Asprey, Blaedel, Bockhop, Connick, Cunningham, Eyring, Ghiorso, Goeckermann, Howland, Huffman, James, King, Knox, La Chapelle, Lindner, McVey, Miller, Morgan, Newton, O'Connor, Perlman, Templeton, R. Thompson, S. Thompson, Werner, and Westrum. Manning briefly outlined plans for the Metallurgical Laboratory, which will be known as the Argonne National Laboratory after July 1. Templeton spoke on the most recent bombardment of Pb^{206} with helium ions. Werner reported on the americium work and his efforts to oxidize americium. There was also a discussion about the results of Cunningham and Asprey in which americium was apparently oxidized and then rapidly reduced.

* * * * *

I received the royalty agreement form for the periodic table from M. W. Welch, of the W. M. Welch Manufacturing Co., to look over and sign.

A letter arrived from Sidney W. Fox (a classmate from UCLA), who is at Iowa State College, asking to whom he might write about procuring 8-day radioiodine. Perlman agreed to answer the letter.
I read a June 1 telegram from Bob Campbell of Life magazine. Campbell has finally been successful in having the text and photos of his article approved for publication, although it was necessary to blur the chart of isotopes to illegibility before General Leslie Groves agreed.

In other correspondence, I wrote to Steve (Stephen) Lawroski, a former member of our Chemistry Section C-I at the Met Lab and now at Standard Oil of New Jersey, to tell him that we are in California, and to ask if he is still planning to spend some time here this summer on our golf courses. I also announced the birth of our twins.

I then wrote a letter of recommendation to Professor Linus Pauling at Cal Tech for Norman R. Davidson. To Nell A. Parkinson of C & E News, I sent my review (following) of "A Report on the International Control of Atomic Energy," prepared for the Secretary of State's Committee on Atomic

A Report on the International Control of Atomic Energy

This report was prepared for the Secretary of State's Committee on Atomic Energy, the "Acheson Committee", by a Board of Consultants consisting of: Mr. David E. Lilienthal, Chairman of the Tennessee Valley Authority, who acted as Chairman of the consulting Board; Mr. Chester I. Barnard, President of the New Jersey Bell Telephone Company; Dr. J. Robert Oppenheimer, of the University of California and the California Institute of Technology; Dr. Charles Allen Thomas, Vice President and Technical Director, Monsanto Chemical Company; and Mr. Harry A. Winne, Vice-President in Charge of Engineering Policy, General Electric Company.

In the words of the Board this discussion of the international control of atomic energy is submitted to the Department of State "not as a final plan, but as a place to begin, a foundation on which to build."

The proposal contemplates an international agency conducting all intrinsically dangerous operations in the nuclear field, with individual nations and their citizens free to conduct, under license and a minimum of inspection, all non-dangerous, or safe, operations. "Dangerous" and "safe" operations are carefully defined in terms of the applicability or non-applicability of the operations toward bomb production. The international agency, referred to as Atomic Development Authority, ADA, would have authority to own and lease property and to carry on mining, manufacturing, research, licensing, inspecting, selling, or any other necessary operations. As much freedom as possible would be left to national and private

research and other activity in the atomic energy field. The authors envisage an advanced research organization within the ADA and rightly believe that the presence of competent research workers engaged in a positive research program would make inspection a more certain and less laborious process.

The plan recommends a multiplicity of safeguards which would render evasion or violation of international control difficult. These safeguards include complete control of raw materials, and of the production of fissionable materials, denaturing of dangerous materials, inspection of key plants and international exchange of information on scientific and technical activities. Worth noting is the fact that the plan does not, and could not, depend on denaturing alone, as some of the earlier popular interpretations implied.

In assessing the feasibility of the proposals, one should place great weight on the fact that the plan was drawn up by men of diverse professional background, all leaders in their respective pursuits: public administration, academic science, industrial science, and business and technical administration. The reviewer strongly recommends this report for study by both technical people and the public in general.

Energy. To John E. Pfeiffer, Science Director of CBS, I wrote to say that I am unable to send any more material to him covering my Gibson Island talk later this month.

I read a letter from A. S. Bishop. He said that Oppenheimer has agreed to be a sponsor of the "Committee for Foreign Correspondents," and will be present at a press conference on June 17, 1946. Other sponsors include Professors Raymond T. Birge, Joel H. Hildebrand, Paul H. Kirkpatrick, James P. McBaine, Otto Stern, Francis A. Jenkins, and Jerzy Neyman. I told Mary to call Bishop at his extension (316) in the Physics Department and say that I will be a sponsor.

At 2:00 p.m. I attended the final examination of Edward J. Lofgren in Room 222, LeConte Hall. Lofgren passed his examination.

I went by the hospital to see Helen, who is doing fine. Since I am still concerned about the twins because they are so tiny, Helen and I talked with Alice Sedey, a nurse in the premature baby section of the nursery, who is giving special attention to Peter and Paulette. Helen pointed out the roses that Dag Dreher brought in this afternoon.

Later I took the night train to Los Angeles.

Wednesday, June 5, 1946

I spent much of the day with my parents in South Gate. At about 3:30 p.m. I was picked up at my parents' home and taken to the UCLA campus where I visited some of my acquaintances. In the evening I delivered the William Conger Morgan Lecture (Room 100, Education Building), which was sponsored by Phi Lambda Upsilon. The dinner preceding the lecture began at 6 p.m. and was held in the Green Room at the Albert Sheetz restaurant, 973 Westwood Blvd. My talk was entitled "Atomic Energy and the Transuranium Elements."

[In Berkeley, Helen came home from the hospital (by ambulance at Dr. Borson's insistence).]

Thursday, June 6, 1946

I returned to Berkeley from Los Angeles. Before going to the lab, I stopped at home to see Helen. I gave her the fruit, candy, and card that my parents sent. Helen reported that the hospital says that the twins are doing fine.

At the office I found a note from Ruth Rogers, who described the procedure to go through when I visit Chicago later this month. Ruth said that she hopes everything is going well with the newly-discovered particles and their parents.

A telegram arrived from John Lewellen asking whether he can buy, for his Club Productions brochure, a number of photographs that appeared in the May issue of Fortune magazine.

There was a general meeting of all project chemists in Room 102,

Gilman Hall at 4:15 p.m. Connick, who is coordinating these meetings, plans to have three or four speakers presenting more or less finished work. He will try to inform the speakers several days before the meeting.

Jo Owen spent part of the morning with Helen.

Friday, June 7, 1946

A letter arrived from Donald A. McPherson (John Wiley & Sons) in which he said that he also will be staying at the Windermere East on the nights of June 20 and 21 and that he will get in touch with me.

I replied to John Novak to assure him that there is no danger from the type of nuclear reaction he described.

Iz Perlman wrote a memorandum to Dr. J. G. Hamilton, requesting the following bombardments on the 60-inch cyclotron for the month of June: Pu + deuterons, 50 μah (for Berkeley); U^{233} + deuterons, 50 μah (for Berkeley and Chicago); Pa^{231} + deuterons, 100 μah (for Berkeley and Chicago); Pb^{204} + deuterons, 5 μah (for Berkeley).

In a letter to Richard Hamer, Ottawa, Ontario, Canada, I explained that, because of space limitations, we cannot accept applications for the admission of more scientists to our laboratory.

I went by the hospital and saw the twins. Paulette doesn't seem to be progressing as well as Peter.

Saturday, June 8, 1946

When I checked with Alta Bates Hospital this morning, I was again assured that both twins are doing well.

After talking with some of the fellows, I took care of some correspondence. Leo Brewer, Corresponding Secretary of the Northern California Association of Scientists, asked in a letter of June 3 that I serve in an advisory capacity to this group. Today I accepted the invitation provided that I will not be called upon to partake in formal committee meetings.

I also answered a letter from Dr. William F. Meggers, National Bureau of Standards, dated May 31. I said that elements 43, 61, 85, and 87 have not yet been named and that, if we use the definition that the longest-lived isotope should represent the atomic weight, we have the following: 43^{99}, 61^{147}, 85^{211}, and 87^{223}.

In addition, I wrote a letter of recommendation for Lyle H. Jensen to Mr. J. J. Coleman of the Burgess Battery Company in Freeport, Illinois. Jensen was a member of my Section C-I at the Met Lab.

Morgan prepared a summary of our May 60-inch cyclotron bombardments.

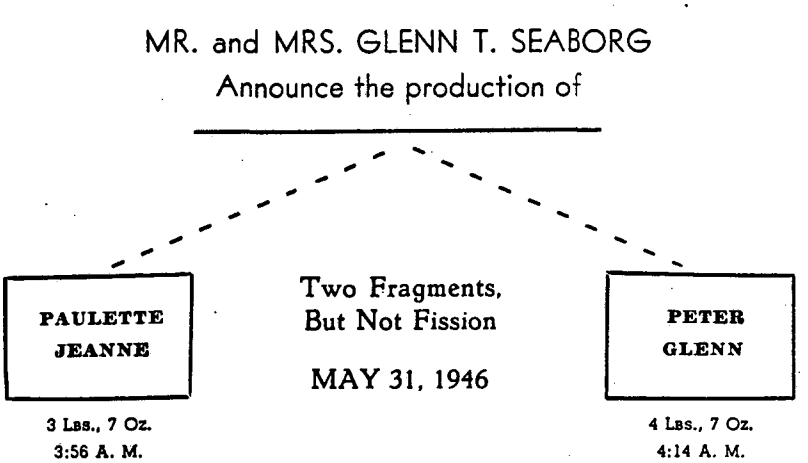
Stan Thompson and I played nine holes of golf in the afternoon at Mira Vista Country Club with Bill Knox (WJK-66, SGT-52, GTS-50). Mira Vista is a private club, located at the end of Cutting Boulevard in the

El Cerrito hills. Since we can play here on a daily fee basis, we shall probably play here frequently.

Sunday, June 9, 1946

I worked for a while on the manuscript for my talk "The Transuranium Elements" for the Chicago APS meeting.

Helen and I have had the following announcement printed to inform our friends of the birth of our twins:



At the hospital today Miss Boynton told Helen and me that Paulette is all right, in spite of our concerns.

Lee Perlman visited us for a while this afternoon, and the Calvins came over with a box of candy for Helen.

Monday, June 10, 1946

As usual, one of the first things I did at the lab was to talk with some of the fellows.

I received a copy of a memorandum (MUC-WMM-33) from Don Stewart to Winston Manning, describing a remote control system that has been set up for periodically processing hundred-gram quantities of plutonium, in order to isolate the americium growing in from the Pu^{241} present.

The following items arrived from Chicago today: one box of counting plates containing 15 mg product 49; GTS-149, 0.1 mg 95 nitrate; one lead shield containing source 155 and 50 ml of source 36; samples WMM#2 and WMM#3; one small package containing one thorium backing plate about 3" diameter x 1/8" thick; and 5.5-6 mg of 23 on a platinum target.

Nell A. Parkinson acknowledged receiving my review of the Smyth book and said that the length was satisfactory.

Tuesday, June 11, 1946

Helen and I again called Alta Bates Hospital early this morning and were assured that the twins are doing all right.

This morning's meeting of the chemistry group, over which I presided, was attended by: Asprey, Bockhop, Connick, Cunningham, Eyring, Ghiorso, Howland, Huffman, James, King, Knox, La Chapelle, Lilly, Lindner, Magnusson, Miller, Newton, Perlman, Reynolds, Robinson, Don Stewart (who is now visiting Berkeley on his way to the tests at Bikini), Templeton, R. C. Thompson, S. G. Thompson, Werner, and Westrum. I spent some time questioning the men about their writing. Perlman asked about the status of the Pu^{239} target, and Morgan explained that it probably will be taken off the 60-inch cyclotron tomorrow. James described his work with the magnetic counter. Cunningham reported on the americium inventory--9.9 mg in varying states of purity. R. C. Thompson said that there are about 15 mg of Am^{241} in one of the barrels from Los Alamos containing fractions separated from Pu^{241} and about 4 mg in the other barrel.

Westrum gave a progress report on his work on the heat of solution of thorium. Templeton told about his pulse analysis curves from samples resulting from the Pb^{206} helium ion bombardment. Miller described his experiments on the separation of americium and lanthanum with TTA and benzene.

* * * * *

Another letter arrived from Donald McPherson, informing me that Victor Weisskopf has decided to publish his book on theoretical nuclear physics with John Wiley & Sons (which should be an inducement for me to write a book for them).

I wired John Lewellen that I was unable to procure the photographs he wanted.

Helen's mother (Iva Griggs) arrived to visit with her while I am away.

I boarded the "City of San Francisco" bound for Chicago in the late afternoon.

Jo Owen visited Helen during the evening.

Wednesday, June 12, 1946

En route to Chicago. I spent some of the day reading and making suggestions on the introduction to a Chemical Reviews article by Jacob

Sacks, Director of Pharmacological Laboratories of Endo Products, Richmond Hill, New York, which I had agreed to review.

[In Berkeley, the furniture and other things we shipped from Chicago arrived.]

Thursday, June 13, 1946

I arrived in Chicago--I shall be staying at the Windermere East Hotel.

After checking in at the Laboratory, I saw Ruth Rogers and gave her the Sacks' article and a covering letter to return to Sacks. I also picked up mail that was awaiting my arrival. There was a note from the secretary to Howard Warrington, Vice President of Prentice-Hall, Inc., asking me to have dinner with him on the 18th. I also received the proposed script for the recording session with John Lewellen.

[In Berkeley, Magnusson spoke at the afternoon meeting in Room 102, Gilman Hall on new neptunium chemistry; Peter E. Yankwich discussed measurement of soft beta rays in thick samples.

Helen uncrated the boxes. Wilma and Al Ghiorso visited her during the evening.]

Friday, June 14, 1946

In Chicago. I received a letter from John E. Pfeiffer of CBS, describing the subject he wants to discuss on the broadcast scheduled for 11:15 p.m. EDST on Tuesday, June 25.

At 2:30 p.m. at United Broadcasting Company, 64 East Lake Street, I made recordings with Neil Hamilton (Lewellen's Club Productions) on "Peaceful Uses of Atomic Energy." I mentioned our twins in the recording. Later I offered to go over the write-up on atomic energy by Mel Silverman.

I went to an ACS dinner and meeting at the Furniture Mart, where I saw Linus Pauling receive the Gibbs Medal of the Chicago Section and heard him deliver his acceptance address.

Saturday, June 15, 1946

In Chicago. Among some of the men with whom I talked was Truman Kohman. I gave him a portion of our new "Table of Isotopes" and asked him to criticize it; in addition, I gave him a few specific questions that Perlman and I have.

I had dinner at Ruth Rogers' home.

[In Berkeley, Wilma and Al Ghiorso visited Helen during the evening and brought her a box of candy.]

Sunday, June 16, 1946

In Chicago. I phoned Helen, who told me that Peter is gaining very

fast--Paulette is not doing so well.

The Winston Mannings invited me to their home for an excellent early afternoon dinner. About 4 p.m. Winston and I went to Jackson Park at about 4 p.m. for nine holes of golf (WMM-57, GTS-51).

Back in my hotel room I decided (about 8:30 p.m.) to go to the Hobby House, a favorite haunt of Helen's and mine, for a cheeseburger and coffee. When I got back to the hotel, I found a telegram from Peter and Paulette.

Father's Day Greeting *by* **WESTERN UNION**

CU16 NL PD=TDJD ALBANY CALIF JUN 15 .(48)

DR GLENN T SEABORG=

HOTEL WINDERMERE EAST=

WERE FINE AND GAINING SO PLEASE HURRY HOME LOTS OF LOVE

ON FATHERS DAY=

PETER AND PAULETTE=

[In Berkeley, Stan and Alice Thompson visited Helen. They gave her a bouquet of sweet peas and then took Helen and her mother for a ride.]

Monday, June 17, 1946

In Chicago. I gave Ruth Rogers Mel Silverman's write-up on atomic energy, with my corrections along with a covering letter, for return to him.

From 10 a.m. until 12 noon I attended the Technical (Information) Meeting of Research Staffs of the Manhattan Project (Room 2, Rosenwald Hall) at which Frank H. Spedding presided. Then from 2 p.m. to 5 p.m. I presided over a second session of the Technical Meeting (Room 2, Rosenwald Hall). I gave the concluding paper in the session on "Chemistry of Am and New Isotopes of Po."

Iz Perlman called me from Berkeley and gave me the bad news that Paulette is very ill.

[In Berkeley, Edgar Westrum wrote a memorandum to George Everson, requesting that Winifred Heppler be promoted to technician.

Jo Owen visited with Helen during the evening.]

Tuesday, June 18, 1946

In Chicago. I attended the continuing Technical Information meeting in Room 2 of Rosenwald Hall.

When I telephoned Berkeley, I learned that Paulette has been moved to Children's Hospital in Oakland.

Dr. Warrington of Prentice-Hall and I had dinner together at the Windermere Hotel East in order to discuss the possibility of my writing a book on nuclear chemistry. Warrington claims that Prentice-Hall has perhaps the best sales coverage of any of the publishers and its college business ranks third after McMillan and McGraw-Hill, even though their company is only 33 years old. However, I found my mind was more concerned with Paulette's illness than the book business.

[In Berkeley, our group met as usual with Perlman presiding. In attendance were Asprey, Blaedel, Bockhop, Cunningham, Eyring, Ghiorso, Goeckermann, Howland, Huffman, James, King, Knox, La Chapelle, Miller, Morgan, O'Connor, Perlman, Robinson, Templeton, R. C. Thompson, S. G. Thompson, Werner, and Westrum. Eyring talked about his qualitative results on the calorimetric measurements on thorium. Morgan discussed his bombardment of 8 mg of 171 gt plutonium (70 μah of 22 Mev deuterons). He said the TTA extraction is not satisfactory for decontaminating americium from fission products such as neodymium, praseodymium, and element 61. Goeckermann described his U^{235} plus proton bombardment. R. Thompson reported on his progress with the extraction of Am^{241} from the Los Alamos barrels. Blaedel talked about his work with the pulse analyzer.

* * * * *

Mary Millard replied, in my name, to another query from Richard Hamer, explaining that elements 43, 61, 85, and 87 were erroneously claimed to have been discovered and named. All four have recently been discovered in radioactive forms and will be named by their discoverers. These names will appear on the new issue of the Welch chart.

Helen had lunch at Jo Owen's. Don and Milicent Cooksey and Bee

Barbachano, a girlhood friend of Helen's, visited her during the early evening.]

Wednesday, June 19, 1946

In Chicago. At 2.30 p.m. I attended the Council Meeting for laboratory division directors in the conference room (A-5) of the New Chemistry Building. Then, at 5:30 p.m., I took the Capitol Limited (B&O#6) bound for Washington (Lower 1, Car 58).

[Paulette died at 11:35 p.m.]

Thursday, June 20, 1946

I arrived in Washington, then travelled to Gibson Island, Maryland. When I arrived at Gibson Island, I called Helen, in response to a message, and learned of Paulette's death. I told Helen that I would return home as soon as possible.

I gave the talk "Use of Radioactive Tracers in Chemical Research" to the Gibson Island Conference on Petroleum.

Cutting short my planned stay at Gibson Island, I rode with Paul Bartlett and others back to Baltimore, where I caught a train to Chicago.

[In Berkeley, there was no Thursday afternoon meeting.]

Friday, June 21, 1946

I arrived in Chicago in the morning and gave my talk on "The Transuranium Elements" at the American Physical Society meeting at the University of Chicago; the talk was moved forward from the scheduled afternoon time in order to allow for my early return to Berkeley. Ruth Rogers managed to get me a seat on a DC-3 leaving Midway Airport about 2 p.m. bound for Oakland. The plane, a "puddle jumper," made many stops en route.

Saturday, June 22, 1946

Helen and Jeanette met me when I arrived in Oakland at about 4 a.m. and took me home for a couple of hours sleep. Paulette's funeral service was held at 10 a.m. at Little Chapel of the Flowers (Adeline Street near Ashby Avenue) with Reverend Fred Stripp, a professor in the Speech Department at the University of California, conducting the service. Helen's mother and Don and Milicent Cooksey attended the service. Reverend Stripp also spoke briefly at Paulette's internment, which took place at Sunset View cemetery in Kensington.

Later Helen and I went to the hospital to check on Peter, who seems to be doing well and now weighs about 5 pounds.

Sunday, June 23, 1946

Helen and I spent a quiet day together.

Monday, June 24, 1946

As usual, I made the rounds of the laboratories in Bldg. 4 in order to be brought up to date on the research.

I learned that Iz and I have been given an office on the third floor of Gilman Hall--Room 317.

Bertrand Goldschmidt, a French radiochemist who spent some time in Chicago working with our group during the early Metallurgical Laboratory days, called Iz and me, explaining that he is on his way to the Bikini tests. Since it is rather difficult to have foreign visitors at the laboratory, we invited him to accompany us while we played 18 holes of golf in Tilden Park (IP-108, GTS-119). This park is located in the Berkeley hills behind the Radiation Laboratory and has a public golf course. It was a most pleasant afternoon with lots of reminiscing and conversation.

While I had dinner with some visiting colleagues, Jo Owen came to our home to have dinner with Helen and Jeanette. Bee Barbachano also stopped in to see Helen.

Tuesday, June 25, 1946

Today's meeting was attended by Asprey, Blaedel, Connick, Cunningham, Eyring, Ghiorso, Goeckermann, Howland, Huffman, James, King, La Chapelle, Magnusson, McVey, Morgan, Newton, O'Connor, Perlman, Reas, Reynolds, Robinson, Seaborg, Templeton, S. Thompson, Werner, and Westrum. I brought up the question of conflict between the Thursday afternoon meeting and Dr. Herbert Young's seminar, and we agreed to have the research meeting on Wednesday afternoon. I then gave a report on the Chicago Information Meeting. Perlman, James, and Morgan talked about the bombardment of plutonium with deuterons and the difficulty in decontaminating the americium. Werner reported on his experiments investigating the change of oxidation states of americium. Westrum discussed his measurement of the heat of solution of thorium (-182.7 Kcal/mole) and the heat of formation of $ThCl_4$ (-290.7 Kcal/mole). Cunningham mentioned the preparations for work on the extraction of Am^{241} from the Los Alamos barrels, i.e., extraction residues from the decay of Pu^{241}. Blaedel, Howland, and Robinson discussed instrumentation. There was some discussion on other bombardments (lead, U^{235}, etc.). Finally, Perlman asked if we can get some samples from Bikini (where the atomic bomb tests will be held beginning the end of the month), and I replied that Don Stewart is going to check on this.

* * * * *

A thank-you letter arrived from L. Reed Brantley for the paper on atomic energy. Reed said that his talk was well received. He mentioned that he attended the biennial conclave of Alpha Chi Sigma in St. Louis during the week of June 12, and he hopes that we can get together soon.

Wednesday, June 26, 1946

I read a memo, dated June 6, from Don Stewart, describing the

fabrication of an "all platinum" interceptor target to be used in the Berkeley cyclotron for bombardments for the Berkeley and Chicago groups.

Amos S. Newton prepared a document "Activities of Berkeley Chemistry Group, Period May 6 - June 23, 1946" for his boss, C. E. K. Mees of Eastman Kodak, a copy of which I received today and read. Newton covered the following topics: isotopic ratios of uranium by alpha particle pulse analysis, heat of formation of thorium tetrachloride, chemistry of americium and the separation from the rare earths and curium, isotopes of heavy elements, and separation of isotopes of elements other than uranium in the calutron.

There was a Wednesday afternoon research meeting in Room 102, Gilman Hall.

Thursday, June 27, 1946

I read a number of reports and journals, in addition to talking with the fellows.

One gram of radioactive soda pulp arrived from Chicago today.

Mary Millard submitted my claim for reimbursement for my Chicago trip (per diem basis of $6.00) with my cancelled train ticket, pullman stub, and plane ticket stub.

Ellen Jane Grahlman began working today as a counting girl for our group (Laboratory Technician--$165/month).

I accepted membership on the Commission of Radioactive Constants of the International Union of Chemistry in a letter written to Professor Dr. R. Delaby.

Friday, June 28, 1946

I talked with some of the men and then looked over the mail. I received and immediately replied to a letter from Roy H. Beaton. Roy asked about experimental cancer treatment at the University of California in behalf of the wife of one of the operators at Hanford. I said that I know of no treatment by radioactive methods and suggested he contact Dr. John H. Lawrence here.

To a June 24 request from Willard L. Valentine (Science), I said that I will send him a section of my paper on "The Transuranium Elements," given at the Chicago APS meeting, as soon as the photostats of some of the slides are ready.

Earlier this month a request arrived from Robert Littell, Senior Editor of The Reader's Digest, for a copy of my report to the American Chemical Society meeting in Pittsburgh. Today I mailed him a copy, asking that he allow me to check his material if he uses the report for an article in his magazine.

I answered a letter, dated June 5, from Edward M. Crane, President of D. Van Nostrand Company, Inc. Crane was impressed with my article in C & E

News on "The Impact of Nuclear Chemistry," and asked if I was interested in writing a survey book for them. In my reply I explained that I and a colleague (Iz Perlman) have practically completed arrangements with another publisher to write a book on nuclear chemistry.

Maurice E. Nahmias of Seine, France, who was a Rockefeller fellow working at the Radiation Laboratory in 1937, wrote asking for some reprints and copies of the Bulletin of the Atomic Scientists of Chicago. Today I mailed the material to him and said that it was good to hear from him.

Saturday, June 29, 1946

As is our routine, most of the fellows worked on their research during the morning. I read and then looked over the mail. A thank-you letter arrived from John Pfeiffer of CBS. Pfeiffer enclosed a radio script on the use of radioactive tracers and asked for my criticism so that they can do better reports in the future.

Kenneth E. Davis of the University of Rochester, in a letter dated June 25, asked for a print of the "Table of Isotopes" for elements of Z greater than 90. This is for use in making a pocket-sized table for their staff.

In a letter dated June 27, I received a signed royalty agreement from M. W. Welch, who said that he will be glad to have final copy for the new periodic chart whenever it can be released.

Helen and I had dinner at the Perlmans' home.

Sunday, June 30, 1946

Tom Morgan, Stan Thompson, and I played nine holes of golf at the Contra Costa Golf Club (TM-51, SGT-49, GTS-49). This semi-private club, open to the public on a daily fee basis, is located in Pacheco (near Concord). It is a nine-hole course. Unfortunately, its fairways are parched and its "greens" are oil-stained dirt.

Monday, July 1, 1946

In the mail today I received a letter from E. S. Proskauer (Interscience Publishers, Inc.), asking if I have come to a decision about editing Advances in Nuclear Chemistry. Milton White wrote to inquire if I could spare any reprints of the "Table of Isotopes." Alfred O. C. Nier inquired about a long-lived (~250-day) titanium. Ruth Rogers informed me that the talk I gave at the Chicago APS meeting on the transuranium elements has not yet been cleared for publication (in Science) and said that it must be submitted again. She also mentioned that she changed the order to 25 copies of the chart. I also heard from Donald A. McPherson of John Wiley, who described terms of their contract. French Hagemann sent me a recalculated value of 0.017 seconds for the half-life of 85^{217}.

Mary Millard returned to Ruth Rogers some signed consultant's agreements from the Metallurgical Laboratory, explaining why they were not notarized and adding that I am not sure I should sign their patent agreement since I signed one here in Berkeley.

Later I read, worked on various writing projects and with Iz on revisions to the "Table of Isotopes." Then I wandered through the labs to watch the men at work.

Tuesday, July 2, 1946

Present at today's group meeting were Asprey, Beaufait, Blaedel, Connick, Cunningham, Howland, James, King, La Chapelle, Magnusson, Miller, Morgan, Newton, O'Connor, Perlman, Prescott, Reynolds, Robinson, Seaborg, Templeton, R. Thompson, S. Thompson, George Watt (who is visiting Berkeley from the University of Texas), Werner, and Westrum. Perlman announced that two people (Ghiorso and James) will probably go to Chicago to make fission measurements of some short-lived isotopes including Am^{242}, isotopes from plutonium + deuterons, Cm^{242}, polonium isotopes, RaE, and then some older samples. Cunningham said that the equipment for handling the Los Alamos barrels containing Am^{241} should be ready the end of the week. Westrum said that he will probably convert 125 μg of 85% americium hydroxide to the fluoride today for use in the preparation of americium metal. Werner talked about experiments with the oxidation states of americium. Miller reported on experiments on TTA extraction of lanthanum and americium to determine the H^+ and TTA power dependence. Magnusson stated that he is running some bromate oxidation curves for the rate of oxidation of neptunium. Westrum talked about the vapor pressures of americium metal and other heavy elements. Robinson announced that the pulse analyzer has not been performing well. Howland said that one gram of neutron-irradiated bismuth has arrived from Dayton. Unfortunately, it is relatively new so we must wait for its decay to get rid of the Bi^{210} before we can look for Bi^{208} (formed by the n,2n reaction). Morgan concluded the meeting by saying that the neutron bombardment of plutonium to look for Np^{239} from the n,p reaction will come off the cyclotron immediately after the meeting.

* * * * *

A letter arrived from Don Stewart, expressing sympathy at Paulette's

death and saying that George Everson (Personnel Director, Radiation Laboratory) made a formal offer to him to join our group.

Helen and I brought Peter home from Alta Bates Hospital today. He now weighs six pounds 2 ounces and is doing well. When we picked him up, his nurse Alice Sedey gave us a cap she made for him. We also received a couple of pages of instructions. Miss Boynton, who is in charge of the premature baby nursery, offered to purchase him from us for $10. After we arrived home, Peter became irritable and cranky and had difficulty taking his bottle. He sucked so hard that he closed the nipple. It was a difficult night, and the instruction sheets did not help.

Alta Bates Hospital
MATERNITY

[To be given to mothers when they leave]

Born 5-31-46

Baby—Name Seaborg B

Birth Weight 4 lbs 7 ozs. Length 19"

Discharge Weight 5 lbs 15 ozs. Head 13"

Chest 12"

Never Use Anything for Baby that is not Labeled

1. Bathe baby in a warm room.
2. Have everything ready before you start, all clothing aired, etc.
3. Bathe just before morning nursing, if cord is off give tub bath.
4. Wash face, head, eyes, ears, and nose—and proceed with bath. (Do not use powder.)
5. Keep baby covered as much as possible while bathing, if sponge bath is given.
6. Dry thoroughly with a bath towel.
7. Keep baby's crib in a sunny and well-aired room day and night.
8. Do not cover baby's face. The baby needs fresh air.
9. Don't keep baby too warm. If the day is hot, remove extra covers.
10. Have a firm baby mattress instead of pillows.
11. Do not use head pillows for baby.
12. Give baby water twice daily (which has been boiled and cooled to blood heat). 8 am 4 PM
13. *Change position in bed after each nursing.* 2 ams
14. When in doubt about anything, call your Doctor or go to his office.
15. Don't ask the neighbors.

ALTA BATES HOSPITAL

FORMULA

Amount baby takes at feeding 3 - 3 1/2 ounces

Number of feedings daily 7

Time of feeding 6 - 9 - 12 - 3 - 6 - 10 - 2

FORMULA—

Plain Morning Evaporated Milk 7 ounces
Boiled water 17 ounces
(Meads) Dextro Maltose No 1 2 tablespoons
Lactic acid 16 drops
Drisdol 3 drops
Vitamin C 1 Tablet

Preparation:

Equipment—

1 Measuring Cup—preferably enamel.

1 Small Sauce Pan.

All boiled 10 minutes—1 teaspoon, 1 tablespoon, 1 large milk bottle or seven six-ounce baby bottles.

Method— Dextro Maltose

Dissolve ~~sugar~~ in the hot water, add the milk. ~~Place directly over the flame and boil 3-5 minutes, stirring constantly.~~

~~Measure, add boiled water to make up amount boiled away, pour immediately into a large boiled milk bottle or~~ measure each feeding into the baby bottles which have also been boiled. Cork with rubber stoppers or cotton, cool and place on ice.

Nipples—

After feeding, wash nipple immediately in cold water and boil. Keep in sterile glass.

Do not use nipples with holes so large that the milk flows too freely or with holes too small. Enlarge opening, if necessary, with a small red-hot needle point.

If nipple becomes soft and collapses when in use, discard it for a new one at once.

Dissolve Vit. C Tablet in small amt. of warm formula. Mix with remaining amount. Put in refrigerator until cold. Add Lactic by dropper stirring constantly. Add Drisdol 1 drop in 3 bottles.

Wednesday, July 3, 1946

Much of my day was spent making the rounds of the laboratories in Building 4. There was a Wednesday afternoon research meeting in Room 102 Gilman Hall.

I also did some shopping in Berkeley for Helen. I ran into Dr. Lowell E. McAlear, Peter's pediatrician, who surmised from my expression that Peter had given us a hard time last night. He assured me that babies of this age do not like a change in environment and that the situation will improve.

Thursday, July 4, 1946--Independence Day

Peter still seems to be frustrated when he takes the bottle. So, later in the morning I left him with his mother and went with Stan to Mira Vista for 18 holes of golf (SGT-97, GTS-95). Stan and I really enjoy this course.

Friday, July 5, 1946

I checked in my hill office and read a July 3 memorandum from Dan Miller to Iz Perlman entitled "The Extraction of Americium and Lanthanum with TTA-Benzene." The work reported was a continuation of the work described in Report BC-1 and was conducted in order to determine more completely the effect of the conditions on the extraction and separation of americium and lanthanum by the procedure. Few attempts were made to explain the results.

After talking with some of the fellows, I spent some time in our office in Gilman Hall preparing for the course "Nuclear Chemistry" (Chemistry 123) that Iz Perlman and I are going to teach this fall semester. We expect a rather large class and plan to use, to some extent, the lecture notes from the talks I gave at the Met Lab in the summer of 1942. These were issued as Met Lab Report CL-440 and were widely reproduced.

Saturday, July 6, 1946

After going to the lab in the morning, I spent the afternoon with the family--Jo Owen and her mother visited. Later Gen and Melvin Calvin stopped by with some Easy-Flow nipples and bottles, which Gen thinks may help with Peter's feeding problem.

Sunday, July 7, 1946

I spent the day with Helen, Jeanette, and Peter. Peter seems to be adjusting better to his parents and to being at home. Helen and I have been astonished and pleased with the number of gifts, cards, and letters we have received from our friends (both local and Chicago) and relatives.

At 10:15 p.m. the CBS program "University Explorer" was broadcast over San Francisco station KQW. I participated in the production of the program "Twentieth Century Wonderland" with Hale Sparks and discussed generally the subject of nuclear power.

Monday, July 8, 1946

I talked with some of the fellows and then looked over the mail. Lyle H. Jensen, now at the University of Washington, wrote requesting that I write a letter of recommendation for him to Dr. H. L. Johnston at Ohio State University, where Jensen has applied for a position.

Donald J. Simkin, who will work with Eugene Huffman in the analytical chemistry group, began working today (as a Chemist P-1).

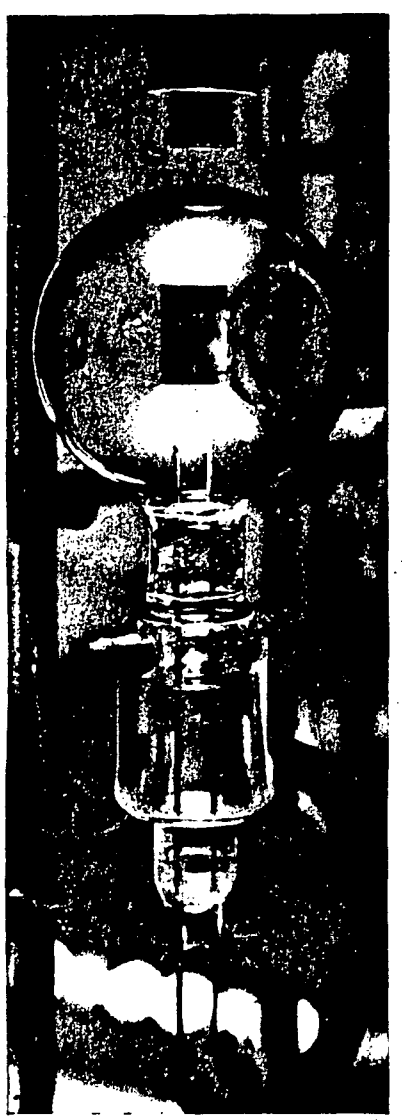
I spent some time in my Gilman Hall office looking over the material Iz Perlman and I plan to cover in our course on nuclear chemistry this fall.

The Ghiorsos stopped over during the evening to visit and to see Peter. The article about our laboratory in Chicago appeared in the July 8 issue of Life magazine. [A few excerpts follow:]

PLUTONIUM LABORATORY

HISTORIC RESEARCH ON MAN-MADE ELEMENT USED IN ATOMIC BOMBS WAS DONE AT UNIVERSITY OF CHICAGO'S "NEW CHEM"

photographs for LIFE by F. W. GORO



CLOSE-UP OF FURNACE on opposite page shows substances vaporized in the reaction which produces a pure metal condensing in silvery coating on the bulb.

AMONG the barren hills near Hanford, Wash. are massive concrete structures housing great atomic piles which make plutonium for atomic bombs. The piles ingest rods of pure uranium, manufacturing plutonium in them by atomic fission. When the rods are taken out of the piles they are dissolved in concrete "canyons" and passed through a long series of chemical reactions to separate the plutonium. The development of this chemical process was an enormous stride in the making of the atomic bomb. It is also one of the classic adventure stories of science. LIFE presents much of this epic story in these 16 pages of pictures by F. W. Goro, the first exploration of one of the great government-built laboratories which worked on the bomb. The pictures show the University of Chicago's secluded New Chemistry Laboratory and the little-known scientific methods by which "New Chem" solved critical problems in the chemistry of plutonium.

The study of plutonium's chemistry formally began in April of 1942 when a little group of chemists led by the University of California's Dr. Glenn T. Seaborg assembled in Chicago. It was the beginning of a desperate summer for the U.S. and its Allies. The Russians and English were to fall back on Stalingrad and El Alamein; the U.S. had still to fight its defensive battles of the Coral Sea and Midway. Only a handful of scientists knew that the Allies faced a graver danger: the possibility that the Germans could make an atomic bomb.

The secret campaign of the chemists was closely fitted into the magnificent structure of research and technology which led to the bomb. The foundation of the structure was laid in 1939, when scientists discovered the fission of U-235, a rare form of uranium which in nature is evenly mixed with the much commoner U-238. So much energy was released in fission that U-235 was quickly suggested as an explosive of vast destructive power. In 1943 the U.S. Army engineers began to build plants to separate U-235 and U-238. Even earlier, however, the enormous difficulty of this had led some scientists to a new line of thought. If U-235 could be made to split without separating it from U-238, they reasoned, its fission would convert part of the U-238 into a completely new artificial element, called plutonium. This element, known theoretically to be fissionable, might then be separated much more easily from uranium to make atomic bombs. Early in 1942 the University of Chicago's Metallurgical Laboratory was set up to look into this possibility. The laboratory had two main objectives. The first was to determine if a chain-reacting atomic pile could be operated to make plutonium. The second, taken up by the chemists, was to find out how plutonium could be separated from uranium and numerous other elements produced by fission.

The chemists seemed to face an appalling task. No man had ever seen plutonium. Furthermore, it was impossible to wait until a pile could make a sample: the plutonium separation plants had to be ready the day the first pile made its first plutonium. On Dec. 2, 1942 the first self-sustaining pile was run in a squash court at Chicago's Stagg Field, not to make plutonium but to prove that a pile would work. By this time the chemists were already solving their biggest problems.

A few fragments of early research gave the chemists their starting point. In May of 1940 E. M. McMillan and P. H. Abelson detected invisible amounts of the artificial element neptunium after bombarding uranium compounds in the University of California cyclotron. Neptunium was expected to be an intermediate step in converting uranium into plutonium. Later in 1940 McMillan, Seaborg, A. C. Wahl and J. W. Kennedy used the California cyclotron to make tiny quantities of plutonium. Even though these infinitesimal samples could not be isolated, their chemistry could be roughly outlined by following them through chemical reactions by their radioactivity (*see pp. 70–71*). This secondary evidence, however, was not enough to determine all of the properties of plutonium. It was now necessary to make enough plutonium for direct observation of its chemical reactions.

The chemists made their first visible samples of plutonium by the same general method they had used earlier. The big cyclotrons at California and at Washington University in St. Louis were set to work bombarding uranium compounds. After many weeks the cyclotrons had manufactured a few hundred millionths of a gram of plutonium, somewhat less than the head of a pin. About a thousandth of a gram was the entire world's supply of plutonium until an experimental pile in Clinton, Tenn. made its first sample early in 1944.

Working with their tiny samples of plutonium, the chemists quickly reached their first objectives. On Sept. 10, 1942 Dr. Burris B. Cunningham and L. B. Werner weighed the first pure plutonium compound (*see p. 74*). By the end of the year the still-secret process for separating plutonium from uranium had been worked out by Stanley G. Thompson. Within months plutonium was as well understood as many natural elements. By the fall of 1944 the processes which grew out of this early research, amplified ten billion times from the laboratory scale, separated the first plutonium made in the piles at Hanford. By the summer of 1945 plutonium had been used to make at least one of the three atomic bombs set off at Alamogordo, Hiroshima and Nagasaki.

CONTINUED ON NEXT PAGE

DR. GLENN T. SEABORG, WHO HEADED INVESTIGATION OF PLUTONIUM'S CHEMISTRY, STANDS IN FRONT OF CHART IN HIS OFFICE WHICH LISTS THE VARIOUS ISOTOPES OF THE ELEMENTS

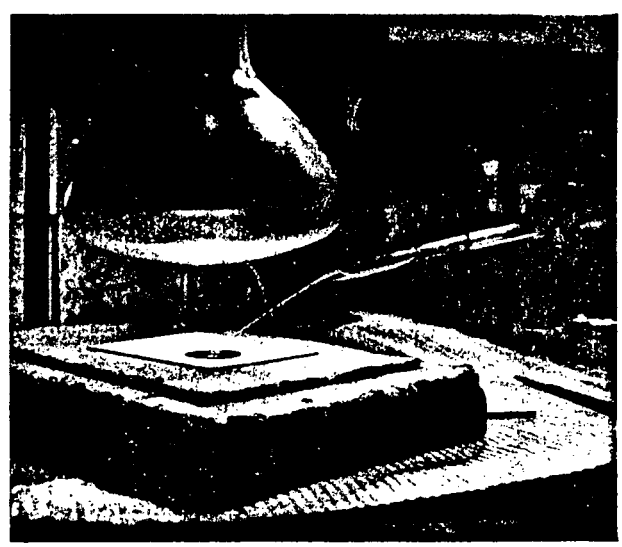
RARE SAMPLES ARE TRACED BY RADIOACTIVITY

In the beginning of the summer of 1942 the only samples of plutonium were too small to see but the Chicago chemists coolly began to work with them. The only way these infinitesimal quantities could be studied was by the modern scientific method called tracer chemistry.

Tracer chemistry is the science of following invisible amounts of radioactive elements through chemical reactions with instruments such as the Geiger counter (LIFE, April 1) which detect radioactivity. Plutonium is radioactive, so tracer chemistry could be used in the study of it. Later researches in New Chem used tracer chemistry to investigate many other rare radioactive elements.

Because the samples of elements studied in tracer chemistry are too small to be isolated, their properties must be inferred from how they act in the presence of other elements and compounds,

IN TRACER EXPERIMENT Chemist Stanley Thompson, watching his work in a mirror while protected by lead bricks, adds one solution to another which contains a radioactive element. A solid compound then precipitates, or settles, out of the solutions.



INFRARED LAMP dries a sample of precipitated compound placed on a platinum disk. The chemist is trying to find out if the radioactive element under study has also precipitated out of the solution. If it has, element will be present in the dried compound.

MICROCONE IS LOADED with chemical solution by Dr. Cunningham. Solution is in a long, thin pipette held by micromanipulator at right. Dr. Cunningham turns knobs of two micromanipulators to bring microcone and pipette together. When solution is squirted out of the pipette, reaction is observed through the microscope.

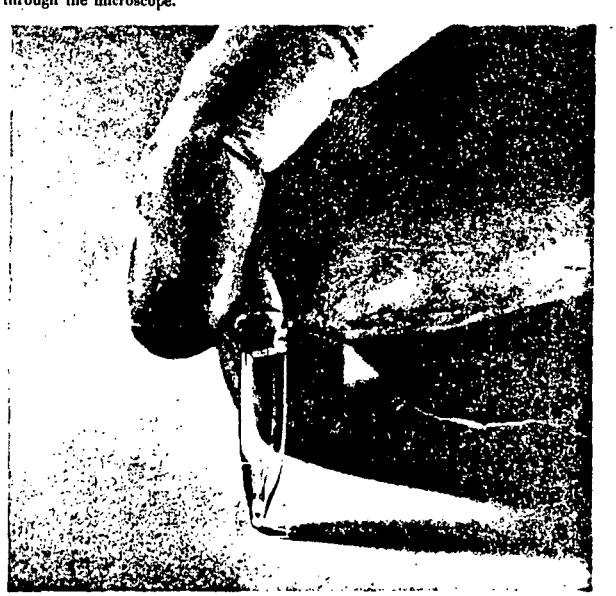
EVEN BIGGEST LAB SPECIMEN IS VERY SMALL

Even after they had passed the milestone of isolating the first visible quantities of plutonium made in cyclotrons, the Chicago chemists continued their work on an incredibly small scale. At the end of 1942 they had less than 500 micrograms of plutonium in pure compounds. A microgram is a millionth of a gram. A dime weighs 2,500,000 micrograms (2.5 grams). Before the war the smallest observable chemical reactions had been performed in microchemistry with quantities seldom less than a thousandth of a gram. It would have taken years for the cyclotrons to make enough plutonium for extensive work on this scale, so the chemists evolved a branch of their science called ultramicrochemistry.

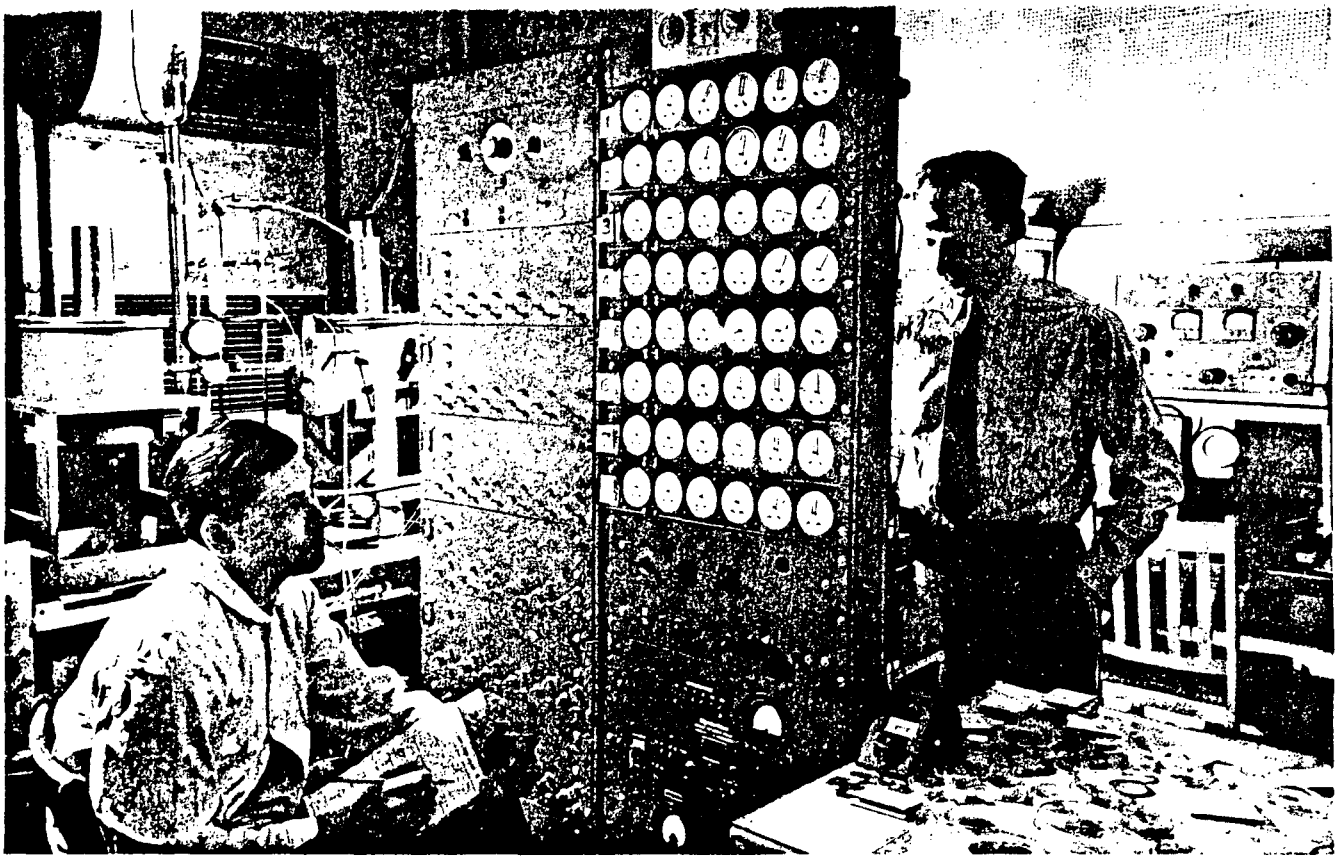
Ultramicrochemistry is ordinary chemistry evenly scaled down in all its parts. Its chemicals are weighed in sensitive balances, squirted through tiny pipettes, heated in tiny crucibles by tiny furnaces. The test tube of ultramicrochemistry is the microcone, shown below. On the opposite page are compounds of plutonium and neptunium as they are prepared in the precipitation cell at the tip of the microcone.

Ultramicrochemistry contributed heavily to early achievements in the study of plutonium. Dr. Cunningham (*left*) and L. B. Werner used ultramicrochemical methods to achieve a historic climax in the work of the chemists: the isolation of plutonium's first pure compound (*see p. 74*). The next great step, the discovery of the chemical reaction which was to separate plutonium from uranium, was also studied in microcones. Before they had finished, the ultramicrochemists went through all of the reactions now used to separate plutonium in the canyons outside Hanford's atomic piles.

CHEMIST MEASURES OUT solution for ultramicrochemistry by carefully pushing it from a graduated glass tube. Pressure inside the tube is increased by turning knob. The solution is automatically stirred by a thin glass rod entering bowl from the left.



MICROCONE is a closed glass tube with a narrow neck at the bottom. Triangular speck in the tip of the tube is a compound which has been precipitated from solution. Part of the microcone shown in pictures on the opposite page is indicated by square.

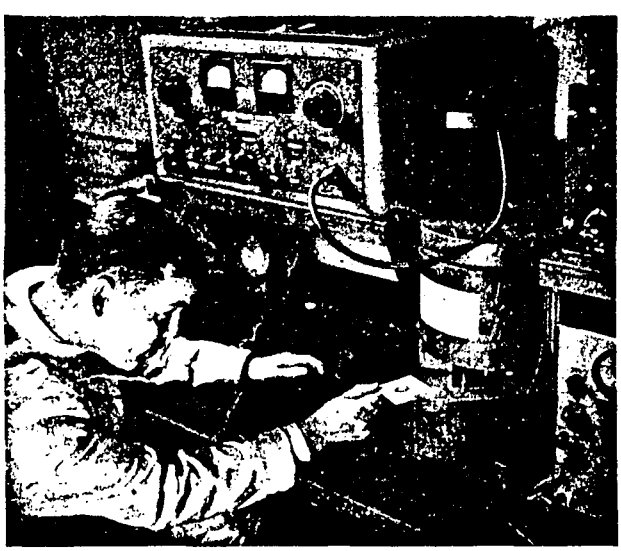


ALBERT GHIORSO (LEFT) READS DIALS OF PULSE ANALYZER HE DEVELOPED TO DISTINGUISH BETWEEN DIFFERENT RADIOACTIVE ISOTOPES BY RELATIVE SPEEDS OF PARTICLES THEY EMIT

i.e., combinations of elements. A typical tracing experiment begins with a solution containing 1) a known compound and 2) tiny amounts of a radioactive element. A second solution is then added to the first which causes a new compound to precipitate, or settle, out of the mixture. If a Geiger counter detects most of the original solution's radioactivity in this precipitated compound, the chemist may assume that the radioactive element forms a similar compound. From this he can infer at least some of the radioactive element's properties.

Few problems of tracer chemistry are this simple. Some radioactive elements decay by emitting heavy alpha particles, others by emitting light beta particles. When an alpha and a beta emitter occur together it is fairly easy to tell them apart. When two alpha emitters (or two beta emitters) occur together it is more difficult. The only difference between similar particles from different elements is their speed. Accordingly tracer chemists use sensitive instruments which can determine particle speed. One such machine is the elaborate pulse analyzer shown above.

But even this ingenuity was not enough to solve all of the chemists' problems. To complete their knowledge, the chemists had still to work with visible, weighable amounts of plutonium (*see next page*).



DRIED PRECIPITATE is placed in a Geiger counter to determine if the radioactive element is present. Chemist here works without the protection of lead bricks because radioactive element could at most be present in precipitate in harmlessly small amounts.

EXPERIMENTER TESTS HANDS in another counter to see if they are contaminated with traces of the radioactive element. Chemist Thompson used this method to find compound which would separate plutonium from uranium and fission products.

CONTINUED ON NEXT PAGE

Tuesday, July 9, 1946

This morning's group meeting was attended by Asprey, Beaufait, Blaedel, Cunningham, Eyring, Goeckermann, Howland, James, King, La Chapelle, Magnusson, Miller, Morgan, Newton, O'Connor, Perlman, Reynolds, Robinson, Seaborg, Templeton, R. Thompson, S. Thompson, Watt, Werner, and Westrum. I outlined the plans for the trip to Chicago by James and Ghiorso, who will work with William C. Bentley and Earl K. Hyde on the neutron fissionability of various isotopes using the pile. I announced that the 100 μg sample of Am^{241} bombarded with neutrons at Clinton will be worked up soon. Goeckermann reported on the neptunium fraction of the U^{235} + proton bombardment at the 60-inch cyclotron, and Morgan discussed the bombardment of 100 mg of plutonium with fast neutrons. Werner reported on his solubility experiments with americium and curium. R. Thompson described the work being done on the two barrels from Los Alamos containing Am^{241} arising from the decay of Pu^{241}, saying that the two barrels are quite different. Westrum is getting ready for the preparation of americium metal. Blaedel will take over this work when Westrum leaves. Magnusson concluded the meeting with a discussion of some unreported work he did with Hindman at the Met Lab on Np(V).

* * * * *

Iz Perlman and I had lunch with Robert R. Gibson. Gibson, who is in the San Francisco office of Prentice-Hall, Inc., is interested in our writing a book on nuclear chemistry and described their publication policy for us.

Wednesday, July 10, 1946

Much of my day was spent talking with the fellows in their laboratories in Bldg. 4.

Speakers at the Wednesday afternoon meeting in Gilman Hall were Roy Thompson reporting on protactinium chemistry and B. Peters talking about counting beta rays.

Thursday, July 11, 1946

After the usual administration matters, I looked over the mail. Truman Kohman wrote, saying that he would prefer to look at a photostat of the entire corrected "Table of Isotopes" rather than just a portion of it. He said he will check the points about which I asked him when I was in Chicago and will write to me in a few days.

A thank-you note arrived from Robert R. Gibson for the meeting Perlman and I had with him on Tuesday. He is looking forward to our decision on signing the publication agreement.

Peter had a check-up with Dr. McAlear this morning; he is doing fine. In the afternoon Lois Russell and Dorothy Axelrod visited Helen and Peter. [Lois worked as a secretary with Helen before Helen left for Chicago in June of 1942; Dorothy was a biologist in the Radiation Laboratory at that time.]

Friday, July 12, 1946

This morning I went to an overflowing Room 212 of LeConte Hall for the 273rd meeting of the American Physical Society. At 10 a.m. the group moved to Room 210, a more comfortable auditorium. There were a variety of interesting papers from "Description of a Frequency Modulated Cyclotron and a Discussion of the Deflector Problem" by E. J. Lofgren and B. Peters to Emilio Segrè's paper, "A Paradox on Nuclear Isomerism." In recognition of Professor Birge's retirement as Pacific Coast Secretary, there were a number of laudatory remarks about his service. I did not attend the luncheon but went to my office to check on the status of things there.

A telegram arrived from Daniel Melcher, Director of the National Committee on Atomic Information, asking for a paragraph of comment on the first anniversary of the opening of the atomic age. This will be distributed to the newspapers on August 6.

I read a letter from Ruth Rogers, who brought me up to date on a number of items, including consultant and patent agreements, the refund on my Baltimore trip, reservations for my September trip, and developments on the publication of my article on the transuranium elements. Ruth asked about the return of Don Stewart from the Bikini tests (Don is planning to move to Berkeley from Chicago). Finally, she said, "Don't look now, but I think the army is about to get a death grip on Science--at least as represented here at Argonne. Things are rather discouraging, in spots." I then called Ruth to acknowledge the letter and to ask her about several other things.

In the afternoon I returned to the APS meetings in LeConte Hall.

Saturday, July 13, 1946

"The Production and Use of High Energy Particles" was the title of today's symposium at the American Physical Society's meeting in LeConte Hall. Again the meeting was packed. The invited papers included "Why Are We Interested in High Energy Particles?" by J. Robert Oppenheimer, "The Experimental Problem of Accelerating Charged Particles" by Ernest O. Lawrence, "The Design of a Linear Accelerator for Protons" by Luis W. Alvarez, "The Synchrotron" by Edwin M. McMillan, and "The 184-inch Cyclotron" by Robert L. Thornton.

Later in the afternoon Stan Thompson and I went out to the Mira Vista Country Club for nine holes of golf (SGT-52, GTS-46).

Sunday, July 14, 1946

Much of my day was spent working on the "Table of Isotopes."

Lee Perlman and then Professor and Mrs. Axel Olson stopped by to visit us and to see Peter.

Monday, July 15, 1946

I talked with some of the fellows before looking at the mail. An

agreeable, most interesting response to my letter of April 26 arrived from Professor Fritz A. Paneth. He agreed that Coryell et al. should name element 61 and that Segrè and Perrier should name element 43--astatine. Corson, MacKenzie, and Segrè, he agreed, are the discoverers of element 85. Although he feels that Miss Perey is the discoverer of element 87, he does not like her first-proposed name--catium. However, she recently suggested the name francium (symbol Fa), and he approves of this. Paneth then went on to say that he will mention the names "americium" and "curium" and give a reference to our recent publications in his article about the naming of the artificial elements.

In June I mailed, in response to his request, a copy of my Pittsburgh talk ("Future Possibilities with Radioactive Tracers") to Robert Littell of The Reader's Digest. Today a letter arrived from John T. Beaudouin, Littell's associate, saying that he is not interested in an article based on the material.

Ruth Rogers wrote, in a letter I received today, that material from my files is being shipped today but that my requests for duplicate files of reports and currents reports are being held up. She is trying to check on the problem.

In response to his request of last Friday, I sent the following statement to Daniel Melcher, Director of the National Committee on Atomic Information:

> The realization of nuclear energy in the form of the atomic bomb a year ago has given rise to international political and social problems of unprecedented magnitude and importance. The first step toward their solution lies in bringing before the people the facts about nuclear energy and its implications and great progress in doing this has been made in the State Department's Acheson Report on the International Control of Atomic Energy and its favorable reception. This year has also seen the beginning of actual plans for the development of nuclear energy for application to industrial uses, and it is expected that research in this field will get a good start in the second year of the atomic age.

I received a letter from John H. Manley in Los Alamos, who said that he understands that Oak Ridge considers the material on the chart of heavy isotopes that I used in Chicago to be declassified, and asks if he may use it. I immediately replied, saying that I have been told that the chart was cleared only for the talk and must be further cleared before publication. I suggested that he may be able to get it cleared faster than I can. I also replied to Kenneth E. Davis' June 25 letter, saying that the isotope chart has not yet been cleared for publication.

I wrote to Donald A. McPherson of John Wiley & Sons, Inc., and said that Dr. Perlman and I have decided to publish with Prentice-Hall. I explained that John Wiley's offer was as good as Prentice-Hall's, but, beginning a number of years ago, I have been influenced by conversations with Professor Latimer in the matter of publishers.

Later, a telegram arrived from Truman Kohman, stating that a request

for the fission product table should be directed to J. M. Siegel, Clinton Laboratories.

In other correspondence, I wrote to E. S. Proskauer, Interscience Publishers Inc., to say that my commitments are too heavy for the next several years to accept the editorship of Advances in Nuclear Chemistry. Another letter went to Willard L. Valentine, Editor of Science, in which I explained that my Chicago paper, "The Transuranium Elements," is ready, and I will airmail it to him as soon as it is cleared for publication.

I mailed Milton White five reprints of the "Table of Isotopes," noting that they are in short supply. I also reported that we are preparing an up-to-date version that we hope to have ready for publication in the open literature in six months or one year. I explained that Science will soon publish my Chicago talk with a copy of the slide containing more information on the transuranium elements.

Then I sent Steve Lawroski a reminder that Stan and I are waiting to hear from him about our golfing vacation.

Helen and I had dinner at the home of Dr. and Mrs. Charles Prescott. [Charles was in charge of much of the work in chemistry for Ernest Lawrence's isotope separation project carried on here in Berkeley during the war.]

Tuesday, July 16, 1946

Present at this morning's group meeting were Asprey, Beaufait, Blaedel, Cunningham, Eyring, Fontana, Ghiorso, Goeckermann, Howland, James, King, La Chapelle, Lindner, Magnusson, Miller, Morgan, Newton, O'Connor, Perlman, Prescott, Reynolds, Robinson, Seaborg, Templeton, R. Thompson, and Westrum. There was some discussion about the fission measurements and samples to take place in Chicago later this month. Perlman announced that we now have skeleton plans for the Chemistry Annex (previously known as the hot lab). Newton said that he plans to study the distribution of fission products and the cross sections for fission by high energy alpha particles. R. Thompson reported on the process for the extraction of Am^{241} from the Los Alamos barrels. I mentioned that we would like to know about vacation plans. Morgan then reported that the fluosilicate procedure for the separation of americium and curium from the rare earths is not as bad as previously reported and then said that they have done further work on Werner's oxidation procedure for separating americium from rare earths. Westrum reported the production of a silvery metallic material, presumably americium metal, formed by reduction of the fluoride, but it will have to be verified.

* * * * *

I replied to a June 11 note, forwarded from Chicago, from George G. Manov, a friend since graduate school days here in Berkeley. Manov is now living in Garrett Park, Maryland. I explained that I am now back in California so that perhaps we can see each other when he visits here in September or at the fall meeting of the American Chemical Society in Chicago.

In other correspondence I answered a letter received on July 1 from Alfred O. C. Nier and said that I have been unable to find evidence in the literature for a 250-day titanium isotope. I acknowledged a March 14 letter from Professor German E. Villar in Montevideo, Uruguay, which called my attention to some of his work on the position of the heaviest elements in the periodic table. However, I said that I cannot agree with his formulas in which a tetravalent character is assigned to hexavalent uranium compounds. I also wrote to John E. Pfeiffer to say that I believe his broadcast script is well done.

I telephoned Charles D. Coryell to discuss getting a copy of the table of fission product activities to use in the preparation of our "Table of Isotopes." I assured Charles that the "Table of Isotopes" will not be published until after the fission product table appears in the open literature.

Alexander Bratenahl began working as a Chemist P2 with Eugene Huffman in the analytical chemistry group.

Wednesday, July 17, 1946

Today I accepted a July 5 invitation from Professor Oliver J. Grummitt of Western Reserve University to give two lectures on "Transuranium Elements" in their sixth annual "Frontiers in Chemistry" series on March 21, 1947, in Cleveland.

Paul C. Aebersold asked, in a letter dated July 9, about my plans for updating the "Table of Isotopes." In my reply sent to him at Oak Ridge, I said that Iz Perlman and I are revising it and that it will be published in some publication of Argonne or in the Handbook. We want it in the open literature as soon as it is cleared. I mentioned that I had not thought about selling it and asked if he thinks that is consistent with publishing it in Reviews of Modern Physics.

I read a copy of a letter from Martin H. Studier to Ralph James. Marty asked Ralph to check his calculations, on the basis of which he assigned the 4.4-day activity to Np^{233} rather than Np^{234}.

Lofgren and Cunningham spoke at this afternoon's meeting of the chemistry groups in Room 102, Gilman Hall.7

Helen had an appointment for a postnatal checkup with Dr. Josephine Borson this afternoon; she has another appointment with Dr. Borson in a couple of weeks.

Thursday, July 18, 1946

This morning I answered a letter from E. K. Burger of Venice, California, saying that I have no information about the type of camera he wishes to buy. I suggested he contact Professor C. S. Garner (Department of Chemistry, UCLA) for information on matters of nuclear science.

In addition, I wrote to Professor Roger Adams, University of Illinois, to ask advice about how to handle a letter I received from Dr. Eberhard Grassmann, a German nuclear scientist, who would like to obtain

a passport to leave Germany and to receive books in the nuclear field. I then wrote to Dr. Grassmann to explain that I have never been in a policy-determining position that would allow me to answer questions such as he has asked. I suggested that perhaps Professors James Chadwick, M. L. Oliphant, or J. D. Cockcroft in England might help him. I told him that I have brought his situation to the attention of Professor Roger Adams, who has been scientific advisor to the administrators of the American Zone of Germany.

Another letter went to Jim Gibson of Ellensburg, Washington, a prospective student, to whom I gave the historical explanation for the use of the letters s, p, d, and f in the designation of electronic structure. In answer to a letter of June 29 from Baltzar von Platen, Stockholm, Sweden (co-inventor of the Electrolux refrigerator), I said that I do not believe that it will be possible to obtain energy from the nuclear fission reaction at as high a concentration (500 watts/cm^3) as he mentioned.

E. S. Proskauer (Interscience Publishers, Inc.) acknowledged my letter of July 15, in which I declined the offer to become the editor of Advances in Nuclear Chemistry. He said he hopes, however, that if the plan materializes, I will at least become one of the authors.

Friday, July 19, 1946

At the laboratory today I read reports and journals and talked with the fellows.

Helen and I had dinner at the Axel Olson home (2696 Cedar). We met two of Hanna Olson's sisters. Later, the older Olson son Bill, his wife Margaret, and the younger son Pete came by to greet us. [Axel Olson, who has been on the faculty here since about 1920, taught our Chemical Thermodynamics in the fall of 1934--my first course in chemistry here at Berkeley.]

Saturday, July 20, 1946

While Jo Owen spent the time with Helen, I went to Mira Vista Country Club with Al Ghiorso and Stan Thompson for 18 holes of golf (AG-109, SGT-100, GTS-102).

Sunday, July 21, 1946

Jerry Howland shot 112 and I shot 98 in our 18 holes of golf at Mira Vista Country Club.

Later the Latimers stopped in to visit us and to see Peter.

Monday, July 22, 1946

In today's mail was an executed copy of my consultant agreement for the Metallurgical Laboratory at the University of Chicago from W. B. Harrell, Business Manager.

An invitation arrived from Norris E. Bradbury, Director at Los

Alamos, for me to attend a conference there on fundamental problems in nuclear physics on August 19 to 24, and to present a paper on a subject of my choosing.

Willard L. Valentine, Editor of Science, acknowledged my letter and the delay in clearance of my paper "The Transuranium Elements" but said they are still looking forward to receiving the paper.

I read James' reply to Studier about the 4.4-day neptunium. James gave Studier some revised information but said that he doesn't believe the question of the assignment can be resolved with the information they have. He suggested a repeat of the proton bombardment of U^{233}.

Iz Perlman and I signed and returned to Prentice-Hall an agreement to prepare a book of about 175,000 words on the subject of nuclear chemistry by October 1, 1948. We will receive an advance royalty of $1,000 for this.

I made the usual rounds of the labs. All of our graduate students have had laboratory experience, which makes research somewhat smoother than it would be with inexperienced students.

Tuesday, July 23, 1946

I attended our group meeting in the morning along with Asprey, Beaufait, Blaedel, Bratenahl, Connick, Cunningham, Eyring, Fontana, Goeckermann, Heppler (a technician with Westrum), Howland, Huffman, King, La Chapelle, Lilly, Lindner, Magnusson, Miller, Newton, O'Connor, Reynolds, Templeton, R. Thompson, Werner, and Westrum. I announced that there will be no meeting next week and then asked for and received reports on the status of the samples for James and Ghiorso to be used in the fission measurements. We discussed the controversy about the assignment of the 4.4-day neptunium, which Studier now claims should be assigned to Np^{233}. I said that a good proton bombardment of U^{233} would probably clear up the assignment. Westrum described his three americium metal reduction experiments and his new melting point measuring apparatus. He also mentioned his preparation of NpF_3 for neptunium metal production runs. Blaedel talked about another melting point apparatus being set up. I announced that we should have x-ray facilities soon for determining molecular structure; William Shand, Jr. is now here and will work with us and the Gilman Hall group on the problem. Magnusson reported on the Np(IV) TTA chelate solubility in benzene. Templeton and Howland talked about their lead bombardments. There was some discussion about the desirability of obtaining suitable radiation protection devices as soon as possible. Goeckermann then talked about his new program for the investigation of isomeric transitions in the tellurium isotopes. Finally, I reported that Prescott and Reynolds are starting a program of measuring the ionization potentials of the transuranium elements.

* * * * *

A letter arrived from Steve Lawroski, who will spend part of his vacation in California. Steve asked for our plans so that he can adjust his vacation to fit ours. I immediately went to show the letter to Stan and discuss the matter with him.

In a memorandum to Donald Cooksey, Associate Director of the Radiation Laboratory, I asked that he make arrangements through the Area Engineer's Office to have a complete copy of the Metallurgical Laboratory card index sent to Berkeley; the index is necessary for reference work and for the writing on the Plutonium Project Record.

Wednesday, July 24, 1946

The first thing I did this morning was to reply to Steve Lawroski's letter to give him the plans for the golfing vacation that Stan and I have planned. I said we plan to fly to Los Angeles over the dinner hour on August 9 and will conclude our vacation on August 19. I suggested that he may plan to come to Berkeley and fly down with us or fly directly to Los Angeles. In either case he may stay with one of us in Los Angeles--with me at my parents' home, with Stan at his folks' place, or in a hotel. In addition, I said that if he wants to send his clubs out here, he may send them to my parents' home in South Gate.

I wrote to Norris Bradbury to accept his invitation to speak at the August meeting. I said that I plan to arrive in Albuquerque at 2:00 p.m., Monday, August 19. The title of my talk will be "Nuclear Properties of Some Heavy Isotopes."

I had a chest x-ray today in the office of Dr. E. Schulze Heald (2560 Bancroft Avenue)--this is a part of the routine lab physical examination.

Then, at the afternoon meeting in Gilman Hall, Edward L. King spoke on the activity coefficient of uranyl chelates as a function of the TTA concentration in the benzene layer; Stan Thompson talked about the bombardment of Np^{237} with helium ions.

Thursday, July 25, 1946

Much of my day was spent making the rounds of the laboratories in Bldg. 4, but I also found time to work with Iz on the "Table of Isotopes."

Friday, July 26, 1946

Before going to the hill, I worked for a while in my office in Gilman Hall (Room 317), preparing lecture notes for the course in nuclear chemistry (Chemistry 123) that Iz Perlman and I are going to give during the coming fall semester.

On the hill I spent time on the "Table of Isotopes."

Saturday, July 27, 1946

I wrote a quick note to Steve Lawroski to explain that we now have reservations leaving San Francisco at 8:00 a.m. on August 9, instead of the later time I mentioned earlier.

Amos Newton and I played nine holes of golf at Mira Vista Country Club (ASN-62, GTS-48).

Sunday, July 28, 1946

Al Ghiorso and Ralph James have left for Chicago to participate in some fission measurements.

Today Jerry Howland was my golfing partner--we played 18 holes of golf at Mira Vista (JJH-113, GTS-100).

Monday, July 29, 1946

I sent a revised version of Report A-33 ("Properties of 94-239") to Joe Kennedy for his and Art Wahl's approval, explaining that the original version was not cleared for publication because it is not yet permissable to publish quantitative data on the fission properties or hitherto unpublished radioactive properties of Pu^{239}.

PROPERTIES OF 94-239
May 29, 1941*

We would like to report that we have observed the fission of 94^{239} with slow neutrons. The cross section for the fission of 94^{239} with slow neutrons is even larger than that of U^{235}. The cross section was determined by comparing the number of fissions obtained with a sample containing 94^{239} with the number obtained with a sample of ordinary uranium under conditions which were as identical as possible in every detail. Two separate methods were used to prove that the fissions in the 94^{239} sample could not be attributed to the presence of uranium impurity: (1) complete and identical chemical tests on the isolation of rare earth carrier material for element 94 from non-irradiated uranium, i.e., "blank tests," in which the absence of uranium impurity in the final product was established by showing the absence of uranium alpha-particles and (2) demonstration that the ratio of the cross section for slow neutrons to that for fast neutrons for the fission of 94^{239} is different from the same ratio for natural uranium. This latter experiment has not yet been carried out in its optimum form, and we hope to submit a later report giving details of an improved experiment.

The details follow:

A sample of uranyl nitrate ($UO_2(NO_3)_2 \cdot 6\ H_2O$) weighing 1.2 kilograms was distributed in a large paraffin block, placed directly behind the beryllium target of the 60-inch Berkeley cyclotron, and given over a period of about two days a 3500 microampere-hour bombardment with neutrons from beryllium plus 16 Mev deuterons. This uranyl nitrate was placed in a continuously operating glass extraction apparatus, two liters of diethyl ether were added, and practically all of the uranyl nitrate was extracted into the ether phase. The 93^{239} was isolated from the aqueous phase with rare earth fluoride carrier by the method of McMillan and Abelson[1] and the mixture of 3 mg of La and 3 mg of Ce which was "carrying" the 93^{239} was reprecipitated as fluoride six times in order to remove any uranium impurity. This sample of 93^{239} at the time of its purification (which included purification from element 94) had an activity of 125 millicuries as determined with the aid of an ionization chamber, connected to an FP-54 vacuum tube electrometer,

which had been calibrated in an absolute manner for 93^{239} radiation by the use of a Geiger counter and the method of aliquots. After the 93^{239} had decayed into 94^{239}, preliminary fission tests were made on this sample which then contained 0.5 microgram of 94^{239}. This sample was placed near the screen window of an ionization chamber which was imbedded in paraffin near the beryllium target of the 37-inch Berkeley cyclotron. This gave a small, but detectable, fission rate when a 6 microampere beam of deuterons was used. However, this count was too small to be sure that the fissions were not due to uranium impurity and the fission rate with fast neutrons (i.e., when the chamber was surrounded with cadmium) was too small to determine with any accuracy at all.

This sample, which had a thickness of total material amounting to 4.8 mg per cm^2, was then subjected to a chemical procedure[2] designed to isolate the 94^{239} in much less material. The procedure was carefully tested in blank experiments with the aid of element 94 from deuteron-activated uranium as tracer to show that the yield was practically 100 per cent. In this manner the 0.5 microgram of 94^{239} was isolated with lanthanum fluoride in which the thickness of total material amounted to 0.16 mg per cm^2. (In a blank experiment in which the same amount of lanthanum fluoride was isolated from non-irradiated uranium by an identical chemical procedure the alpha-count was less than 1/4 per minute. Correcting for the geometrical factor, as determined with the aid of a known amount of uranium of the same thickness, this means that the sample contained less than 3 micrograms of U^{238} impurity and hence less than 0.03 microgram of U^{235} impurity. This is the upper limit of uranium impurity which may be present, as determined by the limitations on the method of detection, and the actual amount might very well be much less than this.) A standard uranium sample, probably in the form of anhydrous oxide, of a thickness and area very nearly the same as that of the 94^{239} sample, was prepared by the electolysis of ordinary uranium out of absolute ethyl alcohol solution. This uranium standard contained 200 micrograms of U^{238} and hence 1.46 micrograms of U^{235}. The easily measurable fission rates of the 0.5 microgram 94^{239} sample and the 1.46 microgram U^{235} sample were compared when placed near the screen window of an ionization chamber imbedded in paraffin near the beryllium target of the 37-inch Berkeley cyclotron. When the ionization chamber and samples were completely surrounded with a shield of cadmium and boron carbide (B_4C), the fission rates dropped to negligibly small values.

Worth appending here is the information which we have obtained about the alpha-activity of 94^{239}. During the decay of the 125 millicurie sample of 93^{239}, the sample was placed near an ionization chamber connected to a linear, pulse amplifier in order to watch for the growth of alpha-particles. A strong magnetic field was used to bend out the beta particles. An alpha-particle activity was observed to grow with a half-life of about 2.3 days, which is the half-life to be expected for growth from 93^{239}. The alpha-count grew to the value 240 per minute. After the sample was thinned to 0.16 mg per cm^2, the alpha-count became 800 per minute. Correcting for the geometrical factor, as determined with the aid of a known amount of uranium of the same thickness as this sample, the total alpha-emission of the

sample amounted to about 60,000 per minute. This corresponds to a half-life of about 3 x 10^4 years.

*This letter was received for publication on the date indicated but was voluntarily withheld from publication until the end of the war. The original text has been somewhat changed, by omissions, in order to conform to present declassification standards.

1. E. M. McMillan and P. H. Abelson, Phys. Rev. 57, 1185 (1940)
2. G. T. Seaborg, A. C. Wahl and J. W. Kennedy, Phys. Rev. 69, 376 (1946)

In a reply to a request from M. W. Welch, I explained that I do not have a print of the photograph that he wants but perhaps he may be able to get it from Bob Campbell of Life magazine.

Tuesday, July 30, 1946

A most interesting letter arrived from Roger Adams in response to my request for information about the problems of Dr. Eberhard Grassmann, a German scientist. He said:

> In my opinion there is no chance to obtain a passport for a German to come to the U.S.--except for the special categories--until after the peace settlement. Even bringing in Germans for specific jobs has not yet been clarified. However, I advise you to write to Mr. J. C. Green, Publications Branch, Dept. of Commerce, Washington, D.C., who is following this matter closely and can give you the latest information.
>
> When I left Germany in March, and from the newspapers, there is no indicated change as yet, each zone is run with complete independence so that the U.S. would have no influence with respect to an individual in the French, British or Russian zones. I'm not sure in which zone Neustadt/Holstein is located.
>
> Many of the scientific libraries in government institutions in the Russian zone were removed completely to Russia, as well as the more important equipment in the laboratories. In the other zones the thieving has been chiefly by individuals rather than by the military.
>
> Conditions in Germany are bad. They have received essentially no scientific journals since 1940 and any governmental support of research no longer exists. Since May 1, research along practically every line has been permitted, but it is something else to get a salary, needed equipment or chemicals and I might add enough food to induce a person to want to do research. Our military government has little sympathy for scientists and they fall in the same category as the average man on the street.
>
> I understand it is possible to send packages up to 11 lbs. to Germans so that I presume books are acceptable. Grassmann might, however, prefer food since I see no opportunity for any intensive research in

Germany for some time to come. Frankly, I don't know what to advise.

Wednesday, July 31, 1946

I had an 8:40 a.m. appointment with Dr. Kent F. Kohler, my dentist, who is now located at 2200 Grove Street. Dr. Kohler cleaned and checked my teeth. I then went to the hill.

A termination notice, effective June 30, of my consultant contract with the University of Chicago arrived from W. B. Harrell, who explained that as of July 1, 1946, the Metallurgical Laboratory was succeeded by Argonne National Laboratory.

I also received a thank-you note from Daniel Melcher for my statement for the National Committee on Atomic Information.

Harry G. Cisin of Electronic Exhibitors asked, in a letter dated July 22, if I, as a member of a jury committee, would help select the ten living persons in the U.S. who have made the most outstanding contributions to the science and art of electronics.

I received from Hoylande D. Young a copy of my paper, "The Transuranium Elements," along with a letter, dated July 24, explaining that Dr. Manning wants some changes incorporated into it regarding credit. She asked that I review the manuscript and then resubmit it.

A lengthy letter, dated July 25, arrived from Paul Aebersold about our proposed revision of the "Table of Isotopes." Paul is primarily concerned about distribution at a small cost.

I also read a copy of a letter from Kenneth Priestley, our Radiation Laboratory business manager, to William B. Harrell, University of Chicago, suggesting that the Metallurgical Laboratory notebooks we have that are to be returned to Chicago be photostated here, rather than in Chicago, since some of them are in continuous use.

A. H. Bazell was employed as a Chemist P-1 today.

Tom Morgan, Iz Perlman, and I left the lab right after lunch and played 18 holes of golf at the Tilden golf course (LOM-107, IP-104, GTS-98).

Thursday, August 1, 1946

I handled a few administrative matters and then looked over the mail. I received a July 26 letter from Geoffrey Wilkinson in England. Wilkinson informed me that he expects to be in Berkeley to begin his research with our group about September 17; however, he has not yet received an official letter from the University nor a reply from the International House. He said he needs these documents to satisfy the immigration authorities.

As usual I visited our laboratories in Bldg. 4 to discuss the research progress with the members of our group. Later I read--I am trying to keep up with the report and journal reading.

Helen had an appointment with Dr. Borson this afternoon for another postnatal checkup; she seems to be doing fine.

Friday, August 2, 1946

This morning a teletype arrived from Winston Manning, requesting that Roy Thompson visit Argonne for a week or so because they are having trouble with non-extractability of plutonium in the Redox process.

In the afternoon, Amos Newton and I played 18 holes of golf at Tilden Park (ASN-111, GTS-93). Amos claims to be a non-golfer.

Saturday, August 3, 1946

Part of the morning was spent working in my Gilman Hall office on my notes for Chemistry 123 (Nuclear Chemistry).

Later, up in Bldg. 4, I read a letter from Norris Bradbury. He included a questionnaire for the purpose of providing proper local transportation when I visit Los Alamos later this month.

Herman J. Bradley began working as a Senior Laboratory Technician for our group.

Sunday, August 4, 1946

Jerry Howland and I played 18 holes of golf at Mira Vista Country Club (JJH-113, GTS-98) in the morning. Later Jeanette, Helen, and I took Pete for a ride in Jeanette's car.

I spent some of the afternoon and evening working on the "Table of Isotopes."

Monday, August 5, 1946

I received a letter from Robert Machol of Unicorn Press, asking for information about the names of elements 43, 61, 85, and 87, to be used in an encyclopedia on which he is working.

A formal letter of appreciation for my contributions to the Metallurgical Laboratory arrived from Farrington Daniels, who has now

returned to the University of Wisconsin:

> For a long time I have meant to write a letter to express formally, the appreciation of the Metallurgical Laboratory for the splendid contributions which you made to its success. Through your previous background of experience, your keen insight into the problems involved and your outstanding leadership you were able to solve rapidly all the chemical problems needed for the production of plutonium. These successful accomplishments played an important part in the early termination of the war. Your influence was not confined to the Metallurgical Laboratory alone; the men that you trained went out into the other laboratories of the project to solve additional problems.
>
> I enjoyed greatly attending your seminars and I was impressed with high scientific caliber of the research and with your complete knowledge and guidance of the program.
>
> Although we have both left the Metallurgical Laboratory I am sure that there will be effective, continuing cooperation between the Chemistry Division of the Argonne National Laboratory and the California Laboratory. In my position on the Board of Governors I will do all that I can to encourage such cooperation. Dr. Manning seems to be carrying on well in spite of the loss of many key men.
>
> I want you and Mrs. Seaborg to know, too, how saddened Mrs. Daniels and I were to hear of the loss of one of the twins.
>
> I'll expect to see you in Chicago in September and possibly at Los Alamos in August.

A lengthy letter arrived from Truman Kohman about the Plutonium Project Record. He has completed the survey chapter on the plutonium isotopes. Kohman also proposed a revision of the editorial organization and would include another volume, entitled something like Radiochemistry or Radioactivity of the Heavy Elements. He again mentioned that he would like to see the Isotope Table, even though it is not completed. Finally, he answered some specific questions about a number of isotopes that I had asked him to check.

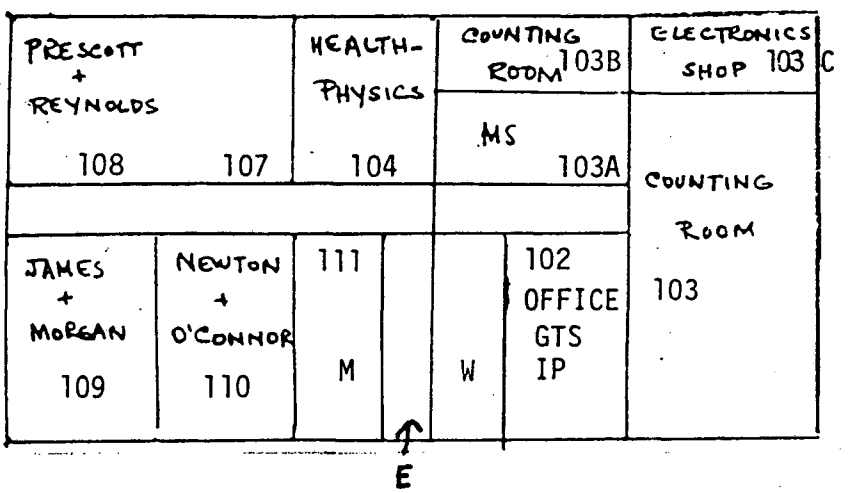
Robert Matteson, Vice Chairman, California Section of ACS, sent me details of the advance publicity for my talk in October. I promptly sent him a photograph, biographical information, and an abstract of my talk entitled "Plutonium and Nuclear Energy":

> The discussion will center primarily around the chemical developments and achievements in the nuclear energy field, with most of the emphasis placed on the important new synthetic element, plutonium. The development of the chemical procedures which are used for the extraction of plutonium from uranium and fission products at the plutonium production plants will be described with particular emphasis placed upon 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.

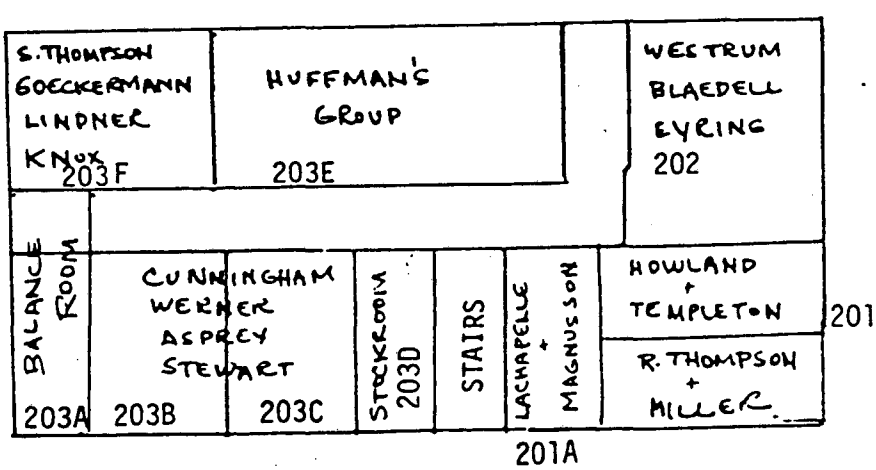
The talk will include a discussion of the discovery and study of a number of interesting new transuranium isotopes and also a discussion of the prospects for the commercial applications of nuclear energy.

Tuesday, August 6, 1946

Present at our group meeting this morning were Asprey, Bazell, Beaufait, Blaedel, Connick, Cunningham, Eyring, Ghiorso, Howland, Huffman, James, Lilly, Lindner, Magnusson, Miller, Morgan, Newton, O'Connor, Perlman, Reynolds, Robinson, Simkin, Templeton, R. Thompson, S. Thompson, Westrum, and I. I announced that Nelson Garden has been assigned full time by Professor Lawrence to the problem of radiation protection. Perlman presented the following plan for room assignments:

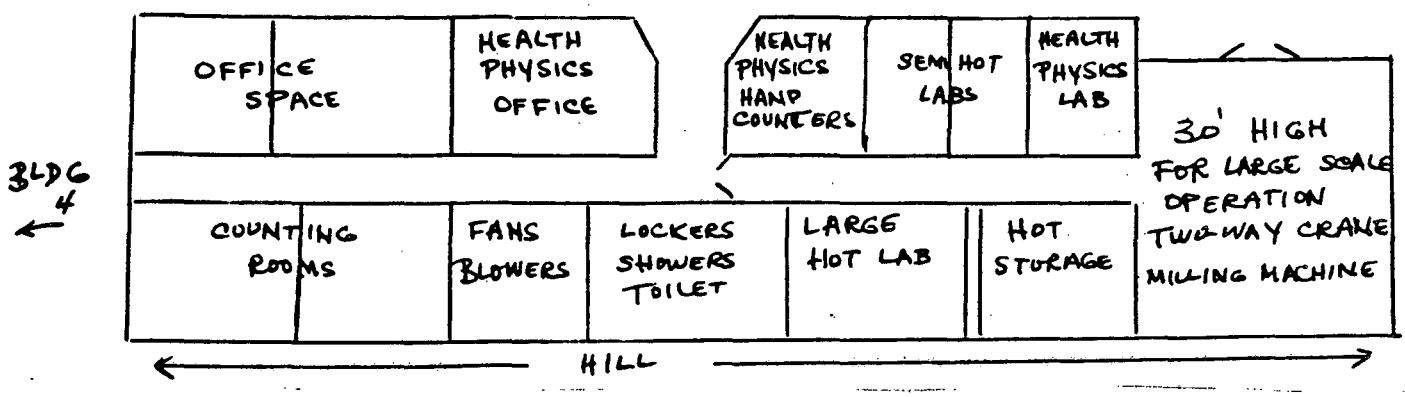


1st floor



2nd floor

Newton gave a preliminary sketch of the plan for the annex:



I announced that the next Tuesday morning meeting will be on August 20 and then asked for a status report on the counters, which Robinson gave. Ghiorso gave the results of the thermal fission cross section measurements he helped make in Chicago.

Isotope	Half-life	Fission (barns)
Cm^{242}	150 d, α	2800 b if Am^{241} = 3 b
Po^{208}	Long	95 b (max.)
Pb^{204}	Stable	10^{-4} b
Am^{242}	17 h, β^-	6000 b (doubtful)
U^{230}	20 d, α	25 b
U^{231}	4.4 d, K	100-400 b
U^{232}	30 y, α	10^{-4} b
Pa^{233}	27.5 d, β^-	40 b

Morgan described the Np^{237} plus helium ion bombardment, and I summarized the situation on the americium isotopes. Templeton summarized the new radioisotopes that our group has discovered in the region of lead.

* * * * *

Another letter arrived from Steve Lawroski, agreeing to Stan's and my latest plans. Steve will arrive in Berkeley on Wednesday (tomorrow) and wants a hotel reservation.

I wrote to Paul H. Fall of the ACS (with a carbon to L. L. Quill), saying that today I received notification that, sponsored by the Division of Chemical Education at the Chicago ACS meeting, I am scheduled to speak at 2:45 p.m. on Tuesday, September 10. I went on to say that, to the best of my memory, this is the first news I have had of this talk.

I mailed a photograph to Professor Oliver Grummitt at Western Reserve University and asked whom he has selected to speak on "Artificial Radioactivity" in his "Frontiers in Chemistry" series. I offered to make some suggestions if he has not yet come to a decision.

Other letters went to Roger Adams, whom I thanked for his opinions about Dr. Grassmann's problems, and to Eugene P. Wigner, whom I asked whether he thought I need attend the Wednesday, September 25, evening session of the

Princeton University bicentennial celebration. I explained that classes in Berkeley begin that week and there is some advantage to my returning to Berkeley by early Thursday morning. I also wrote to Dr. C. Reid in Chalk River. Reid wrote me in May from England that he has been awarded a Commonwealth Fellowship to study and work in Berkeley.

The National Committee on Atomic Information put out a press release today in observation of the first anniversary of the atomic bombing of Hiroshima. This included the statement I mailed Daniel Melcher on July 15 and contained remarks by people such as Robert F. Bacher, Arthur H. Compton, Helen Gahagan Douglas, David Lilienthal, Eugene P. Wigner, and others.

Wednesday, August 7, 1946

Earl Hyde arrived from Argonne to collaborate on a U^{233} plus deuteron bombardment.

When I saw Joe Kennedy, who is visiting Berkeley, he gave me a letter that he had written before he left St. Louis about our article, "Properties of 94-239" (Report A-33). Joe agrees that it should be published, even with the declassification deletion of the fission cross section. I also mentioned to Joe the idea of Lawrence and McMillan that Segrè and I write a brief history of the transuranium elements, to be published in a journal such as Nature. Joe was less than enthusiastic and said he did not see the necessity of such an article.

I received a letter from Chas. E. Waring, recently appointed head of the Department of Chemistry at the University of Connecticut. Waring is interested in hiring a nuclear inorganic chemist for his staff.

A letter arrived from Leonard I. Katzin about Volune 17A (Production and Separation of U^{233} - Survey) of the PPR. I promptly answered, saying that I agree that some of the chapters now available could well be issued in advance as secret reports. My reply regarding Chapter I, about which he specifically inquired, is that it is primarily an introduction and I doubt that it is worth issuing. In response to his query about joint publication of the work on the 4n + 1 radioactive series with the Canadians, I said that I would like to think about it. I asked for the opinions of Hagemann, Studier, and Manning. I also mentioned that if it is decided to handle it in this way, the best approach may be through conversations with the people involved.

I replied to Geoffrey Wilkinson's letter, explaining that Professor Latimer is out of town during August, and that this may delay things if Latimer hadn't taken care of it before he left.

In addition, I wrote a note to Alan E. Florin to inform him that I will be in Los Alamos August 19-23, and to ask if he is interested in some golf during that time.

Thursday, August 8, 1946

A note arrived from R. D. Richtmyer of Los Alamos, saying that he has tentatively scheduled my talk for a meeting on the fission process on Tuesday afternoon, August 20.

In response to his request of July 22, I sent the following list of ten living persons who have made the most outstanding contributions to electronics to Harry Cisin of Electronic Exhibitors: E. H. Armstrong, W. D. Coolidge, Lee De Forest, Lee DuBridge, Philo Farnsworth, Donald Kerst, E. O. Lawrence, F. E. Terman, R. J. Van de Graaff, and V. K. Zworykin.

I mailed an additional copy of revised Report No. A-33 to Art Wahl in Santa Fe, explaining that I have learned from Joe that he hasn't seen it yet. I said that we are disappointed that we can't publish the slow neutron fission cross section of Pu^{239}, but we feel it is worth publishing the deleted version unless he objects.

An acknowledgment of the receipt of our signed publication agreement, along with a $500 check, arrived from D. E. White, secretary to Howard Warrington of Prentice-Hall, Inc.

Later I went down to my Gilman Hall office to spend some time working on my notes for Chemistry 123. As usual when I am on campus, I checked in with Miss Kittredge and the Department office.

Friday, August 9, 1946

Thompson, Lawroski, and I left San Francisco at 9 a.m. on United Flight 530 and arrived in Los Angeles at 11:27 a.m. We took the airport bus to the Biltmore, where we were met by Stan's mother, Bessie Fay. During this vacation I shall stay at my parents' home (9237 San Antonio Ave., South Gate), Stan will stay with his mother (9414 Dorothy St., South Gate), and Steve will be in a motel at Firestone and Long Beach in South Gate. Later the three of us played 18 holes of golf at Rio Hondo Golf Club (607 No. Old River School Road, Downey) (SGT-94, SL-98, GTS-109).

[In Berkeley, Waldo Cohn is visiting the laboratory from Oak Ridge. Helen's mother arrived to stay with her while I am on vacation.]

Saturday, August 10, 1946

In South Gate. Steve, Stan and I again played at Rio Hondo Golf Club--nine holes in the morning, lunch at the clubhouse, nine holes after lunch. For 18 holes I shot 111, Steve shot 110, and Stan shot 93. The weather is hot but not uncomfortable.

After dinner with my parents at home, I wrote to Helen.

Sunday, August 11, 1946

In South Gate. I am enjoying being here with my parents, whom I found to be well. I'm also enjoying the dinners my mother cooks for us--it reminds me of my childhood.

Steve, Stan, and I again played 18 holes of golf at Rio Hondo (SGT-98, SL-100, GTS-100). My score was somewhat better today. [I sometimes caddied on this course when I was a boy in South Gate, and our Boy Scout camp was located on the edge of the course, near the Rio Hondo River.]

I telephoned Helen and learned that everything is fine there.

Monday, August 12, 1946

In South Gate. Today Steve, Stan, and I went to the Baldwin Hills Golf Course (5800 West Slauson Ave., Los Angeles) and played 18 holes (SGT-90, SL-94, GTS-100). This course is somewhat hilly.

[In Berkeley, Roy Thompson has gone to Chicago for a week. A letter arrived for me from W. M. Welch, explaining that his interest in having a print of the Life picture was merely to have it for their library. A thank-you letter also arrived from Jim Gibson for my suggestions. Gibson said that he has been accepted at Berkeley.]

Tuesday, August 13, 1946

In South Gate. Steve, Stan, and I went to Monterey Park and played 18 holes of golf at the Southern California Golf Course and Club (SGT-98, SL-99, GTS-96). This course is rather heavily trapped and relatively difficult.

My parents went to Hollywood to see a Swedish movie, leaving chicken, pie, and coffee for me; so I ate dinner alone and then wrote to Helen.

Wednesday, August 14, 1946

In South Gate. Thompson, Lawroski, and I played at the Western Avenue Golf Course, Inc. (Western Avenue and 121st St., Los Angeles) (SGT-93, SL-97, GTS-108). I should have done better on this flat course, but I was suffering from a migraine headache.

This evening I went to the home of Clayton and Rita Shelden (1005 Hildreth Ave., South Gate) and had dinner with them and their sons Ronnie and Gayle. [I have known Clayton since grammar school days.]

Thursday, August 15, 1946

In South Gate. Today Fred Albaugh joined Stan, Steve, and me for our 18-hole golf game at Recreation Park Golf Course in Long Beach. (FWA-128, SGT-93, SL-97, GTS-97). [Fred and Edrey Albaugh live at 1848 W. 69th St., Los Angeles. Fred is a UCLA classmate and Metallurgical Laboratory colleague while Edrey (Smith) was my first secretary in Chicago.] This is the last day Steve will vacation here.

[In Berkeley, Peter had a doctor's appointment. He weighs nearly 9 pounds and is doing fine.]

Friday, August 16, 1946

In South Gate. Stan Thompson and I played 27 holes of golf at Lakewood Golf and Country Club in Long Beach (for 18: SGT-95, GTS-92; for 9: SGT-51, GTS-51). My score is improving!

I called my office in Berkeley and learned that I received a letter from Al Florin who is pleased that I am coming to Los Alamos. He has borrowed some golf clubs. He said, however, that the Los Alamos course is being depleted to make room for houses, and he is trying to find out about the course in Santa Fe. Al also mentioned that there is some possibility that he

may have to go to Chicago while I am in Los Alamos.

Saturday, August 17, 1946

In South Gate. Today my cousin Elmer Johnson, Vance Cooper (UCLA classmate and Met Lab colleague), Stan Thompson, and I played 18 holes of golf at Rio Hondo Golf Club (EJ-113, VC-109, SGT-90, GTS-87). This is one of the few times that I have broken 90. Elmer, Stan, and I then went to visit Bud and Bonnie Coffin in their Downey home. [Bonnie (Edwards) is a friend from Jordan High School days.]

Sunday, August 18, 1946

In South Gate. I watched the Southwest Amateur Golf Championship for a while. Bruce McCormick came in first, and Russell York was second.

Monday, August 19, 1946

I left Los Angeles at 8:00 a.m. on TWA Flight 50 and arrived in Albuquerque at 2:00 p.m. Upon arrival at Los Alamos, I found that I have been assigned Room 20 of The Big House. Meals will be taken in Fuller Lodge.

Tuesday, August 20, 1946

In Los Alamos. After breakfast I attended the morning session of the conference, chaired by Egon Bretscher. Speakers were Robert Serber, Luis Alvarez, Robert Thornton, and Edwin McMillan. The afternoon session was chaired by E. E. Wilson, and the speakers were David Inglis, Glenn Seaborg, Katharine Way, Emilio Segrè, and Nicholas Metropolis. My topic was "Some Heavy Isotopes," and I described the reactions for the production of americium and the isolation of pure americium. I then described the reactions for the production of curium and noted that we have not yet isolated pure curium. I also talked about the chemistry of these two elements, the electronic structure and predictions about the chemical and nuclear properties of the transcurium elements.

[In Berkeley, the Tuesday morning meeting was held as usual. In attendance were Asprey, Bazell, Beaufait, Blaedel, Bockhop, Bradley, Connick, Cunningham, Eyring, Ghiorso, Goeckermann, Hicks, Horace Hopkins (who was with my group in Chicago and more recently has been working at Hanford; he is now in Berkeley to enter graduate school), Howland, Huffman, James, King, La Chapelle, Lilly, Lindner, Magnusson, Miller, Newton, O'Connor, Perlman, Robinson, Simkin, Templeton, R. Thompson, S. Thompson, and Werner. R. Thompson reported on his trip to Chicago, saying that no one knew anything about the 5% beta spectrograph except that it was developed in Canada. Jaffey and Freedman will find out about it and let us know. R. Thompson said that he brought back 8.7 mg protactinium and that our Pb^{206} was bombarded in the thimble of the Argonne pile for two days. (Templeton reported 200 c/m of Geiger activity in the Pb^{206} with some short-lived activity.) R. Thompson went on to say that Grosse claims he is willing to sell 500 mg of protactinium. Howland reported on the bombardment of Pb^{204} with helium ions, saying he found an 11-day alpha activity and an 11-day Geiger activity, whose daughter is the 6.4-day Bi^{206}. James then described the results of the bombardment of U^{233} with deuterons. The neptunium fraction yielded a 2.5-day alpha growth and 2.5-day electrons.

* * * * *

Jeanette drove Helen and Peter to Richmond (728 Kern Street) to visit and have lunch with Wilma and Kristine (now two and a half years old) Ghiorso. Wilma is expecting a second child the latter part of September. This was Peter's first social outing.]

Wednesday, August 21, 1946

In Los Alamos. The morning session was chaired by R. P. Feynman with Julian Schwinger, E. P. Wigner, T. W. Bonner, and C. L. Critchfield as speakers.

In the afternoon I talked with Frank K. Pittman in Bldg. D about the background of the barrels containing Am^{241} that we have received from them.

Thursday, August 22, 1946

In Los Alamos. This morning's speakers were Enrico Fermi and R. G. Sachs in a session chaired by L. D. P. King. The afternoon session was chaired by Rolf Landshoff with Victor Weisskopf, Frederick Reines, and D. J. Hughes as speakers.

Friday, August 23, 1946

In Los Alamos. Edward Teller chaired this morning's session with Felix Bloch, I. I. Rabi, and S. De Benedetti as speakers.

Later I met with Eric R. Jette in Bldg. D in order to discuss dry boxes. Their stainless steel dry boxes were made by the Kiwani Laboratory Furniture Manufacturer but are not made any more. Jette said he will have Colonel A. V. Peterson ask Kiwani to make more. If Peterson is successful, Jette will let us have one of theirs. He told me to request "a single unit dry box." In addition, Jette may set 2 kg of plutonium aside for a year or dissolve some metal for us as a source of Am^{241}. He said that the metal will not dissolve in HNO_3 but needs HCl and therefore special equipment is needed.

Saturday, August 24, 1946

I left Albuquerque at 7:35 a.m. on TWA Flight 219 and arrived in Los Angeles at 2:05 p.m.

Later I met and played nine holes of golf with Elmer Johnson at Rio Hondo (EJ-63, GTS-56).

Sunday, August 25, 1946

In South Gate. I spent the day with my parents and then took TWA Flight 39 at 7:05 p.m. to San Francisco, arriving at 9:10 p.m.

Helen reported that Pete really recognized her yesterday and gave her a beautiful smile--his first social smile.

Monday, August 26, 1946

Much of morning was spent reading my accumulated mail.

I read a June 21 letter from Dr. Venancio Deulofeu (Buenos Aires, Argentina), who said he had mailed me a copy of Ciencia e Investigacion, a journal like Nature. Deulofeu asked for a contribution to the journal and is especially interested in work on the transuranium elements.

A July 31 letter from R. Delaby, Secretary General of the International Union of Chemistry, informed me that I was elected a member of the Commission of Radioactive Constants at the meeting in London. The Commission, Delaby said, has decided to edit a table of constants of the artificial radioactive elements. Other members of the Commission include Professors Samuel Lind (Minnesota), Stefan Meyer (Vienna), and Frederick Joliot (Paris).

Laurence L. Quill explained, in a lengthy letter dated August 12, the confusion about my name appearing on the program of the Division of Chemical Education for the fall meeting in Chicago. Paul H. Fall also apologized, in a letter dated August 15, for the confusion about the talk.

In an opinion dated August 6, Robert A. Lavender disallowed the claim of Seaborg, Davidson, and Thompson (Case S-781) on the inventorship relating to the process for improved decontamination of plutonium-containing solutions by the use of scavengers. Lavender concludes that Dr. Thomas M. Davies was the first to conceive and reduce to practice the subject matter defined.

I received and read two letters from Hoylande D. Young, describing the necessary deletions to "Nuclear Properties of 93^{237}" and "The Chemical Properties of Elements 94 and 93" (both by Seaborg and Wahl) in order to declassify these articles.

I noted a letter from Frederick George Sawyer, recently appointed Associate Editor of Industrial & Engineering Chemistry and C & E News (in charge of the western territory). Sawyer, who has recently seen Jim (James M.) Crowe, is interested in producing an article in a simple, concise style that would bring the average chemist and chemical engineer abreast of developments in the nuclear field. He would like to meet me and discuss this at the Chicago ACS meeting.

John E. Pfeiffer of CBS, in a letter dated August 8, noted that I am on the Princeton bicentennial program. He said that he hopes to meet me then. I also read a letter from Eugene Wigner, dated August 14, about the Princeton meeting. He would like a two-page summary of my talk. In addition, Wigner mentioned that CBS would like a broadcast on September 24, and asked if I would participate with Daniels in a 15-minute round table discussion of the importance and significance of radioisotopes and their applications.

Art Wahl sent me a note, dated August 14, saying that he believes Report A-33 is worth publishing, even with the deletion of the Pu^{239} slow neutron fission cross section. Wahl returned the document since he has no place to keep it.

Dr. C. Reid, in a letter dated August 15, wrote that he will arrive in Berkeley on September 16.

I also looked over a draft copy of a brochure on "The Peacetime Uses of Atomic Energy," sent by John Lewellen.

Another letter I read came from Robert Littell of The Reader's Digest. Littell said that it is very difficult to make such a complicated subject clear to the average fellow, and therefore he is not ready to publish a simplified version of my Pittsburgh talk.

My parents arrived by train to visit with their grandson, his parents, and their daughter Jeanette.

Tuesday, August 27, 1946

I acknowledged Donald A. McPherson's letter (August 6) of disappointment. I said that, although it seems unlikely, if we ever decide to write another book we shall contact him first.

Professor Claude Schwob of the Carnegie Institute of Technology asked, in a letter dated August 23, for slides or pictures of hot lab remote control equipment to be used for a talk to the Instrument Society. In my reply I said I do not have such slides but mention there are excellent pictures in the Life article "Plutonium Laboratory" and in a similar story in the July 4 issue of the Chicago Tribune. I also suggested that Schwob contact Dr. J. R. Coe at Clinton Laboratories.

I wrote to Thomas Larkin III, explaining that I have received my copy of my biographical sketch for "Who's Who in the Central States." However, I said, I have taken up permanent residence in Berkeley, California, and I presume that he will not want to include the biography in his book.

Dr. Robert M. Joyce, Chairman of the Program Committee of the Delaware Section of the ACS, asked that I address his section during the 1946-47 season. I declined the invitation, explained my heavy schedule, and suggested Winston Manning as a possible speaker.

I returned the copy of INDUSTRIA Y QUIMICA to Robert F. Gould, C & E News. Gould had sent it to me so that I could look at an abstract of one of my articles from C & E News that was published there in Spanish.

Another note went to James T. Grady, Managing Editor of ACS News Service, who had asked for an abstract of my paper scheduled for the Chicago meeting. I explained that the paper was scheduled and cancelled without my knowledge.

I mailed a check for ten dollars to Mrs. Dorothy C. Johnson, 8/27/46-2
Institute of Nuclear Studies, University of Chicago, to be applied to the Louis Slotin Memorial Award. This is in response to an August 15 request from Samuel K. Allison.

Oliver Grummitt, in a letter dated August 13, thanked me for my photograph and asked if I had any suggestions other than J. G. Hamilton

and E. Segrè (whom Ernest Lawrence had suggested) for a speaker on "Artificial Radioactivity." Today I wrote him to suggest the additional name of Iz Perlman.

Wednesday, August 28, 1946

I made the usual rounds of the laboratory rooms to check on the research progress of members of our group.

I responded to a letter, dated August 6 and received during my absence, from Frank H. Verhoek of the Columbus Section of the ACS. Verhoek asked me to speak at the May 1947 Section meeting. I explained that since I have resumed my teaching position and have already accepted some speaking engagements, I must decline. I suggested that he contact Dr. Winston Manning at Argonne National Laboratory.

Iz Perlman sent a memorandum to Russell Ball in the Area Engineer's office, requesting authorization for 15 bombardments at the 60-inch cyclotron to be completed over the next three or four months: thorium backing plate, 200 microampere-hours helium ions; 100 mg Pu^{239}, 3000 microampere-hours helium ions; 25 mg Pu^{239}, 60 microampere-hours helium ions; 25 mg Pu^{239}, 100 microampere-hours deuterons; U^{238} backing plate, 3000 microampere-hours helium ions; 2.5 mg Pa^{231}, 75 microampere-hours helium ions; 5 mg Am^{241}, 200 microampere-hours helium ions; 5 mg Am^{241}, 8 microampere-hours helium ions; 5 mg Am^{241}, 75 microampere-hours deuterons; 10 mg Np^{237}, 100 microampere-hours helium ions; 20 mg U^{233}, 100 microampere-hours deuterons; 100 mg Pr, 100 microampere-hours helium ions; 20 mg Pb^{204}, 100 microampere-hours helium ions; 20 mg Pb^{204}, 50 microampere-hours deuterons; 200 mg Tl, 50 microampere-hours deuterons.

Donald C. Stewart began working with our group today. Stewart, who was with us in Chicago, will also be a graduate student this fall.

A letter of agreement arrived for my signature from Leroy R. Goodrich of Frontier Press Co. I am to be paid $400 to revise the section on chemistry of the Lincoln Library of Essential Information by December 31. Iz Perlman has agreed to prepare the revision, and I will give him the honorarium.

Helen and I had dinner at the Perlmans' home, along with Harry (Harrison S.) and Adele Brown, Vance Cooper, and Spof (Spofford G.) English. [Harry was an undergraduate chemistry major at Berkeley before the war; he and Adele, who are visiting Berkeley, were friends of ours in Chicago where Harry worked in our Section C-I at the Met Lab. Vance, a UCLA classmate who now lives in Los Angeles, was also a member of Section C-I. Spof obtained his Ph.D. with me, accompanied me to the Met Lab, then transferred to Clinton Laboratories. He is now an Assistant Professor here in the Department of Chemistry and a member of our Nuclear Chemistry Division in the Radiation Laboratory.]

Thursday, August 29, 1946

I checked into administrative matters; then, in a memorandum to Kenneth Priestley, I requested that Edgar Westrum, who will leave in

September to go to the University of Michigan, be retained indefinitely as a consultant. I also asked that arrangements be made for his services for three months next summer.

I called Winston Manning and asked him several questions related to instruments. He said that he would have Jaffey answer them.

In a lengthy letter dated August 26, Truman Kohman suggested the desirability of a detailed and critical summary on "Radioactivity of the Heavy Elements," either as a separate volume or as a Chapter 15 in Volume 2 of the Plutonium Project Record. Truman also suggested that various persons should be asked to accept responsibility for individual elements or groups of elements--I should handle transuranic elements. Truman asked if it would be possible to get together with him some time later, in September.

Friday, August 30, 1946

A telegram arrived from Art Jaffey answering some of the questions I asked Manning yesterday: the 5% magnetic lens spectrometer is based on one described in Review of Scientific Instruments of 1944 but with improvements. The circuit of the low absorption coincidence counter is the same as the one described in a report by Bradley (Wendell H.) and Epstein; Ghiorso has the number of it. Bernard Weissbourd, who is away, has had no luck getting the crystal counter book duplicated. Westrum's sample yielded practically no x-ray diffraction pattern.

I phoned Truman Kohman to discuss some of the items he brought up in the letter I received yesterday.

I spent some time in my campus office going over lecture notes for Chemistry 123.

Saturday, August 31, 1946

Much of the morning was spent reading. Today's mail brought a response to my request to Kenneth Priestley--he prepared a consultant agreement for Westrum. This is subject to the Area Engineer's approval.

Elwin H. Covey, who worked for us in Chicago and then transferred to Oak Ridge, has arrived in Berkeley to start work in our group.

E. O. Lawrence talked with me about a conversation he had with L. P. Elliott of California Research Corporation. Elliott wants to send one of his men to Clinton Laboratories for training. Lawrence and I then phoned E. P. Wigner, Technical Director of the Clinton Laboratories, who agreed. Wigner does, however, want a first-class scientist.

Sunday, September 1, 1946

My parents left by train to return home to South Gate.

Much of my day was spent working on my talk entitled "Application of Artificial Radioactive Tracers to Chemistry and Medicine" for the Princeton University Bicentennial. I also prepared a summary to be typed and mailed to Eugene Wigner.

Monday, September 2, 1946 (Labor Day)

I worked for a while on the revisions to the "Table of Isotopes" that Iz and I are preparing. Later the two of us went to Mira Vista Country Club for a round of golf (IP-101, GTS-103).

Tuesday, September 3, 1946

In today's mail was a note from H. F. Beeghly of the Pittsburgh Section of the ACS. Beeghly sent some extra prints of a photograph of me remaining from the publicity of the talk on March 26.

I also received a lengthy August 30 letter from Truman Kohman. Kohman included a number of items about volumes of the Plutonium Project Record, the isotope chart, and the "Table of Isotopes," and said he will not be in Chicago when I visit. He suggested that we send photostatic copies of the table to him, Manning and Nathan Sugarman in Chicago, and to Paul W. Levy, Kay Way, W. H Sullivan, Ralph Overman, and Ray Stoughton at Clinton.

In addition, a copy of a letter arrived from William E. Elliott (University of California) to Francis W. Test (Patent Advisor at Argonne). Elliott answered a number of questions about Case S-52-D. [The cases S-52 (subdivisions A,B,C,D), which were filed on December 27 of last year, are the fundamental patents covering the discovery of plutonium that emphasize the chemical aspects. The inventors are listed S-52A--Kennedy, Seaborg, and Wahl; S-52B--G. T. Seaborg; S-52C--Kennedy and Wahl; S-52D--Arthur C. Wahl. There is another patent, case-61, filed December 11 of last year, in the names of Seaborg, Kennedy, Wahl, and Segrè, which covers the fission properties of Pu^{239} and its use as a source of energy in a chain reaction.]

William Shand, Jr., hired by Dean Latimer as an instructor in the Chemistry Department to set up x-ray facilities for our nuclear chemistry group to use for determining molecular structure, was unfortunately killed in an automobile accident before he could even set up a laboratory. We now have neither a research worker nor a laboratory. I have talked with Professor James D. McCullough (UCLA) about this; one possible solution is for McCullough to help us set up a lab while we try to recruit a qualified man. Today I wrote to Professor McCullough, suggesting that he spend a day or two in Berkeley in order to discuss his participation in our program of investigation of the heaviest elements along the lines of structure and formulae determinations by the x-ray method.

To Dr. E. P. Wigner, I mailed the summary of the address entitled

"Application of Artificial Tracers to Chemistry and Medicine" that I plan to give at Princeton:

The large number of artificial radioactive isotopes which are now available are finding and will find many applications in the field of chemistry, biochemistry, and medicine. There are about 450 induced radioactivities known at the present time, there being at least one radioactive isotope for each of the elements of atomic number 1 to 96 inclusive.

The work in connection with the Plutonium Project of the atomic energy development has resulted in the production, or possibility of production, of a considerable number of such isotopes. The chain-reacting uranium pile has given rise to vastly superior methods for the large scale production of a number of these isotopes and in particular a number of the most important ones.

In a recent announcement from the headquarters of the Manhattan Project it was disclosed that a large number of such radioisotopes are now available for distribution to research men through qualified research institutions. A complete description of the available isotopes, their method of production, and the form and quantity in which they are available, together with a description of the organization for their allocation and distribution has been published in a recent issue of Science magazine (June 14, 1946). 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 such radioisotopes rather than from the harnessing of the power itself.

In the near future there will also be available other machines which will give rise to other milestones in the fields of transmutation and artificial radioactivity. Among these are the betatron and synchrotron with which it will be possible to accelerate electrons to the energy region of hundreds of millions of electron volts and hence to produce electromagnetic radiation of this energy. Also, in a very short time the new 184-inch giant cyclotron at the University of California at Berkeley will be ready for operation. Using the new frequency modulation principle in order to compensate for the relativisitic increase in mass at these tremendous energies, this instrument will generate deuterons at 200 and helium ions at 400 million electron volts. The use of these instruments will undoubtedly add to the list of induced radioactivities.

One of the most important and interesting uses to which artificially induced radioactive isotopes have been put has been the investigation of the properties of those elements which do not exist in nature. The most spectacular examples of the application of the tracer technique lie in the discovery and the study of the chemical properties of the transuranium elements, of atomic numbers 93, 94, 95, and 96. There are also a number of lighter elements, namely those of atomic numbers 43, 61, 85, and 87, which are now known to be extremely rare or nonexistent in nature, but whose chemical properties have nevertheless been rather well defined as the result of their production by artificial means and the investigation of their properties by the tracer technique.

Perhaps the most extensive application of the artificial radioelements in chemical work has been to the study of "exchange reactions." Experiments of this type give information on chemical bond types, the strength and reactivity of chemical bonds and the effect of solvents on these properties, the structure of ions and compounds, the mechanism of reactions, and the mechanism of catalysis.

Probably the most important isotope from the standpoint of future possibilities is the radioactive C^{14}. This isotope opens a whole vista of opportunity in the fields of organic chemistry, biochemistry, and medicine as a result of its availability in large amounts by use of the chain-reacting piles as neutron sources. Another isotope which will have great importance in these fields and which will be available as the result of pile irradiations is the radioactive H^{3}.

Besides radioactive C^{14} and H^{3}, such isotopes as the 14.3-day P^{32}, the 180-day Ca^{45}, the 47-day Fe^{59}, the 250-day Zn^{65}, the 53-day Sr^{89}, the 8.0-day I^{131} and many others have offered and will offer many opportunities for important research.

One of the most important problems which has been and will be attacked by the use of tracer isotopes is the study of the process of photosynthesis. The elucidation of this process is of prime importance because all the life on earth depends on the synthesis by plant life of organic matter from the non-organic materials, carbon dioxide and water.

The use of radioisotopes in the study of plant and animal metabolism offers the greatest possibility in the understanding of life processes. Besides carbon and hydrogen, the radioactive isotopes of phosphorus, sulfur, iron, strontium, manganese, etc. find application here.

Another possible use of radioactive isotopes is, of course, in the study of cancer. Here, in addition to the possibilities for their use as indicators in the investigation of growth mechanisms, there is the therapeutic possibility of effecting the selective deposition of the radioactive material in the cancerous tissue.

The use of radioelements in the fields of therapy and diagnosis of disease show promise. The radioactive I^{131} has already found some therapeutic uses in certain cases of hyperthyroidism and diagnostic uses in certain types of thyroid disorder.

Another possibility in the field of medicine is the tagging of bacteria with a radioactive tracer such as C^{14} in the study of disease.

The present availability of large quantities of radioactivities has opened up the possibility of their use in industry and industrial processes.

The future seems to hold unlimited possibilities for the application

of these isotopes to scientific problems. It is certain that the applications which have been made so far are just the beginning of what is going to become an extremely large and fruitful field of research.

I also agreed to participate in a 15-minute radio discussion with Farrington Daniels, particularly if it can take place on Tuesday evening. I wrote to Daniels to send him the summary of my talk and to say that I am willing to participate in the radio discussion with him. Finally, I thanked him for his very laudatory letter of July 31.

I wrote to Professor Samuel C. Lind at the University of Minnesota, saying that I have noted that he too has been chosen as a member of the Commission of Radioactive Constants. I suggested that perhaps we can meet at the Chicago ACS meeting to discuss this.

Another letter went to L. P. Elliott of California Research Corporation. I confirmed Lawrence's conversation with him and our conversation with E. P. Wigner that Wigner felt that Clinton Laboratories could accommodate one of his first-class scientists.

I also wrote to Fritz Goro and told him that we were pleased with the Life "Plutonium Laboratory" story that appeared in the July 8 issue. I then mentioned that I probably would be in New York at the Commodore Hotel for the weekend of the 20th, and I would enjoy seeing him and Bob Campbell then.

Wednesday, September 4, 1946

Today's meeting, which was switched from Tuesday to Wednesday this week, was attended by Asprey, Bazell, Bockhop, Bradley, Connick, Eyring, Ghiorso, Goeckermann, Hopkins, Huffman, James, La Chapelle, Lilly, Lindner, Magnusson, Miller, Morgan, Newton, O'Connor, Perlman, Reynolds, Robinson, Seaborg, Simkin, Stewart, Templeton, R. Thompson, Werner, and Westrum. I announced that the next meeting will be on Tuesday, September 17, and that we shall have to arrange a different time after school starts in order to accommodate the class schedule of the graduate students. Covey, I said, is making a stock of pure chemicals and will try to include as many elements as possible. I also reported that a Pu^{239} target to be bombarded with helium ions will go on the 60-inch cyclotron at the end of the week and will be bombarded whenever possible, until possibly November 1. Perlman reported that we must now make written requests through the Area Engineer's office for bombardments (pile and cyclotron), spectrographic and x-ray analyses, and transportation by ATC.

Then Reynolds described the apparatus and methods he and Prescott plan to use to measure the ionization potentials of the heavy elements.

Templeton talked about his natural thallium plus helium ions bombardment in which there was a large yield of the 6.4-day Bi^{206} (EC) and a 13-hour activity (Bi^{205} or Bi^{204}), and a 10-18 hour activity (Bi^{206} ?). In the lead fraction a 68-minute activity was observed but no 52-hour activity. There was then a discussion about relative yields.

Westrum reported he has found a higher density for neptunium (19.6-19.7) than previously reported. He has measured the heat of solution of neptunium metal but hasn't finished the calculations. For the density of americium metal, he found a value of about 10-11, but the purity of the metal is in doubt.

* * * * *

E. W. Doty (Personnel Manager, Los Alamos) asked in a letter dated August 22 for an evaluation of Herbert H. Anderson. Today I dictated a reply, saying that it is difficult to write a letter of appraisal because Anderson is a shy, "lone-wolf" type of worker. I said that he has had much experience in the synthesis and identification of new inorganic compounds, particularly plutonium compounds. Because of this he may be useful in the Los Alamos research program.

Lorraine A. Johnson, who was hired as a clerk in our group to replace Margaret Lamm, began working today. [The work load for our office staff is quite heavy, particularly because of the required procedure for handling classified documents and mail. One of the fellows recently told me that, in Chicago, I was known as a "secretary killer."]

I received a copy of a memo from General Groves in which very strict rules are given for the procurement of any amount of top secret (Pu^{239}, U^{233}, and U^{235}) material. The rules are so strict that I feel they will hinder our research.

I made my usual rounds of the laboratories to check on the progress of the research.

I left San Francisco at 9 p.m. on UAL Flight 36 bound for Chicago, where I will consult at Argonne and attend meetings of the American Chemical Society.

Thursday, September 5, 1946

I arrived in Chicago at about 9:15 a.m. I went out to Argonne (University of Chicago campus) to talk with the men there. I got together with W. H Sullivan and others and spent much time going over, in great detail, the "Table of Isotopes." I saw Marty Studier, who gave me the latest values for the gamma-ray energies for U^{233}.

[In Berkeley, Mary Millard returned my temporary pass to Los Alamos--I neglected to return it when I left. Iz Perlman requested a 10 microampere-hour bombardment with helium ions on 25 mg BaO. The purpose is to identify rare earth activities probably encountered in some of our americium bombardments.

Delpha (Wollert) Chesterman, one of Helen's college roommates, visited her during the afternoon. Later Jo Owen came by, and she and Bee Barbachano had dinner with Helen.]

Friday, September 6, 1946

In Chicago. I spent a full day at Argonne, going over the status of

the papers for the Plutonium Project Record (PPR), and clearing up some questions Iz and I have about entries for the "Table of Isotopes." In addition, I talked with some of the men about personnel--in particular, we need a molecular structure man, a radiation chemist, and a spectroscopist in our Berkeley group. Darrell Osborne told me that Henri Levy knows x-ray work, and I talked with Levy, who showed some interest in Berkeley. I told him I will send him a PSQ. Aaron Novick will look for an expert in radiation chemistry for us, possibly Edward Shapiro, Melvin Bowman, or himself. Norman Nachtrieb suggested John G. Conway at the University of Pittsburgh as a capable spectrocopist. Paul Dana told me that Henry Faul is a good tinkerer but not an electronics man--Oliver Simpson said that Faul is good in mechanics.

[In Berkeley, a memo went out over my name to George Everson, in which the salary history of men in Chicago was compared with that of the men who came to Berkeley. The Chicago men received substantial raises this July but the Berkeley men did not, even though the ones brought to Berkeley are considerably better. Mary Millard also mailed to Willard L. Valentine (Editor of Science) my paper "The Transuranium Elements," which has finally been declassified.

Mary Ellen La Chapelle, Ted's wife, visited Helen today.]

Saturday, September 7, 1946

In Chicago. A telegram arrived from Eugene Wigner, again inviting me to participate in the CBS radio program "In My Opinion" at 6:15 p.m. on Tuesday, September 24, with Farrington Daniels. We are to discuss the application of artificial radioactive tracers to chemistry and medicine. I wired back my acceptance.

Winston Manning and I went to one of my favorite golf courses during my Chicago days--Timber Trails Golf Course--and played 18 holes of golf (WMM-113, GTS-110).

[In Berkeley, Helen had Jo Owen, Jo's sister Catherine, and Jo's mother come for dinner.]

Sunday, September 8, 1946

In Chicago. In the afternoon I went to the Exhibition Hall of the Palmer House and registered for the fall meeting of the American Chemical Society. I saw a number of friends also attending the meeting.

Monday, September 9, 1946

In Chicago. In the afternoon I went to the Red Lacquer Room of the Palmer House for a session of papers in the Division of Physical and Inorganic Chemistry. One of the papers was presented by T. Higuchi and J. E. Willard. At 8:30 p.m. I went to the general mixer in the Grand Ballroom of the Stevens Hotel and saw many familiar faces.

Tuesday, September 10, 1946

In Chicago. There were a number of interesting papers in the

sessions today. I listened to a couple of papers by Herbert C. Brown et al. on hydrides in Room 14 of the Palmer House and then went to the Red Lacquer Room, where I heard papers by Ralph T. Overman, Willard F. Libby, Lawrence M. Litz, and Bruce Longtin. In the afternoon I heard Kenneth S. Pitzer report on "The Protonated Double Bond Structure of the Boron Hydrides."

At 3 p.m. I went to a meeting of the ACS Beilstein-Gmelin Committee, also in the Palmer House. In attendance were Drs. Henry B. Hass, E. H. Huntress, C. S. "Speed" Marvel, and Aristid V. Grosse. Dr. Matthew W. Miller of the U. S. FIAT (Field Intelligence Agency Technical) in Germany, an arm of the U. S. Office of Technical Services whose objective is to gather, classify, and distribute all technical information from enemy countries, and Professor W. H. Haworth, President of the Chemical Society of London, were invited to attend. There was a discussion of the interest, also of the British Society, in the future of Beilstein and Gmelin. Professor Haworth suggested that contact between our committee and British FIAT, whose interest is in the Gmelin Redaktion, be established. Dr. Miller said that the U. S. FIAT is supporting the Beilstein Redaktion, supplying each member with one hot meal a day and helping them finish the Second Supplementary Volume Series. The Gmelin volumes formerly covered inorganic chemistry while the Beilstein covered organic chemistry, both in a comprehensive manner. Both were published in Germany and were discontinued when the war broke out; it is considered essential, in order for chemists throughout the world to stay abreast of advances in chemistry, that these compilations be rejuvenated.

[In Berkeley, Helen attended a luncheon for Rose Jura.]

Wednesday, September 11, 1946

In Chicago. I met with A. Foster York and Frank W. Test to discuss our plutonium patent claims and the relevant reduction to practice. They believe I should find out who reduced to practice making of these compounds: $Pu(NO_3)_4$ (work done before 1/44) (ask Cunningham), $Pu(OH)_3$ (before 6/44) (ask Cunningham), $PuBr_3$ (before 4/44) (they will contact Earl Hyde and Norm Davidson); these claims are to be included in patent case S-52B. I should then phone York about this. They suggested that Stan Thompson draft a better statement on the bismuth phosphate process and let them know by phone or during my November trip. In addition, they believe Iz should give up his claim on plutonium peroxide, and it should be included, instead, in one of my patents.

I left Chicago to return home at about 8:45 p.m. on UAL Flight 35.

[In Berkeley, Helen stopped by Lee Perlman's to return a book and then went to Wilma's to return a roll-away bed we borrowed during my parents' visit.]

Thursday, September 12, 1946

I arrived at Oakland Airport at about 6 a.m.

I checked in with Helen and then went to the laboratory, where I read a lengthy letter from Charles Coryell, who along with Samuel Siegel,

Larry Glendenin, and Nathan Sugarman, recently sent us the completed fission product tables. These, he said, have been submitted to the Journal of the American Chemical Society. Coryell also wrote about Paneth's request for information about the discovery and names for new elements in connection with an article he plans to write. Coryell suggested that several people should be consulted in each case--for element 61, for example, the names consulted should be Coryell, Nate Ballou, J. A. Marinsky, Glendenin, and possibly Dave (David N.) Hume and Waldo Cohn. Coryell also said that he believes "americum" is much more euphonious than "americium."

I also read a copy of a letter from Lavender to Kennedy (dated September 3), in which Lavender referred to Kennedy's letter of August 14 about our plutonium patent interests; he said that he is trying to arrive at a procedure that would protect the interest of all scientists engaged on the Project in regard to the reporting of inventions made and covered by the Atomic Energy Act of 1946. He pointed out that there is no Atomic Energy Commission formed yet under the Act and therefore no authorized agency to accept the reports.

I talked with some of the men about the progress of their research. I was particularly interested in the plans that Stan and Burris have for handling the 2 mg sample of Am^{241} being neutron irradiated at Hanford and scheduled to arrive in about a week.

Friday, September 13, 1946

I chatted with a couple of fellows and then handled some correspondence. A note went to John E. Pfeiffer of CBS, saying that I will be staying at the Commodore Hotel in New York and will be pleased to have lunch with him on September 21.

In a memorandum to George Everson, I recommended a merit raise of $20/month for Mary Millard.

Stan and Burris prepared a rather detailed memo about their plans for the neutron-irradiated americium sample. They plan to separate americium and curium and then subject the curium fraction to procedures designed to detect element 97 (by the oxidation of 97 to the +4 state and then coprecipitating it with zirconium phosphate).

I read a reply from James McCullough, who is interested in our program but will not have usable diffraction equipment before four to six months. I called him to suggest that he come up to the Radiation Laboratory to do some work here with us on a per diem basis; I learned that his salary is $4800/academic year. I then phoned Latimer, who suggested a 12/9 factor, i.e., a rate of $6400/year; George Everson suggested a pay rate of $600/month.

Later I went to my campus office to work on my notes for Chemistry 123.

Saturday, September 14, 1946

In today's mail was a thank-you note arrived from E. W. Doty for my

critique of Herbert H. Anderson. I also received a thank-you note from Dorothy C. Johnson for my contribution to the Slotin Fund.

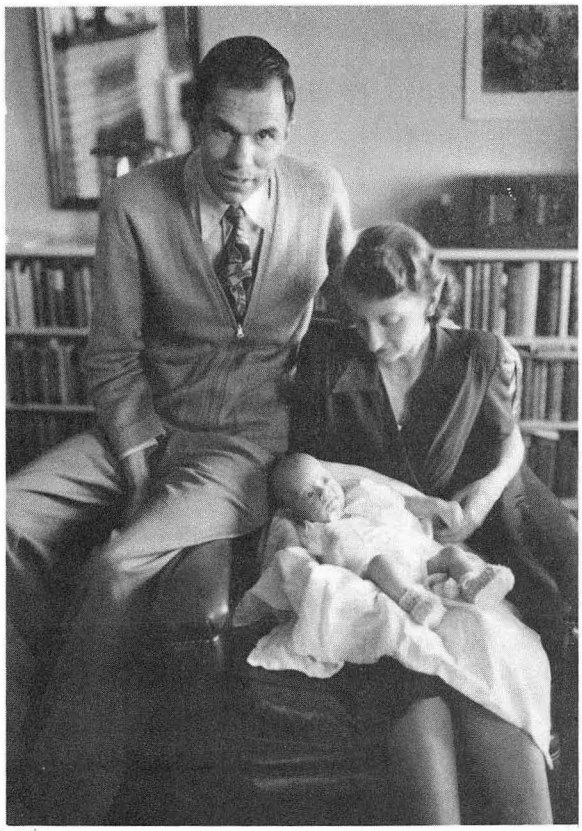
At 11 a.m. I, along with E. M. McMillan, R. L. Thornton, J. G. Hamilton, R. Serber, M. Calvin, J. H. Lawrence, D. Cooksey, L. W. Alvarez, W. M. Brobeck, I. Perlman, R. Connick, R. W. Barton, N. Garden, R. Loevinger, B. J. Moyer, W. B. Reynolds, H. York, and R. Hildebrand attended a meeting in the Radiation Laboratory Conference Room that Ernest Lawrence called to discuss the health program. He has in mind, among other things, the protection needed when the 184-inch cyclotron begins to operate. He appointed a committee consisting of himself, J. H. Lawrence, R. L. Thornton, E. M. McMillan, G. T. Seaborg, L. W. Alvarez, R. Serber, J. G. Hamilton, M. Calvin, and some others to be responsible for the health of the personnel. Another group under Burton Moyer will monitor radiation, develop radiation measuring instruments, operate an assay laboratory to determine the strength of radioactive samples, develop shield and shielding methods, and monitor the operation of the isotope separation program. The importance of the provision of safe equipment, even at considerable expense, was emphasized. Joe Hamilton suggested that 15 minutes of the Thursday afternoon meeting (the weekly lab seminar) be spent in the discussion of the health physics problems.

Sunday, September 15, 1946

I spent part of the day going over my Princeton talk and putting new information into our "Table of Isotopes" and chart of isotopes.

Herman Robinson came by later in the afternoon and took some pictures.

Peter Seaborg
September 15, 1946
836 Washington St.
Albany, California



Glenn, Peter, and Helen Seaborg
September 15, 1946

Jeanette Seaborg holding
Peter Seaborg
September 15, 1946

Monday, September 16, 1946

Dr. C. Reid, who has been awarded a Commonwealth Fellowship, arrived in Berkeley from Chalk River.

In handling my correspondence today I replied to a September 16 letter from Clifford Garner, now at UCLA. Garner asked for reprints of some of my papers. I suggested a regular exchange of reprints. I also mentioned that I am preparing my Harrison Howe lecture and want to refer to our observation of plutonium in carnotite if it is all right with him.

I noted a letter from Burt S. Kinsella (President of The Frontier Press company) saying that he approves of my agreement with L. R. Goodrich for the revision of the chemistry section in The Lincoln Library of Essential Information.

Harry G. Cisin of Electronic Exhibitors wrote that he appreciated the honor conferred on them by my membership on their Advisory Committee, but unforeseen production difficulties have made it necessary to cancel their planned exposition. Therefore they will not continue to use my name.

John Pfeiffer wired me that my hotel reservation in New York is ok

and that he is looking forward to lunch with me on Saturday.

I wrote a letter of recommendation to the University of Cincinnati for Sigfred Peterson, who is applying for a position there. I said that I think so highly of Peterson that I offered him a position with my group in Berkeley and the opportunity to work here for his Ph.D. However, I said his personality is distinctive and different, so that it might be worthwhile to interview him before offering him a position.

Another letter went to Ray Stoughton at Clinton Laboratories. I said that we shall put him on the mailing list for the minutes of our biweekly planning meetings. I asked Stoughton to check to see if we are on the mailing list for their monthly reports. Then I mentioned that Iz Perlman is going to the meetings at Clinton next month.

Tuesday, September 17, 1946

As usual I conducted this morning's meeting (now biweekly) of our group. Also attending were: Beaufait, Blaedel, Bockhop, Bradley, Covey, Cunningham, Garden, Ghiorso, Hicks, Howland, Huffman, James, La Chapelle, Lilly, Lindner, Magnusson, Miller, Morgan, Newton, O'Connor, Perlman, Prescott, Reynolds, Robinson, Templeton, R. C. Thompson, and S. G. Thompson. Perlman began with a few announcements and a few words about the health situation. He mentioned that we are going to request more blood counts and urinalyses.

Blaedel reported that when the americium metal was pressed in a pellet press, it still had the same density of 10 to 11. He and Westrum have made a large number of very small protactinium metal pellets and will try to make a larger pellet. In addition, they plan to work on neptunium metal. I commented that the low density of americium metal is interesting because of the analogy with europium. I said the atomic radii in the rare earths follow a systematic decrease with increasing atomic weight, except that europium is abnormally high; this, we believe, is due to the outer two electron configuration similar to the barium structure.

Stan Thompson described the chemistry and results of the helium ion bombardment of Am^{241}. An element 97 fraction was isolated by oxidation and carrying by zirconium phosphate; no long range alpha particles were found--only beta particles, probably due to 17-hour Zr^{97}. He concluded that the half-life of 97^{242}, if due to short-lived K electron-capture, is less than two hours. I commented that there is very little probability that this particular bombardment will give a 97 isotope of long enough half-life to be detected.

James reported on the possible observation of Np^{232} from a bombardment of U^{233} with deuterons. He found, in the neptunium fraction, a small amount of alpha-particle growth from a 2.3-day parent, which he interprets as Np^{232} decaying by K electron capture to U^{232}.

Perlman then reported that Werner is working on the higher oxidation state of americium and that Lindner and Goeckermann have bombarded barium with helium ions. They found a new lanthanum positron activity of 90-minute half-life and a cerium K electron-capture activity of 36-hour

half-life.

* * * * *

I read Amos Newton's report to C. E. K. Mees about the work of the Berkeley chemistry group for the period August 1 to August 30, 1946. Newton reported on the work to clarify the situation of the assignments of the isotopes of bismuth, lead, and polonium formed by cyclotron bombardments. He described the bombardment of U^{233} with 22 Mev deuterons and the bombardment of Np^{237} with helium ions (a new 34-hour activity was observed in the americium fraction). Newton described the work on the transuranic metals and the preparation by vacuum sputtering to produce thin (0.02 mils) of beryllium foils. Finally, he gave a summary of the slow neutron fission cross sections of some heavy isotopes (Ghiorso, James, and Chicago work recently done in Chicago).

When I went home today, Helen reported that she gave Pete his first cereal this morning--a rather messy operation.

Wednesday, September 18, 1946

The first thing this morning I responded to a telegram that arrived yesterday from Bob Campbell: I said that I will be in New York on Friday, Saturday, and Sunday for APS meetings but I will have only Saturday dinner, Sunday, and possibly early Wednesday evening free to see him and Fritz Goro.

I telephoned Stuart W. Scott, a patent attorney in Oak Ridge who has been recommended to us (Segrè, Kennedy, and Wahl) to handle our claim to the government for compensation for our plutonium patents. I explained our problem to him, and he said that he could see no reason why he could not handle the matter. In the meantime Scott will think about our questions: the question of our agreement between the inventors, the question of the future Atomic Energy Commission and what we should do to pursue our claim, and the question of foreign rights. Scott suggested that we eventually consult a tax expert. I then tried to call Kennedy, but I could not reach him; so I wrote him a letter to explain what had transpired. I said I will send Scott copies of the agreement between the inventors, the Chicago memo of January 17, 1945 (describing the preliminary conference between Anderson, Kennedy, and Seaborg), and the agreement between the inventors and the Government.

I then had several photostatic copies of the "Table of Isotopes" mailed out, requesting additions and corrections and saying that we want to give wider distribution to the secret version soon. The copies went to Maurice Goldhaber, Anthony Turkevich, Charles Coryell, W. H Sullivan (to whom I explained that Perlman will be at Clinton at the time of the meetings in October), and Truman Kohman. I answered for Kohman several questions that he asked about the Chart of Isotopes earlier.

In today's mail was a notice of the filing of patent application case S-834 (August 16) from Frank W. Test of the Chicago Patent Group.

Professor Lawrence sent out a memorandum from Earl R. Miller, who is in charge of the health physics group. Miller clarified the

responsibilities of the health physics group and the experimental group leaders for the safe handling of radioactive material.

I sent a note to the District Engineer at Oak Ridge, requesting reservations at the Guest House and transportation from Knoxville for Amos Newton, who will arrive at 9:19 a.m. on October 14 and will leave at 8:39 p.m. on the 18th. Amos will attend the General Information Meeting there that Iz Perlman is attending.

Much of the rest of the day was spent talking with the fellows about the research.

Thursday, September 19, 1946

This morning, after taking care of some administrative matters, I wrote to French T. Hagemann (Pete) at Argonne and asked whether I have all the latest corrections in the paper on the decay products of 23 since we want to include them in the "Table of Isotopes."

Later, down on campus, I stopped in the Department office and spoke with Miss Kittredge, et al. I also talked with Dr. Geoffrey Wilkinson who has arrived in Berkeley to work with our group. Wilkinson did graduate research with H. J. Emeléus at the Imperial College in London and spent the war years at Chalk River, where he became an expert in fission product chemistry. Since he can not now obtain a clearance to work in Bldg. 4, he will work in a laboratory on the third floor of Gilman Hall and use the detection equipment in our counting room there (Room 303).

George Manov, who was a fellow graduate student here in Berkeley a some ten years ago, is vacationing in California. Today we met to reminisce--Manov told me that he would like to change fields and perhaps get into nuclear work. I encouraged him to do so.

A. H. Bazell terminated today to return to school.

I left San Francisco via UAL Flight 4 at 5:30 p.m. to fly to New York.

Friday, September 20, 1946

I arrived in a hot and muggy New York at about 8 a.m. and checked in at the Hotel Commodore (42nd St. and Lexington Ave.) ($4.00/night). I registered at the Engineering Societies Bldg. (29 West 39 St.) for the American Physical Society meetings. Most of the meetings in which I am interested take place tomorrow (yesterday's meetings were devoted to cosmic ray and subnucleonic physics). However, I went to a few talks and met some of the participants. Maurice Goldhaber told me that Bernard Waldman can't find the 1.6-minute lead using x-rays. I learned that Wayne R. Arnold and Arthur Roberts of Argonne have measured the magnetic moments of the proton, neutron, and the deuteron. This work will be published soon.

The APS dinner this evening was held in the Mens' Faculty Club at Columbia University; the speaker was P. M. S. Blackett, who spoke on "The Tradition of Science."

Saturday, September 21, 1946

In New York. I attended sessions of the APS meetings and heard a number of interesting papers: "An Attempt to Observe the Radioactivity of the Neutron" (Erwin F. Shrader, David S. Saxon, and Arthur H. Snell), "The Mass of the Mesotron as Determined by Cosmic-Ray Measurements" (Donald J. Hughes), "Mass of Cosmic-Ray Mesotrons" (William B. Fretter), "Measurement of Meson Masses by the Method of Elastic Collision. Probable Existence of a Heavy Meson (1000 m_o) in the Cosmic Radiation" (L. LePrince-Ringuet).

I then had lunch with John E. Pfeiffer of CBS to discuss the radio program on which I am to participate at Princeton next Tuesday.

Afterwards I returned to the APS meetings and heard Luis Alvarez talk on "The Design of a Proton Linear Accelerator," Edwin McMillan speak on "Resonance Acceleration of Charged Particles," and H. R. Crane speak on "The Racetrack." At 4:15 p.m. I attended a discussion meeting under the leadership of C. G. Suits on "Relative Advantages of Proton and Electron Accelerators."

[In Berkeley, Mary Millard mailed Stuart Scott two copies each of (1) the agreement between the inventors for the plutonium patents; (2) the Chicago memo of January 17, 1945, describing the preliminary conference between Roland Anderson, Kennedy, and Seaborg; and (3) the agreement between the inventors and the Government.]

Sunday, September 22, 1946

In New York. Shortly before noon I telephoned Helen in Albany (California) to hear about the status of things at home and about Peter's activities. I also described some of the things I have been doing and the people I have seen.

I then checked out of the hotel and then took a train to Princeton, New Jersey, to attend and speak at the Princeton University Bicentennial, "The Future of Nuclear Science." The attendees are staying at the Graduate College.

Monday, September 23, 1946

In Princeton. The topic today was "Cooperation of Universities with other Institutions in Science," with Luther P. Eisenhart as chairman. During the morning Frank Spaulding spoke on "Scholarship in the Secondary School" with John H. Bosshart as discussion leader and Isidor I. Rabi spoke on "The Relation of Research in Universities to Government and Commercial Laboratories," with Edward U. Condon as the discussion leader.

At 4:30 p.m. Lee A. DuBridge spoke on "The Place of the Large Laboratory in Nuclear Research" (discussion leader: Charles A. Thomas). The evening program was "On Problems of Scientific Cooperation: The Uses and Hopes of Scientific Societies" with Harlow Shapley as the speaker and Harold C. Urey as the discussion leader. This program was held in the Graduate College.

[In Berkeley, Mildred G. Jentsch was hired to replace A. Bratenahl as a chemist in Huffman's analytical group. Classes started at the University today.

Helen had dinner at the Owens'.]

Tuesday, September 24, 1946

In Princeton. This morning's topic was "Nuclear Physics" with Rudolf W. Ladenburg as chairman. P(aul) A. M. Dirac spoke on "Elementary Particles" with Richard P. Feynman as discussion leader. The second speaker was Robert R. Wilson on "Proton Scattering" with Victor Weisskopf as discussion leader. At 3:00 p.m. I spoke on "Application of Artificial Radioactive Tracers to Chemistry and Medicine." Farrington Daniels was the discussion leader. Ernest Lawrence spoke at 4:30 p.m. on "Experimental Methods of Nuclear Physics" with Enrico Fermi as discussion leader.

Then at 6:15 p.m. I took part in a fifteen-minute CBS radio broadcast "In My Opinion" with Farrington Daniels and John Pfeiffer. The subject was "Application of Artificial Radioactive Tracers to Chemistry and Medicine"; it was part of the "Frontiers in Science" series.

The evening session was held in Frick Chemical Laboratory, and Niels Bohr spoke on "The Observation Problem in Atomic Physics." Henry D. Smyth was chairman for this session.

[In Berkeley, Iz Perlman enrolled the students in our nuclear chemistry class (Chemistry 123) at 10:10 a.m. in Room 219, Gilman Hall and gave the students an outline of the course.]

Wednesday, September 25, 1946

In Princeton. This morning's session was entitled "Physical Science and Human Values" with Hugh S. Taylor as chairman. F(ilmer) S. C. Northrup spoke on "The Physical Sciences and their Bearing on Philosophy and Human Values" [S(ubrahmanyan) Chandrasekhar, discussion leader]. I did not attend the afternoon nor evening sessions since I need to return to Berkeley. It has been an interesting meeting, and I have talked with a number of the attendees. I met Frederic Joliot, also a member of the Commission of Radioactive Constants who told me that the International Union of Chemistry will pay my travel expenses to next year's London meeting, and also Irene Joliot. C. Guy Suits, Vice President and Director of Research of General Electric Company in Schenectady, described for me a "G. E. Nucleonics Project Consultant Committee" for work under their Manhattan District Contract covering the operation of Hanford and the atomic power development at Schenectady. Suits asked me to be a member of this committee. Suits is also interested in finding some high quality men for their research program. I met Dr. W. Bennett Lewis, who is the new director at Chalk River; I asked him to help me find a counting instruments expert for our Berkeley research group. I met Professor Homi J. Bhabha, Tata Institute of Fundamental Research, Bombay, who asked for a copy of the 1944 "Table of Isotopes" and to be placed on our mailing list. Harold Urey asked that I send him a photograph of G. N. Lewis. I was also requested to send a copy of my

Princeton talk to William L. Laurence of The New York Times and to Dennis Flanagan of Life magazine.

I took the train to New York. Robert Littell (Reader's Digest) met me. He took me to the Century Club where we had dinner with Lloyd Garrison. Littell talked about the difficulties in making the subject of nuclear science understandable to the "man on the street." I promised to send him a copy of my Princeton address and some other reprints. About 8 p.m. I boarded UAL Flight 3 bound for home.

[In Berkeley, the americium that has been neutron irradiated at Hanford arrived.

Helen spent the afternoon with Ann Miller, a member of Alta Bates Hospital staff, whom Helen met while she was hospitalized.]

Thursday, September 26, 1946

I arrived in San Francisco at about 7:20 a.m.

At 10:10 a.m. Iz and I met with our Chem 123 class. I gave the class some historical background of the field. Members of the class include W. C. Adams, Miss A. M. Anderson, F. Asaro, L. B. Asprey (G), H. D. Baldridge (G), D. R. Bomberger (G), K. Chivukula (G), D. Christensen (G), M. Chun, J. G. Claypool (G), J. W. Cobble (G), C. C. Delwiche (G), E. L. Dobson (G), A. K. Dunlop, E. Epstein (G), W. H. Eustis (G), R. C. Feber (G), S. P. Gessel (G), P. W. Gilles (G), D. L. Grunes (G), J. H. Guill, Miss C. R. Hawkins, H. G. Hicks (G), R. House (G), Z. Z. Hugus (G), M. A. Islam (G), W. T. Jackson, J. L. Jungnickel (G), R. M. Kallo (G), Mrs. L. S. Kelly (G), R. R. Kitson, J. H. Klinger, J. R. Lai, E. H. Lambert, H. C. Lanz (G), Mrs. H. M. L. Maas, R. L. McKisson, K. L. Mattern (G), Miss H. J. Maxwell, D. R. Miller (G), Miss D. M. Nelson, T. W. Newton (G), D. A. Orth, C. Rosenthal, M. E. Rozen, V. A. Rundle, R. K. Sheline (G), Charles Selvi, D. C. Stewart (G), P. R. Stinchfield (G), B. Suverkrop (G), M. W. Taft, E. Taleck, L. A. Walker (G), A. D. Webb (G), J. W. Weigl (G), A. R. Weiss, W. Weltner (G), R. E. Weston (G), R. W. Wheatcroft (G), B. Williams, Miss I. M. Wilsey, D. M. Woodbury (G). Auditors include M. Azima, W. P. Ball, R. H. Busey, J. H. Eldridge, L. Eyring, L. S. Gilman, R. G. Horvat, Miss D. M. Johnson, W. H. McVey, R. V. Nauman, E. W. Putnam, J. E. Ryus, R. C. Sill, Miss F. J. Spieth, M. Tanabe, S. G. Thompson (G), B. M. Tolbert, P. E. Yankwich (G), and D. W. Yep (G indicates graduate student).

At the laboratory I noted a long letter from W. H Sullivan, summarizing the questions and corrections to the "Table of Isotopes" that we discussed in Chicago earlier this month.

I read an acknowledgement of the receipt of "The Transuranium Elements" from Willard Valentine of Science. Valentine stated that, although it is longer than he expected, he feels it is imperative to publish it. He also informed me that it will be several weeks before we receive the galley proof.

I found a nice reply from Professor S. C. Lind, who was disappointed that he did not see me in Chicago. Lind said that formerly the

Commission on Radioactive Constants had no travel funds for members to attend meetings. He reported that it seemed to be the opinion from members attending the Board of Directors' meeting of ACS that eventually the State Department or some such agency will have funds for such a purpose. He also intends to write to the National Research Council and to the National Academy of Sciences to see if any funds can be made available. Lind went on to describe the status of Stefan Meyer, also a member of the committee. Meyer has again assumed the direction of the Institute of Radium Research in Vienna but goes to Vienna only occasionally because his home was destroyed and, when he does go, he stays in one room at the Institute where his daughter cooks their meals over a bunsen burner. His radium standards were recovered by our army and returned to the Institute. Lind also said that they would be glad if I could visit Minneapolis and lecture although they can contribute only $50 toward my expenses.

Ralph James wrote to Earl Hyde on September 21 to give him a report on the U^{235} target bombarded with deuterons, which the two of them worked up. I read the letter today. James stated that the most probable explanation for their data is that Np^{232} decays with a 2.3-day half-life to U^{232} and the long-lived growth they observed is RdTh and its daughters growing from the U^{232}. The Geiger-Müller counter decay of this neptunium fraction can now be resolved into two very good components of 2.3 days and 4.5 days. The x-ray and gamma-ray decays of the neptunium fraction were straight 4.5-day lines. James went on to say the GM decay of the protactinium fraction shows a half-life of 8.0 days. He also stated that Earl might separate a neptunium fraction, then milk the uranium daughter from it, using the deuteron-bombarded U^{235} that he will soon receive.

I also noted a memorandum from Kenneth Priestley. Priestley enclosed a letter from W. B. Harrell (University of Chicago) giving authorization for our photostating Metallurgical Laboratory notebooks.

Friday, September 27, 1946

I checked on the progress of the research in the various laboratories.

I read the following from Irene Joliot-Curie:

PRÉSIDENCE
DU
GOUVERNEMENT PROVISOIRE

COMMISSARIAT
A
L'ÉNERGIE ATOMIQUE
41, AVENUE FOCH (XVI)

TÉL. : PASSY 56-36, 56-51
56-68, 56-79

RÉPUBLIQUE FRANÇAISE

Noted 9/38 9/27/46
Action
Recd. SEP 19 1946 GTS
Ans.
File Joliot-Curie

C.R. manning 9/25/46

PARIS. LE 13 Septembre 1946.

Professeur G.T. SEABORG
Radiation Laboratory
University of California
BERKELEY.
(California)

Cher Professeur Seaborg,

Nous avons été très touchés de savoir que vous avez nommé l'élément 96 d'après le nom de mes parents. Nous serons heureux quand il sera possible de connaître les beaux travaux qui vous ont conduit à préparer ces corps nouveaux.

Je ne vous avais pas écrit jusqu'ici à ce sujet, parce que pendant longtemps nous n'avons entendu parler de ces nouveaux corps que par les journaux, qui ne sont pas un élément d'information très sûr en matière scientifique.

Mon mari espérait vous voir pendant son séjour aux Etats Unis, mais il a été obligé de partir brusquement et n'a pas pu aller à Berkeley. Nous espérons pouvoir y aller maintenant ou avoir le plaisir de vous rencontrer à la réunion de Princeton.

Recevez, je vous prie, cher Professeur Seaborg, mes cordiales salutations et celles de mon mari.

F Joliot-Curie

I. JOLIOT-CURIE.

(translation):
Dear Professor Seaborg:

We were quite touched to learn that you named element 96 after my

parents. We shall be happy when it is possible to know about the wonderful work which you carried out in order to prepare these new elements.

I have not written you before this on this subject because, for a long time, we have heard about these new elements only through the newspapers, which are not a very reliable source of information on scientific matters.

My husband hoped to see you during his stay in the United States, but he had to leave suddenly and was not able to go to Berkeley. We hope to be able to go there now and to have the pleasure of meeting you again at the Princeton meeting.

Dear Professor Seaborg, I beg you to receive the cordial greetings of myself and my husband.

I. JOLIOT-CURIE

I recently had a conversation with Dr. Joseph W. Weinberg and agreed to give a lecture on "Chemical Aspects of Atomic Energy" for a series at University Extension. Today I received a confirmatory letter and description of the series from Irene L. Hofmeister, Organizer.

Notice arrived from Frank W. Test of the Chicago Patent Group of the filing (August 23) of patent applications S-1725, "Element 95 and Method of Producing Said Element," by Glenn T. Seaborg, and S-2974, "Methods of Preparation of Element 95," by Glenn T. Seaborg and Ralph A. James.

Saturday, September 28, 1946

I wrote a note to Professor John T. Tate to explain the changes and authors of our paper "Properties of 94-239."

Wisconsin defeated California, 28 to 7, in the football game Helen and I saw in Memorial Stadium this afternoon.

Sunday, September 29, 1946

Part of my day was spent working on my lecture notes for Chemistry 123 and on the "Table of Isotopes." I also made a few revisions to the text of my talk, "Applications of Artificial Radioactive Tracers to Chemistry and Medicine," at Princeton to make it more publishable.

Jo Owen came to visit, and then Al Ghiorso and Kristine stopped by to announce the birth of Kristine's brother Bill.

Since I appeared on the "Quiz Kids" show last November and February, Helen and I have more or less regularly listened to the program on Sunday nights. Recently I submitted a question (if the question is missed, the sender wins a prize), asking the participants to name an element named after a state, a country, and a continent. Helen and I were surprised to hear the question used and even more surprised when Ruthie Duskin missed

the answer. She gave illinium for the state, americium and europium for the continent and then indium and samarium for the country. Admittedly, indium and samarium sound as though they were named after countries.

Monday, September 30, 1946

This morning there were a couple of administrative matters to handle, and then I answered a September 20 letter from James T. Grady (Managing Editor, ACS News Service) and explained that "Plutonium and Nuclear Energy," which I am to present to the California Section on October 14, is essentially the same as my talk before the Chicago Section last February. I told Grady that I shall send him a copy of my Harrison Howe Lecture, "Plutonium and Other Transuranium Elements," which will consist almost entirely of new material, if I succeed in obtaining clearance in time to do so.

A thank-you letter arrived today from W. H Sullivan for the photostat of the "Table of Isotopes."

Later I went down to my campus office and looked over my lecture notes for Chemistry 123.

Tuesday, October 1, 1946

After taking care of a few things on the hill, I went to campus and began a discussion of the fundamental properties of the nucleus in Chemistry 123.

Back on the hill, I checked on the research and took care of some correspondence. In response to a request from Otto M. Smith of Oklahoma A&M, I wrote a letter of appraisal for Herbert H. Anderson, similar to another I recently wrote.

A note arrived last week from Hoylande D. Young, in which she said that negatives of our old isotope chart have been returned to her from General Electric. She asked whether I wanted them filed at Argonne or sent to me. Today I requested that she send them to me. I also mentioned that I hope that she will guide my Harrison Howe Lecture "Plutonium and Other Transuranium Elements," which she should have received by now, through the declassification channels.

I answered a September 27 note from Ellsworth C. Dougherty, a research fellow in zoology at the Donner Laboratory on campus. I said that he is correct that the names masurium, illinium, alabamine, and virginium will all be changed, that the 2.5-hour Dy^{165} is well known and the list he referred to merely meant that it can be produced on the project, and that the new artificial isotopes of polonium have not yet been published in the open literature.

I wrote a note to Truman Kohman, saying that I agree with him and accept his recommendation for a deadline of November 1 for Volume 14B and December 1 for Volume 14A of the Plutonium Project Record. I then told him that I will not go to the meetings at Clinton but that Iz Perlman is looking forward to seeing him there to discuss the "Table."

In addition, I prepared an abstract for my Harrison Howe Lecture, "Plutonium and Other Transuranium Elements."

In the evening I went to the Research Conference in Room 102, Gilman Hall.

Wednesday, October 2, 1946

I mailed to Dr. Jacob Sacks (Director of Pharmacological Laboratories, Richmond Hill, New York) a copy of my Princeton talk. I explained that it was prepared for oral presentation but now may be published in their proceedings; however, I feel that it emphasizes the work here too much. Since Sacks is preparing a review article on the use of radioactive isotopes in biology, I asked him to look it over and make it a little more complete with respect to non-Berkeley work.

The abstract of my Harrison Howe Lecture "Plutonium and Other Transuranium Elements" was mailed to Professor Edwin O. Wiig at the University of Rochester:

> Since 1940 the four transuranium elements immediately following element 92 (uranium), namely, element 93 (neptunium), element 94

(plutonium), element 95 (americium), and element 96 (curium), have been discovered as a result of their synthesis by transmutation reactions starting with uranium as the primary material. Of these four transuranium elements, plutonium has assumed the position of dominating importance because of its very successful use as the explosive ingredient in the atomic bomb, and of the excellent prospects which it offers as the base material for the development of an atomic energy industry. Plutonium is the only transuranium element for which methods have been developed for production in relatively large amounts, that is, kilogram amounts.

From a purely scientific point of view, however, the other transuranium elements are of nearly as great interest as plutonium. This is particularly true since the whole group of heaviest elements including those immediately below uranium as well as the four known transuranium elements and a number of as yet undiscovered elements beyond curium, are all members of a transition group. This makes the chemical and physical properties of each of the elements of the whole group of comparable interest. Also, the radioactive properties of the new isotopes in this region contribute greatly to our knowledge of the nuclear properties of heavy radioactive isotopes, and knowledge of the nature of the regularities in these properties contribute to our understanding of nuclear structure.

The present discussion will therefore be concerned with all four of the known transuranium elements, neptunium, plutonium, americium and curium.

Neptunium has an isotope, Np^{237}, with a very long half-life, namely 2.25×10^6 years, which is produced during the operation of the uranium chain-reacting units. This makes it possible to study neptunium in the macroscopic state, that is, by the methods of ordinary chemistry. The discussion includes a description of some of the results that have been obtained as a result of work with this isotope. In the case of plutonium, of course, work on an ordinary scale has been possible by use of the well-known isotope Pu^{239}.

In the case of the elements americium and curium a great deal of investigation on the tracer scale has been carried out. A description of some of this work as well as some nuclear reactions by which isotopes of these elements are produced is included in the discussion. Some attention is also given to the prospects for the isolation of these elements in the pure state.

There is also a discussion of the problem of the existence of the transuranium elements in nature. One of these elements, plutonium, has been found to exist in very small amounts in natural ores, and a discussion of these experiments and the explanation for its existence in these ores is given. Consideration is given to the prospects for the existence of the other transuranium elements in nature.

In my covering letter I told Wiig that I shall arrive from Chicago on NYC Train No. 40 at 11:51 a.m. on Monday, November 18, and will leave on NYC Train No. 65 to Chicago at 10:15 p.m. on Tuesday, November 19. I asked for a reservation in a hotel for the afternoon and night of November 18.

Emilio Segrè gave me the numbers of several Los Alamos reports of interest to our "Table of Isotopes."

I spent some time in my campus office going over my notes for tomorrow's Chemistry 123 lecture.

In the evening Helen and I went to the Curran Theater (445 Geary, San Francisco) for a performance of "State of the Union" with Conrad Nagel, Irene Hervey, and Henry O'Neill.

Thursday, October 3, 1946

On campus this morning I lectured to the Chemistry 123 class at 10:10 a.m. and continued the discussion of the basic nuclear properties.

Later I read Pete Hagemann's latest, and what he hopes is a final, summary of the data on the new isotopes in the 4n + 1 paper.

A nice note arrived from Eugene Wigner, who said, "Your review of the future uses of radioactive tracers was beyond all expectations that we had. It was one of the most successful papers of the whole conference and we feel very much indebted to you for having made such a signal contribution." Wigner also asked that I send the full text to Professors D. Hamilton and G. Reynolds, who may edit a little book of the conference.

Iz Perlman requested of the Area Engineer a 10 μah bombardment of 25 mg Cs_2O with helium ions on the 60-inch cyclotron for the purpose of identifying rare earth activities probably encountered in some of the americium bombardments.

I learned that David Templeton passed his prelims today; his committee consisted of William F. Giauque, Melvin Calvin, Axel R. Olson, George E. Gibson, L. H. Duschak, and Charles B. Morrey.

Jeanette left today for a visit with our parents in South Gate.

Friday, October 4, 1946

This morning I answered a letter from A. P. Weber (The Kellex Corporation), saying that I know of no radiochemists who might be interested in a position on his staff. Such men, I said, are in short supply.

In reply to a letter of September 27, I wrote to Professor Hans A. Bethe, giving him permission to use parts of the "Table of Isotopes" in his book on elementary nuclear theory.

I also replied to another note from Ellsworth C. Dougherty. I said that the best known radioactive isotope of gadolinium is the one with a half-life of about eight hours listed; this apparently has been confirmed by secret work on the Manhattan Project, but the information is not at his disposal until it is declassified. I again stated that there are no "open literature" artificially produced radioactive isotopes of elements 86, 87, 88, and 89.

Earlier this week I was interviewed by Jack Bik '49, a reporter for The Daily Californian. Today the following article appeared:

FRIDAY, OCTOBER 4, 1946 THE DAILY CALIFORNIAN

Seaborg Discusses Atomic Experiments

By Jack Bik '49

"Our goal is to learn as much as we can about the nucleus of the atom," announced Glenn T. Seaborg, professor of chemistry, as he revealed the presence on the campus of a team of 20 chemists from the Metallurgical laboratory of the University of Chicago who are working on the newly discovered trans-uranium elements.

Seaborg, who participated in the discovery of plutonium in 1940, has recently returned to campus after an absence of four years, during which he was in charge of research which led to the identification of elements 95 and 96.

"It was exciting work trying to create the atom bomb in time," Seaborg reminisced. Seaborg left the University in April, 1942, to work on the Manhattan project as research director in charge of plutonium chemistry at the Metallurgical laboratory.

"We thought at that time that it was a race with Germany but it turned out afterward that they were far behind us when we finally dropped the bomb. Apparently Hitler did not have much faith that an atom bomb could be devised."

Seaborg left the University for Chicago when it was decided that all chemists who had been working on the separations processes for obtaining pure plutonium could conduct their work at the Metallurgical laboratory. On leaving, he took with him Isador Perlman, now associate professor of chemistry, and Burris B. Cunningham, now assistant professor of chemistry at the University.

While Seaborg remained at Chicago to direct the work there, Perlman traveled on to the laboratories of the huge plutonium plants at Oak Ridge, Tenn., and to Hanford, Wash., where, according to Seaborg, his work "was of particular importance to the success of the process."

Meanwhile, Cunningham, in collaboration with L. B. Warner, graduate student in biochemistry here, prepared in August, 1942, the first pure sample of plutonium, free from carrier material and other foreign matter. This was the first time in history that a weighable amount of an artificially produced element was isolated.

Finally, as a result of their experiments, the "chemistry of plutonium became as well understood as or better understood than is that of most of the elements in the periodic system," and the army was all ready to construct the second atomic bomb which proved so disastrous to Nagasaki.

Climaxing the work at the Metallurgical laboratory, Seaborg, in collaboration with R. A. James, L. O. Morgan and A. Ghiorso, produced elements 95 and 96, which, like plutonium, never existed in nature.

Deciding that these elements needed a name, these investigators decided to call element 95 "americium," after the Americas, and element 96 "curium," after Pierre and Marie Curie, who fifty years ago first started the experiments in radioactive elements which later reached their climax in the atom bomb.

Other activities included mailing a corrected copy of Chapter XVI (now XV) of PPR Volume 14A to Gordon Leader and telling him that we would like it returned after he transfers the corrections to his master copy.

Paul O'Connor stopped in my office in the later afternoon to inform me that he passed his prelims today. His committee consisted of Axel R. Olson, Gerald E. K. Branch, William D. Gwinn (absent), Robert E. Connick, Robert B. Brode, and Lee H. Swinford.

Saturday, October 5, 1946

My morning was spent working on the talk entitled "Plutonium and Nuclear Energy," which I am scheduled to deliver to the California Section of the ACS on October 14.

Helen went with me to the Oregon-California football game in Memorial Stadium. California was defeated by a score of 14 to 13.

Sunday, October 6, 1946

Part of my day was spent preparing a lecture for the UC Extension Series called "Facts about Atomic Energy." My talk, "The Chemical Aspects of Atomic Energy," will be given on Friday, October 18. I also

worked on a paper on americium isotopes for Volume 14B of the PPR.

I found time to admire Peter's latest accomplishments and to talk with Helen.

Monday, October 7, 1946

After talking with some of the men this morning, I wrote a note to the Publications Manager of the American Institute of Physics to ask for the page reference, as soon as it is known, for the Kennedy, Seaborg, Segrè, and Wahl Physical Review paper, "Properties of 94^{239}." I want to include this in my article, "The Transuranium Elements," soon to be published in Science.

In response to his request, a long letter went to Dr. C. Guy Suits, highly recommending two men who might be interested in getting back into nuclear work--Vance R. Cooper and Frederic W. Albaugh. I told Suits these are the only two possibilities I can think of now.

I replied to Professor Paneth's letter of July 9, saying that I have delayed writing until the picture became clearer about naming elements 43, 61, 85, and 87. Except for element 61, I wrote, the situation now seems clear. I suggested that his review article in Nature should point out which research teams should do the naming; then those teams could follow with journal communications suggesting the names. I concluded with "It seems that this matter has taken more of your time than one might have anticipated at the beginning. However, I want to assure you that we all feel that you are doing a great service to chemistry to take the lead in this undertaking. Without some kind of organized approach, I am afraid the matter could drag on for years without any proper decisions."

Before going home I looked over my lecture notes for Chemistry 123 in my campus office.

At home Helen told me that Jo Owen visited her today.

Tuesday, October 8, 1946

Iz and I have decided that our group meeting, which now convenes on campus in Room 305, Old Chemistry Building, should begin at 8 a.m. during the school year to accommodate the graduate students who might have 10 o'clock classes. Present at today's meeting were Asprey, Blaedel, Covey, Cunningham, Eyring, Garden, Ghiorso, Goeckermann, Howland, Huffman, James, La Chapelle, Magnusson, Miller, Newton, O'Connor, Perlman, Prescott, Reynolds, Robinson, Seaborg, Stewart, Templeton, R. C. Thompson, S. G. Thompson, and Werner. The meeting opened with a number of general items including the announcement of another Chicago trip for measuring slow neutron fissionability in the Argonne pile.

Werner described his work with Am(V): the principal absorption lines of this Am(V) compound dissolved in 0.1 M H_2SO_4 lie at 510 and 720 mμ. He discussed the carrying of Am(V) by Pu(V). Cunningham said there appears to be at least 74 μg of curium formed in the Am^{241} sample irradiated in the Hanford pile (2.2 mg Am^{241} at a maximum flux for 4-1/2

months). James reported that the curium was decontaminated from the americium by a factor of 20. I suggested further decontamination and more decay studies.

R. Thompson discussed the exchange between active ferrocyanide and inactive ferricyanide in neutral and 1 N HCl solutions. Miller reported he is still recovering and purifying the protactinium, and Blaedel reported he is still unsuccessful in preparing protactinium metal.

Howland reported a value of 5×10^4 years for the half-life of Bi^{208} (assuming a cross section of 10^{-4} barn for the n,2n reaction in this particular pile position). Finally, Goeckermann talked about his bombardment of barium with helium ions.

* * * * *

At 10:10 a.m. I gave the Chem 123 lecture on radioactive decay.

Up on the hill I answered a letter I received last week from E. H. Hubert (Liège, Belgium), saying that I feel there are very serious but optimistic possibilities for the use of the nuclear fission chain reaction to run electric power plants in the future. I suggested that he read Farrington Daniels' article in C & E News (June 10, 1946) and write to Dr. C. A. Thomas, Monsanto Chemical Co., St. Louis, Missouri, who recently had a report published in The New York Times.

On September 26, William F. Meggers wrote to ask me to check a number of facts about new elements to be shown on the 1947 Edition of the Periodic Chart of the Atoms. I sent him a table giving such items as symbol, first isotope, discoverer, reference, most stable isotope, etc. I also included some background information on elements 43 and 61.

Yesterday I received a confirmatory letter from C. Guy Suits about his offer to me to serve on his "General Electric Nucleonics Project Consultants Committee." Today I wrote Suits to accept the offer. Others to be invited to participate on the committee include Hans Bethe, R. M. Evans, E. O. Lawrence, W. K. Lewis, Philip H. Morrison, John A. Wheeler, E. P. Wigner, Hood Worthington, and W. H. Zinn.

John Lewellen wrote that I have won a combination radio/phonograph for the question I submitted to the "Quiz Kids"--name an element named after a state, a country, and a continent--that Ruthie Duskin missed. In my reply I told Lewellen how pleased Helen and I are, and pointed out, for his information, that illinium is no longer correct.

I received a letter from James M. Crowe (C & E News), in which he said that he believes I have been informed that I have been nominated for Councillor-at-Large of the ACS. Crowe asked for biographical information, which I immediately sent him, adding that I had not heard anything about the nomination.

I answered a letter I received on campus today from Norris W. Rakestraw (Editor of the Journal of Chemical Education), who asked to publish the talk that I am to give before the California Section of the ACS. In my reply I explained that I will not use a prepared manuscript;

the talk will consist of information from a mixture of previous talks.

I wrote another note to the Publications Manager of the American Institute of Physics, to ask that the reference to the Kennedy, Seaborg, Segrè, and Wahl paper be sent to Mr. Willard L. Valentine (Editor of Science) for inclusion in the article ("The Transuranium Elements") whose proof just arrived. I then spent some time proofing the article.

J. D. McCullough wrote that his department and the Provost at UCLA have approved our consulting plan. He questioned the best time for his trip to Chicago (to see W. H. Zachariasen) and to Berkeley. I immediately replied and suggested that the trip to Chicago be planned for sometime between October 19 and October 31; the visit to Berkeley would fit better at the end of that trip. I said that, while he is in Chicago, he should also visit Darrell Osborne and Winston Manning. Then I wrote to Professor Zachariasen, informing him of McCullough's visit.

A letter went to Gordon Leader to answer the questions he raised about status of various papers in the Plutonium Project Record. I asked him to send me a copy of the present chapter numbers and corresponding titles to verify my outline.

Other letters today went to Fred Albaugh and Vance Cooper, each of them receiving a copy of my letter to Guy Suits and the suggestion that General Electric's nuclear research program is worth investigating.

At 4:15 p.m. there was a joint meeting of Latimer's and Seaborg's groups in Room 102, Gilman Hall. Then, at 7:30 p.m., I went to the Research Conference--also in Room 102, Gilman.

Jeanette returned from her visit with our parents in South Gate.

Wednesday, October 9, 1946

This morning I looked over Roy Overstreet's and Louis Jacobson's paper and then returned it to Joe Hamilton with pencilled corrections.

In September Ray Stoughton wrote to Professor Latimer, with a copy to me, saying that they are interested in determining values of the neutron capture and fission cross sections of U^{234} as functions of neutron energy because of the effects this isotope may have on U^{233} breeder piles. Stoughton had recently heard that people at Site Y had determined the thermal neutron fission cross section to be about 2 barns for U^{234} using a 10 microgram sample of U^{234} produced in Berkeley. Ray had several questions about the purity of the sample. Today I replied and said we do not know about the purity, we don't know where the sample is but are trying to find out, and we think that he should try to get the sample to make the measurements he wants.

Jacob Sacks returned my Princeton talk with a few notes added to it to remove my California bias.

I went down to campus, checked in the Department office, went over my notes for Chemistry 123, and then stopped in to talk with Geoffrey Wilkinson. Wilkinson plans to use bombardments at the 60-inch cyclotron

to look at isotopes of noble elements.

Thursday, October 10, 1946

Today's lecture in Chemistry 123 was about nuclear stability. After class I stopped in and talked with Geoffrey Wilkinson.

Back on the hill, in a memo to George Everson, I asked that Edwin F. Orlemann, who is now an Assistant Professor in the Department of Chemistry (and formerly a member of my Section C-I at the Met Lab), be given a badge to admit him to Bldg 4.

I read a memo from Charles J. Wrobel in the Medical Department to Iz Perlman, calling attention to the fact that the men are eating, making coffee, and smoking close to where plutonium is being handled. He noted that one worker's pipe had a contamination of 20,000 c/m. In addition, the contamination on the surfaces in Cunningham's laboratory and in Room 109 far surpass tolerance standards in other laboratories. Air samples in Cunningham's lab are above tolerance levels.

In the late afternoon Helen and I attended a cocktail party at the Claremont Hotel in honor of Professor and Mrs. Manne Siegbahn of Sweden. Professor Siegbahn (1923 Nobel Laureate in Physics) attended the Princeton Bicenntennial in late September at which I spoke.

Friday, October 11, 1946

I took care of some routine matters and looked over the mail, which included a nice thank-you note, dated September 1, from Eberhard Grassmann for my letter of July 18. Grassmann said that if I knew the situation in Germany today, I could understand why he wants to emigrate. He also congratulated me on elements 95 and 96.

I received a letter from John V. Dodge (Assistant to the Editor of Encyclopaedia Britannica), requesting a photograph relating to the transuranic elements to be used to illustrate the article "Physics" in a new four-volume Britannica publication, Ten Eventful Years. Dodge suggested a picture of a microbalance.

Walter J. Murphy (Editor of Industrial Engineering and Chemistry) asked me to review a paper, "Correcting for the Absorption of Weak Beta Particles in Thick Samples," by Peter E. Yankwich, Thomas H. Norris, and John Huston. Since I had a number of important changes to suggest, I passed these on to Yankwich on campus and then wrote to Murphy to explain what I had done. I said that, when the changes are made, I believe the article is worth publishing.

Helen had lunch with some friends and in the evening went to her Russian class in Wheeler Hall, given by University Extension, with Lee Perlman.

Saturday, October 12, 1946

I went through several of the laboratories to check on the status of the research.

California defeated St. Mary's in today's football game, 20-13. Helen and I saw the game in Memorial Stadium.

Sunday, October 13, 1946

Today's writing projects consisted of papers for Volume 14B of the Plutonium Project Record (PPR) and the chapter, "Nuclear Properties of Transplutonium Isotopes," for Volume 14A. I also made a rough outline of a paper on the history of the transuranium elements that Segrè and I plan to write for Nature.

I spent quite a bit of time admiring young Peter's talents.

Monday, October 14, 1946

Today I wrote a thank-you note to Dr. Venancio Deulofeu for the copy of Ciencia e Investigacion he sent me and said that I am not in a position now to submit a manuscript for publication.

Another letter went to Professor S. C. Lind, explaining that I had to return to Berkeley early and therefore missed him at the ACS meeting. I mentioned that Professor Joliot, whom I met at Princeton, told me that the International Union of Chemistry will pay expenses to the London meeting. Therefore, I shall accept the appointment to the Commission of Radioactive Constants. I then wrote to Professor R. Delaby, accepted the appointment, and told him of my conversation with Professor Joliot.

Robert Machol, who is working on an encyclopedia project, wrote on July 29 that it would be helpful in organizing the project to have the names for elements 43, 61, 85, and 87. He promised that the names would not published until they are officially released. Today I sent him the names technetium, astatine, and francium. I explained that the question of who has the right to name element 61 has not yet been settled, but that he can stay current on the matter by keeping in touch with Professor C. D. Coryell.

I also wrote to Al Florin to ask him to check several references in the Los Alamos reports, which are not available to us. This is information Iz and I need in order to bring the "Table of Isotopes" up to date.

Down on campus I went over the lecture notes for Chem 123.

Helen told me when I got home that Jo Owen had tea with her this afternoon.

At 6:30 p.m. Helen and I had dinner with members of the California Section of the American Chemistry Society in the main dining room of the Claremont Hotel in Berkeley. Then, at 7:45 p.m., in the Florentine Room, I gave the after-dinner speech--"Plutonium and Nuclear Energy." In addition to reviewing the role of plutonium as a nuclear energy source, I again announced that Np^{237} undergoes slow neutron fission with a very small cross section and gave its cross section for the absorption of slow neutrons. I described the discovery of Pu^{241} and announced the mass numbers and properties of Am^{241}, Cm^{240}, and Cm^{242}. I discussed the

chemical properties of americium and curium and announced the discovery of new neutron-deficient isotopes of neptunium produced in the 60-inch cyclotron by bombardments with deuterons and alpha particles.

Tuesday, October 15, 1946

At 10:10 a.m. I gave the Chem 123 lecture on beta particles, including such points as theory of beta emission, allowed and forbidden spectra, etc.

Back on the hill I found a note from Kay Way in Oak Ridge:

> Mr. Sullivan has received a photostatic copy of the revised table but keeps it closely guarded down in the chemistry building. If you have another one available we would certainly appreciate it very much if you could send it along for the physics group here. I will promise faithfully to go over it carefully and compare the changes with those I have made in my copy.

Alden H. Emery, Secretary of the ACS, wrote that he is disappointed that he will be unable to attend my Harrison Howe Lecture.

A note arrived from Hoylande D. Young in which she said that she is returning the negatives for the chart of isotopes to me and that the Harrison Howe Lecture is now in the process of being declassified.

A thank-you letter came from Otto M. Smith for my evaluation of Herbert Anderson.

I also received a letter from Marston T. Bogert (President of the International Union of Chemistry), to whom I sent a carbon of my acceptance of the appointment to the International Commission of Radioactive Constants. Bogert, an emeritus professor at Columbia, said that he thinks it is hardly likely that the Union will have sufficient funds to defray the expenses of delegates to the meetings.

Alice Boniface was hired today to work as junior laboratory technician.

There was a joint group meeting in Gilman Hall at 4:15 p.m. I also went to the evening Research Conference meeting.

Wednesday, October 16, 1946

I took care of some routine matters and visited some of the labs before looking over the mail.

A letter arrived from Edwin O. Wiig, who said that he will meet my train and that Dr. Noyes asked him to invite me to dinner after the talk. Wiig then asked for a photograph for publicity purposes, and this was promptly sent to him.

In a letter dated October 11, James Crowe asked if I would write a short article on shielding. I wrote today to explain that the statement he read was taken out of context from my California Section talk and implies that I have more ideas on the subject than I do. Crowe also

requested a few recent photographs to be used for the preparation of a sketch of me for the cover of the December C & E News. I enclosed some photographs and made some suggestions on symbolism that might also be included on the cover.

Iz Perlman is attending the General Information Meeting at Clinton Laboratories this week. This morning he is scheduled to chair the meeting on chemistry and to speak on "New Isotopes in the Lead-Bismuth-Polonium Region."

I stopped down on campus, checked in with Miss Kittredge, and then looked at my lecture notes for Chemistry 123.

Thursday, October 17, 1946

I gave the usual morning lecture to the Chem 123 class. Today's topic was the measurement of beta particles and gamma rays.

Back on the hill I found that George G. Taylor had answered the letter I wrote to Alden Emery about the nomination for Councillor-at-Large. Taylor said that my name was suggested by more than one section and therefore will appear on the list of suggested names.

Robert F. Gould of C & E News phoned me; he was unhappy that I had not given them an advance copy of my talk to the California Section of the ACS. I described some high points in the talk and explained that it was theoretically old material since I gave the same talk to the American Physical Society on June 21 in Chicago. I told him that, in the future, I will try to let him know about such talks in advance. Gould said he will prepare a story on the talk anyway. I then mailed him a copy of the talk, saying that it is for information only.

Iz Perlman is scheduled to talk on "Chemical Properties of Americium and Curium" at the General Information Meeting at Clinton Laboratories this afternoon.

Friday, October 18, 1946

This morning I received a teletype from R. F. Gould with his story on fissionable neptunium, which Gould explained was sent to the printers last night. He suggested I teletype any corrections to Wa-23.

> The discovery of a fissionable isotope of neptunium was announced by G. T. Seaborg, Professor of Chemistry at the University of California, at a meeting of the California Section of the American Chemical Society on October 14. The new isotope, Np^{237}, undergoes fission upon bombardment with slow neutrons, but the number of atoms which split in any given quantity is so low that it could not be used in atomic bombs, according to Dr. Seaborg.
>
> Np^{237} was first produced in 1942 by Dr. Seaborg and Arthur C. Wahl in the 60-inch cyclotron at Berkeley but its fissionable tendency and low fission cross section 2×10^{-26} sq. cm. were discovered by A. Ghiorso, D. W. Osborne, and L. B. Magnusson working with Dr. Seaborg at the Metallurgical Laboratory in Chicago. It is among the

by-products of plutonium production at the Clinton and Hanford plants.

Dr. Seaborg also reported the discovery of a new isotope of plutonium, Pu^{241}, and announced the mass numbers of the elements, americium and curium. Pu^{241} is produced by alpha particle bombardment of U^{238}. It has a relatively long half-life and forms an isotope of americium by beta emission.

Am^{241} - element 95 - is an alpha emitter and has a half-life of 500 years. Curium - element 96 - has two alpha-emitting isotopes, with mass numbers 240 and 242. Their half-lives are one month and five months respectively.

You will note that we have retained the "announcement" style in this story inasmuch as it didn't get any play when it was given earlier in the year.

My immediate teletyped response was "Your story is o.k."

I talked with some of the men for a while this morning and then looked over my mail, which contained a copy of a letter from Darrell Osborne to Iz Perlman, which contained information on Paper No. 9.8, Volume 178 of the PPR. Darrell then covered a number of changes and suggestions for the "Table of Isotopes."

In the afternoon Helen attended the Section Club Tea at President Sproul's house on campus.

Helen and I went to the auditorium of University Extension Center, 540 Powell St., San Francisco, where at 8 p.m., I gave the second weekly lecture of the series "Facts about Atomic Energy," arranged by George Pettitt. My talk was entitled "The Chemical Aspects of Atomic Energy." Edwin McMillan was the first speaker in this series (October 11). Robert S. Stone will speak on October 25, Joseph Hamilton on November 1, Isadore Perlman on November 8, and J. Robert Oppenheimer on November 15. Tonight I described the chemical processes used in the production of plutonium, the role of chemistry in the production of power with atomic energy devices, and the manifold uses of radioactive tracers. I stated that, in the long run, I believe that the greatest benefit of atomic energy to humanity will come from the use of radioactive isotopes.

Saturday, October 19, 1946

I made my usual rounds of the laboratories to check on the status of the research this morning.

California lost to UCLA, 13 to 6, in a game Halen and I saw in Memorial Stadium this afternoon. The UCLA star, Cal Rossi, was injured, and this may affect their chances to play in the Rose Bowl. After the game Helen and I went to a cocktail party at President Sproul's house.

Sunday, October 20, 1946

As usual, I worked on my writing projects--the chapters on plutonium

isotopes, on neptunium isotopes, and on transplutonium isotopes, for Volume 14A of the Plutonium Project Record. I also went over my notes for Chem 123.

In the afternoon Helen, Peter, and I visited the Ghiorsos' home and saw their new son Bill. Later Helen and I left Peter with Jeanette and went to the nearby (1115 Solano Ave.) Albany Theater, where we saw Fred MacMurray and Anne Baxter in "Smoky." The second feature was "Night Editor."

Monday, October 21, 1946

I talked with some of the fellows and then read a letter from a friend, Jimmie Lu Valle, who said that, if I plan to stay in Rochester through Tuesday, he and his wife Jean would like me to have dinner with them Tuesday night. [Jimmie was a fellow chemistry major at UCLA and is now working for Eastman Kodak in Rochester. In college he was quite an athlete--he ran the 400-meter dash in the 1936 Olympic Games in Berlin.]

I had three copies of the chapter, "Nuclear Properties of Transplutonium Isotopes," for Volume 14A mailed to Gordon Leader. This, I said, should be considered in final form.

Ruth L. Donnell was hired as a junior laboratory technician at $0.75/hour (indeterminate hours).

Much of the rest of the day was spent catching up on my report reading.

Helen went to a child guidance meeting with Rebecca Cason this afternoon.

Tuesday, October 22, 1946

Present at this morning's group meeting were Asprey, Beaufait, Blaedel, Bockhop, Bradley, Covey, Cunningham, Eyring, Ghiorso, Goeckermann, Hopkins, Howland, Huffman, James, La Chapelle, Lilly, Magnusson, D. Miller, Earl Miller, Morgan, Moyer, Newton, O'Connor, Perlman, Prescott, Reynolds, Robinson, Seaborg, Stewart, Templeton, R. Thompson, S. Thompson, and Werner. Perlman described the new hexagonal form of isotope chart being suggested (promoted) by Sullivan at Oak Ridge. I announced that Ghiorso, R. Thompson and possibly one other [later Templeton was chosen] will go to Chicago on November 13 to measure slow neutron fission cross sections of a number of isotopes. The Chicago collaborators will be Osborne, William Bentley, Bob Elson, and Robert Nelson. Ghiorso then described the materials on which the measurements will be made.

Goeckermann talked about his 44 Mev helium ion bombardment of Cs^{133}, in which he found a 27-hour barium isotope that emits x-rays, gamma rays, and electrons; and a 3 to 7-hour lanthanum isotope that emits electrons whose spectrum looks like β^-. More bombardments will be done. Templeton described a thallium bombardment with 44 Mev helium ions.

Blaedel reported a value of 1265°C for the melting point of protactinium, and Eyring said the heat of solution of americium metal has been measured as 156 Kcal/mole.

Earl R. Miller then talked about the work of the Health Physics Organization. It was announced that a room in ORL will be made available for work on very active materials immediately after they come out of the 60-inch cyclotron.

* * * * *

The topic for today's lecture in Chem 123 was alpha emission.

On the hill I received another letter from Professor Paneth. He enclosed copies of his recent letters to Segrè and Coryell. In his letter to Segrè he questioned whether the name for element 85 should be astatine, as Segrè is suggesting, or astasine. He said, "I have a feeling that if Aristotle were alive and you gave him the choice only between astasine and astatine, he would prefer the former."

Walter J. Murphy (Industrial and Engineering Chemistry) wrote, in a letter I received today, that he approved of the manner I handled the review of Yankwich's paper.

A note went to Nan Mountjoy Bowie to decline an invitation to speak to the Queen's Bench, an organization of active women attorneys in the Bay Area. I explained that I shall be in the East on the date of their meeting, November 20.

I have gone over the comments Jacob Sacks made on my talk, "Applications of Artificial Radioactive Tracers to Chemistry and Medicine," made the necessary corrections, and today mailed a corrected copy to Dr. D. Hamilton for publication in the record of the Princeton Bicentennial.

I replied to a letter I received earlier in the month from John Dodge. I explained that we do not have any very good photographs, except for those to be published shortly in Science, but we will be glad to furnish prints of any of these he might want. I then said that I know that Mr. Fritz Goro of Life magazine has some unpublished photographs and perhaps he can make arrangements for the use of one of them.

Gordon Leader telephoned about a couple of figures for a paper in the Plutonium Project Record, Volume 14B. I promised to mail them tomorrow.

At 4:15 p.m. there was the regular meeting of the Connick-Seaborg groups in Gilman Hall. I also went to the Research Conference in the evening.

Wednesday, October 23, 1946

I talked with some of the fellows and then handled some correspondence. I thanked Jacob Sacks for the comments he made on my Princeton lecture.

I answered a letter from Weldon Reynolds (Encyclopaedia Britannica). Reynolds was asked by Edward Teller, who is preparing an article on "Nucleus," to determine whether I am going to include a chart of all known nuclei in the article I am going to prepare on "Radioactivity." I said that such a chart is an extremely formidable project to prepare and difficult to reproduce; however, I enclosed an old "Table of Isotopes" and said that it might be worth including. I cautioned that a new table will be 50% larger and might not be ready for publication for a year or two. I then went on to ask whether the article I am to prepare should only be on artificial radioactivity or should include natural radioactivity.

I returned the PSQ of John G. Conway of the University of Pittsburgh to George Everson and asked that he proceed to offer Conway a position. [Conway is a spectroscopist.] I also asked whether he has heard from Henri Levy. We are interested in Levy in connection with our molecular structure x-ray work.

The figures that Gordon Leader requested yesterday were mailed to him today. I discussed the status of some of the other papers being prepared for Volume 14B, the collected papers volume covering "The Transuranium Elements" in the Plutonium Project Record (PPR). I mentioned that Roy Thompson will help Ghiorso finish his many papers. [Gordon is working in the Argonne editorial office for the PPR.]

In today's mail was a thank-you note from Irene L. Hofmeister for my lecture at University Extension last week.

Thursday, October 24, 1946

The Chem 123 lecture was given at 10:10 a.m. and was a continuation of the discussion on alpha emission.

On the hill, in response to a request dated October 8, I wrote to Professor George B. Kistiakowsky to give him an evaluation of Darrell W. Osborne. After discussing Osborne's research ability and personality, I said that, although Osborne is well satisfied with his position at Argonne National Laboratory, "I believe that at heart he is fundamentally an academic man and that he could be interested in such a position if it were at a large university such as Harvard and if it presented the opportunity to continue work in the nuclear field."

I answered another letter from Robert Machol, who asked for information on the release date of the names of elements 43, 85, and 87. I explained that I do not know and suggested he contact the discoverers. Machol also questioned the symbol for francium, which I had given him as Fa. In this case, I told him, I learned of the symbol from Paneth and again suggested that Mlle. Perey should be contacted. I also said that astatine, derived from a Greek adjective, conveys the meaning "instability," and I believe technetium comes from a general stem such as technology, possibly from the Greek.

C. E. K. Mees recently wrote and asked me to visit Eastman Kodak while I am in Rochester. Mees suggested that I might be interested in seeing the equipment they use for preparing C^{13} and N^{15}. In my answer

today I accepted, saying that I have learned that he will be in Berkeley while I am in Rochester. I also mentioned that I should like to see Dr. James Lu Valle who attended UCLA with me.

I mailed Gordon Leader a few more comments about papers for Volume 14B of the Plutonium Project Record. In particular, I suggested that the paper by Stanley Thompson and me, "The First Use of Bismuth Phosphate for Separating Plutonium from Uranium and Fission Products" (issued in Chicago as memorandum MUC-GTS-2288) should appear as a collected paper in Volume 14B. I mentioned to Gordon a couple of items connected with Volume 17B, the collected papers volume related to U^{233}, and asked him to discuss them with Leonard Katzin (Editor of this volume).

I wrote and thanked Jimmie Lu Valle for his dinner invitation on Tuesday, November 19. I explained that I have accepted a dinner invitation from W. A. Noyes, Jr., for that evening but I hope to see him during the morning at Eastman Kodak and perhaps have lunch with him. I then said that I am still not certain whether UCLA will go to the Rose Bowl but if they do and I can get tickets, Helen and I will be there.

A long chatty letter arrived from Ruth Rogers, who gave me the status of the numerous reports I have asked her to mail to me. Ruth included some gossip about the secretaries there and then told me the startling news that the 250 copies of Met Lab Report C1-440 that I requested (notes covering the instructional lectures on nuclear chemistry I gave during the summer of 1942) were missent to Winston Manning with each copy stamped with the secret classification. I think this is a stupid action since there is nothing secret in the document and thousands of copies have been freely distributed. I need these copies for use by the students in Chem 123, and it is very annoying to have to put them through the tedious declassification process.

J. Robert Oppenheimer, Joe Hamilton, Ernest Lawrence, and I got together to talk about the problems arising from General Groves' directive of August 5 about the allocation of U^{233}, U^{235}, and Pu^{239}. Hamilton, Oppenheimer, and I will prepare a suggested procedure about simplifying the handling of the small amounts we use, and Lawrence will transmit the document to Groves.

I then made the rounds of the labs to look over the research and to chat with some of the fellows.

Friday, October 25, 1946

I declined an invitation to speak to the American Society for Metals on "Metallurgy of Uranium and Plutonium" next March. In my letter to W. H. Eisenman (Secretary), I explained that I am not very familiar with the subject and that Professor Frank Spedding and Professor Cyril S. Smith are much more qualified to discuss this topic.

I replied to the lengthy letter I received yesterday from Ruth Rogers, saying that I am disappointed about the delay of the 250 copies of the Seaborg lectures and noting that they are useless to us if they are marked "secret." I then explained my rearranged schedule for my trip East in November and asked her to inform everyone about my change in

plans.

Dr. Earl Miller informed me that he has arranged for us to use the conference room in Bldg. 8 for lunches since it is essential that the men not eat in the laboratories. I called Rex Barton and made arrangements for the room to be reserved from 12 noon until 1:15 p.m. each day.

My paper, "The Transuranium Elements," finally appeared in today's issue of Science. In it is described the discovery of neptunium and plutonium, the methods worked out for the production of plutonium in quantity, and the Manhattan Project during the war (including the work at the Metallurgical Laboratory on the processes for the separation of plutonium from uranium and fission products at Hanford, the role that the ultra microchemical investigations played in the development of this process, and the discovery of elements 95 and 96 with suggestions that they be named "americium" and "curium"). I included my first version of the periodic table, showing the elements beyond actinium as members of an actinide transition series, and a chart showing the mass numbers of our newly discovered heavy isotopes. Unfortunately, the page reference to "Properties of 94^{239}" was not included.

I read a copy of Burris Cunningham's memorandum to the Area Engineer, in which Burris described the security arrangements for 160 g plutonium (requested in SR-139). This "top secret" material will be kept in a safe in the northeast part of Room 203, Bldg. 4. The Area Engineer will be requested to open the safe at 2 p.m. on Monday of each week to allow an assay to be made to assure that all of the plutonium is there.

Much of the rest of the day was taken up with trying to catch up on my report reading.

Helen and Lee Perlman went to their Russian class during the evening.

Saturday, October 26, 1946

At the laboratory this morning I looked over a rather extensive list of changes and comments on the "Table of Isotopes" sent by Tony Turkevich from Chicago. He included comments by his colleagues Nathan Sugarman, Bill Libby, and Mark Inghram.

In the afternoon I played golf with Professor G. Ernest Gibson at the Mira Vista Country Club, where he is a life-time member (GEG-98, GTS-101). He beat me in match play, 4 and 3.

California played Washington in Seattle this afternoon and was defeated by a score of 20 to 6.

Sunday, October 27, 1946

I worked on chapters for Volume 14A of the Plutonium Project Record (PPR) for a while, then Helen and I went to Tilden Park to attend the Northern California Open. Charlie Sheppard was the winner, with Jackson Bradley second and Cal Smith third.

Peter now weighs about 14 pounds.

Monday, October 28, 1946

Iz and I worked on a midterm examination for the Chem 123 class.

In the mail was a letter from Fred Albaugh who wrote from Los Angeles, thanking me for the remarks in his behalf to General Electric. Fred said that I analyzed the situation properly--that he is not actively seeking a new position, but he is questioning his future in the oil industry, an industry that traditionally has been dominated by engineers. Fred also suggested giving General Electric his home address.

I mailed James Grady (ACS News Service) some material on my Harrison Howe Lecture, including some paragraphs and a picture of the first americium ever isolated. I emphasized that I want Professor Cunningham's name mentioned since he isolated this pure form. In addition, I mentioned Daniel M. Wilkes, University of California Public Information Office, and suggested that Grady and Wilkes should get in touch with each other in order that both might operate to the best advantage.

Later I looked over some of the research and offered advice.

Tuesday, October 29, 1946

Perlman and I gave the following midterm examination to the Chem 123 class.

Chem 123 - Oct. 29, 1946

1. For $_{4}Be^{9}$, give or calculate the following:

Mass number
Mass
Mass defect
Packing fraction
Binding energy

Credit 25

2a. U^{238} and U^{234} are in secular equilibrium and account for virtually all of the radioactivity of pure uranium. One mg. of pure uranium gives 1400 disintegrations per minute. What is the half life of U^{238}? If uranium is 0.6% U^{234} what is its half life? (Express in terms of $T_{1/2}(U^{238})$ if you wish.)

Credit 15

2b. The $T_{1/2}$ of $_{96}Cm^{242}$ is 150 days. A sample originally giving 1.00×10^{7} disint/min was allowed to decay for 1 half life, and the $_{44}Pu$ chemical fraction removed. It contained α activity of 4.1×10^{4} α/min. What is the half-life of Pu^{238} in yr. (Hint: The half-life of Pu^{239} is sufficiently long that one need not consider its deday during the period of 150 days.) Credit 15

3. The following isotopes of uranium with their properties are known

U^{233}	U^{234}	U^{235}	U^{237}	U^{238}	U^{239}
α	α	α	β^-	α	β^-
(assume 3×10^4 yr	7×10^8 yr	7 days		5×10^9 yr	23 min
long, sev thousand yr.)					

Predict for the folowing, the mode of decay:
U^{232} U^{236} U^{240} Credit 15

Guess the half lives for each Credit 5

4. A certain activity is prepared by cyclotron bombardment and certain measurements made

1. No α particles are present
2. Both betas and gammas were observed by crude absorption measurements.
3. In magnetic bending experiments a soft component with negative sign was observed (β^- or e^-) and two hard components with positive sign (B^+)
4. Aluminum absorption curves were taken and the three particle components were resolved

0.2 Mev	15,000 counts/min,
1.2 Mev	20,000 counts/min,
2.0 Mev	10,000 counts/min.

5. Lead absorption curves indicated three gammas.

A weak soft component 0.2Mev 75 c/m
A strong medium compnent 0.5 Mev 600 c/m
A harder compnent 0.8 Mev 200 c/m

Assume counting efficiencies for 0.8 and 0.5 Mev γ to to be 1% and 0.2 Mev γ to be 0.5%. Cred. 25

I recently asked Bob Campbell for colored slides of some of the photographs that appeared in the plutonium article in Life on July 8, which I thought I could use in my Harrison Howe Lecture. Campbell agreed, but today I received a night letter from him saying that unfortunately the slides will not be ready for about a month. I then called Campbell. Although I probably will not get the slides in time for the Lecture, we did discuss the slides I want to have.

Iz and I spent all of our spare time today grading this morning's midterms.

Amos Newton, covering information he learned at the General Information Meeting at Clinton Laboratories two weeks ago, talked about the Clinton helium-cooled "Daniels" pile, the sodium-potassium amalgam-cooled "Zinn" pile, and the Clinton heterogeneous pile at a meeting I attended today.

At 7:30 p.m. I went to the Research Conference meeting in Room 102, Gilman Hall.

Wednesday, October 30, 1946

I looked over Amos Newton's report to C. E. K. Mees for the period September 30, 1946. Newton described our unsuccessful search for element 97 in two sources--bombardment of Am^{241} with helium ions and a 4-1/2 month neutron bombardment of Am^{241} in the center of the Hanford pile. Newton reported on Np^{232}, found to decay to 30-year U^{232}. He also reported on our new long-lived Bi^{208}, the oxidation of americium, the exchange reactions in the ferricyanide-ferrocyanide system, and the use of separated isotopes in assigning nuclear properties.

Iz and I finished checking the Chemistry 123 midterms--it is a large class.

A note of appreciation arrived in today's mail from Robert Littell (Reader's Digest). Littell commented that the reprints I sent him, although lucid to the interested and attentive layman, are still esoteric to Tom, Dick, and Harry. He mentioned that Reader's Digest does have an article in the works revolving around the Geiger counter.

I was informed by Frank W. Test that Case No. S-2950 "Concentration Process for Plutonium Ions, in an Oxidation State Not Greater than +4, in Aqueous Acid Solution," by Glenn T. Seaborg and Stanley G. Thompson, was filed in the United States Patent Office on September 20, 1946.

Iz Perlman requested of Russell Ball two 60-inch cyclotron bombardments: 10 μah of helium ions on 25 mg Y_2O_3 and 10 μah of deuterons on 100 mg palladium.

Jo Owen visited Helen in the afternoon.

Thursday, October 31, 1946

Today's topic in Chem 123 was induced nuclear reactions. Tuesday's midterm was also returned and explained to the class. I then went back

to the hill.

Ernest Lawrence has decided that the Radiation Laboratory should be involved in the design of a nuclear power reactor (pile) of simple construction that can be readily built and tested. Both Lawrence and I hold consultantships with General Electric Co., which is now serving as the operating contractor for the Hanford Engineer Works (HEW) and is interested in the design of power reactors. Today Lawrence, Iz Perlman, Charles Prescott, and I met with representatives of General Electric, who are visiting Berkeley--John Howe, John H. Holloman, and William E. Ruder, to discuss a reactor using a uranium-molybdenum fuel. Lawrence thinks such a reactor, designed to furnish 10,000 kilowatts of heat energy, using 40 kg of U^{235} fuel, and operating at 250°C, is a good practical objective and capable of being built at HEW within 18 months.

C. E. K. Mees wrote, in a letter I received today, that he will not be back from California on the day I am in Rochester, but his assistant, Dr. John Leermakers, will meet me at Eastman Kodak and show me around. He said that Dr. James Lu Valle is also looking forward to my visit. Another letter bearing on my Rochester trip arrived from W. Albert Noyes, Jr., who said that he has been asked to go to Paris on November 15 for UNESCO. If he is not in Rochester when I visit, Dr. Edwin Wiig, other members of the department, and Dr. Lu Valle will entertain me properly.

Bee Barbachano spent the evening with us.

Friday, November 1, 1946

This morning I wrote to Gordon Leader and told him that before Ghiorso can prepare Paper 22.29 of Volume 14B, he will need the sketches William Bentley was to prepare. I also informed Leader that we have added another paper, "The Bombardment of U^{233} with 44 Mev Helium Ions and the Formation of Pu^{234}" by Perlman, O'Connor, and Morgan, and that on Monday I will mail him the chapters entitled "Isotopes of Plutonium and Their Radioactive Properties" and "Isotopes of Neptunium and Their Radioactive Properties" for Volume 14A.

In today's mail another letter arrived from Darrell Osborne, with more answers to questions I have asked the Argonne people about papers for the Plutonium Project Record, the "Table of Isotopes," etc.

I also received a letter from Bob Campbell, who just learned that Eastman Kodak is now making a new color film that can be processed in a regular photo lab, making it possible to prepare the color slides I requested. Campbell listed the slides he has ordered--these should be ready for my Harrison Howe Lecture--and asked if he should send these directly to Professor Noyes and if there are any other slides that I want.

I made the rounds of the laboratories to check on the research.

The 184-inch cyclotron is completed and is being operated as a synchrocyclotron, accelerating deuterons to 200 Mev. McMillan performed the first experiment (17 minutes before midnight) for studies of neutron angular distribution.

From 7 until 10 p.m. Helen and Lee Perlman attended their course in elementary Russian given in Wheeler Hall by the Extension Division.

Saturday, November 2, 1946

I visited a number of the laboratories in Bldg. 4 and then worked with Iz Perlman on the "Table of Isotopes."

California beat Washington State, 47 to 14, in the game Helen and I attended with John Howe at Memorial Stadium this afternoon.

Sunday, November 3, 1946

I spent part of the day on my writing projects, particularly on the article for Nature that I am writing with Segrè. I also worked on the article called "Thermal Neutron Fissionability of Heavy Nuclei" for the PPR.

Later Helen and I went to a cocktail party at the home of Wendell and Glatha Latimer on Euclid Avenue in Berkeley.

Monday, November 4, 1946

I took care of some of the correspondence on the top of my desk this morning. I wrote to Neil E. Gordon (Chairman of the Chemistry Department

of Wayne University), who recently asked me to take part in their "Chemical Forum". In my reply today I said that if I could have the date of March 24, I might be able to combine the visit with a talk in Cleveland on March 21.

I mailed Gordon Leader corrected copies of "Isotopes of Plutonium and Their Radioactive Properties" and "Isotopes of Neptunium and Their Radioactive Properties" and pointed out that Truman Kohman can make his corrections before the final typing. I mentioned a few comments and questions about the various papers for the Plutonium Project Record, both for Volumes 14A and 14B.

In my reply to Bob Campbell, I identified for him the slides I specifically want for my Harrison Howe Lecture and asked that he send them to Professor Edwin O. Wiig since Professor Noyes will be out of town. I also mentioned the other slides I would like for future lectures.

Winston Manning phoned about some missing information concerning one of the papers in Volume 14B of the Plutonium Project Record. After talking with O'Connor and Ghiorso, I wrote to Manning that they think the needed information may be found in J. W. Britain's papers and notebooks.

I sent a brief note to W. H Sullivan to tell him that I shall be in Chicago November 13 through November 17 and that this could be an ideal time to discuss the material to be included in the "Table of Isotopes" and the "Chart of Isotopes," if he (Sullivan) is able to go to Chicago at that time.

Before going to campus where I checked in at the Department of Chemistry Office and then looked over the lecture notes for Chemistry 123, I went over today's incoming mail.

Tony Turkevich sent me a revised figure of 5300 years, determined by M. G. Inghram and L. D. Norris, for the half-life of C^{14}.

I read a letter of October 30 from Darrell Osborne, describing their arrangements for the deuteron bombardment of Pa^{231} at Berkeley and the fission measurements on some of the products at Argonne.

Tuesday, November 5, 1946

In attendance at today's 8 a.m. group meeting were Asprey, Blaedel, Bockhop, Bradley, Covey, Cunningham, Eyring, Garden, Ghiorso, Goeckermann, Hopkins, Howland, Huffman, James, La Chapelle, Lilly, Miller, Morgan, Moyer, Newton, O'Connor, Perlman, Prescott, Reynolds, Robinson, Seaborg, Stewart, Templeton, R. Thompson, S. Thompson, and Werner. The meeting started eight minutes late because of the slow appearance of the participants. Templeton reported that the 12-hour bismuth activity has been definitely assigned to Bi^{204}. He discussed his evidence for this and said that the 68-minute lead daughter must be an isomer of Pb^{204}.

I made a number of general announcements including the fact that Tom Morgan has been chosen to serve as liaison man with Tom Putnam (head of the 60-inch cyclotron crew) for our 60-inch bombardments. I confirmed

the newspaper reports that the 184-inch cyclotron is in operating condition and that bombardments will begin soon. There was some discussion about the bombardments planned for the 184-inch, and Templeton reported on information he heard at last night's Physics Journal Club meeting (held regularly on Monday evenings) of a bombardment at the 184-inch cyclotron of beryllium at the end of a copper probe to produce high energy neutrons.

Blaedel talked about his unsuccessful attempts to produce protactinium metal and retracted the statement he made at the October 22 group meeting about the melting point of protactinium metal (1265°C). Goeckermann reported briefly on the results of his recent bombardment of yttrium with 22 Mev deuterons.

* * * * *

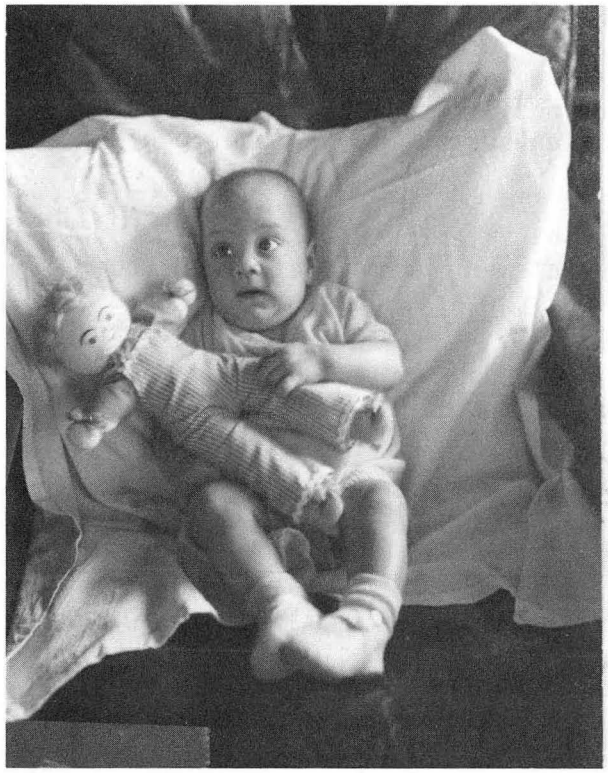
At 10:10 a.m. I gave the Chem 123 lecture on campus. I continued the discussion of induced nuclear reactions and covered nuclear barriers, cross sections, etc. I then went to my office on the hill.

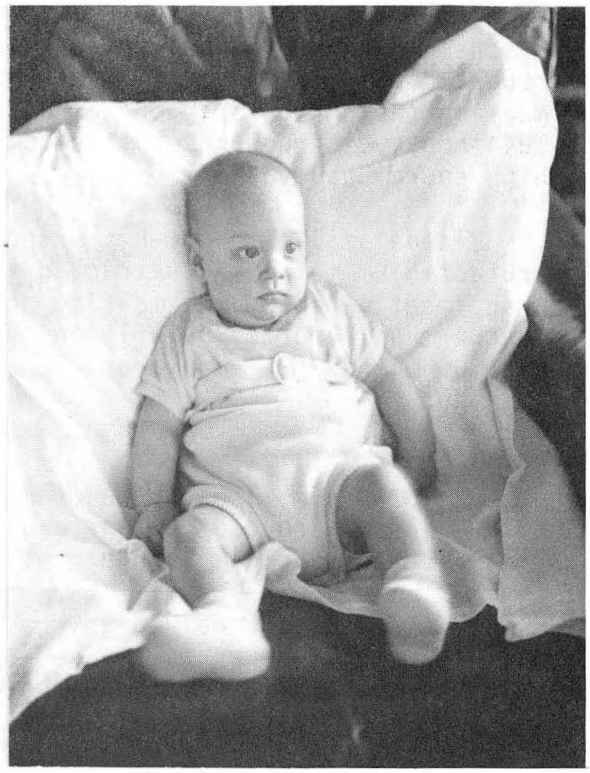
In my mail, I received a very complete reply from Al Florin, who had looked up the references I asked him to check in the Los Alamos reports--this is information needed for the "Table of Isotopes."

Glenn W. Giddings (Personnel Department of General Electric) wrote that he has written to Vance Cooper and Fred Albaugh to tell them that if they are interested in GE's program, he would be happy to meet with them when he is in the Los Angeles area during the week of November 11. Giddings mentioned that he will be in the San Francisco area early in the week of November 18 and hopes to meet me then. Another letter also arrived from General Electric. This one was from Guy Suits, who stated that the Manhattan District will not allow me to receive any reimbursement for the time I spend consulting on their program because of the support furnished by the District for my salary at the Radiation Laboratory. Suits said he hopes that I will still be willing to serve on their committee.

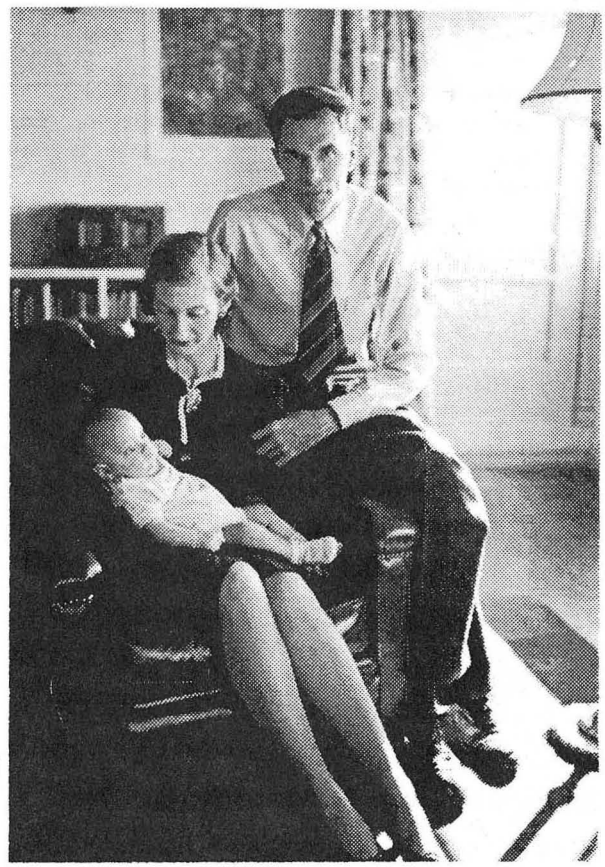
I then dictated replies to some of these letters. At 4:15 p.m., I went to the meeting of the Connick-Seaborg groups in Gilman Hall.

Herman Robinson came to our home to have dinner with Jeanette, Helen and me. Herman brought his camera with him and took pictures of Peter, one of which will be used in our Christmas cards.





Peter Seaborg
November 5, 1946



Peter, Helen, and Glenn Seaborg
November 5, 1946

Wednesday, November 6, 1946

This morning I replied to a letter I received yesterday from William F. Meggers, who had asked a number of questions about chemical evidence for the electronic structure of the transuranium elements.

A week ago I received a letter from a high school girl, Ruth C. Behr, who had some questions about the transuranium elements and wanted to know if she should become an atomic physicist. In my reply I referred her to my recent article in Science and then said, "It is very difficult to advise on the question of whether or not you should become an atomic physicist. There are a few women in this field, but of course they are the exceptions." I suggested that she continue with science in high school, and decide on this question when she is ready to go to a university.

In a note to Joe Kennedy, I informed him that the Kennedy, Seaborg, Segrè, and Wahl article, "Properties of 94-239," is scheduled to appear in the next (October) issue of The Physical Review, and asked him how many reprints he wants.

Another letter went to Donald A. McPherson (John Wiley), who had asked about the advisability of publishing a book entitled Chemical Consequences of Atom Decay. In my reply I said that I expect that Milton Burton's book on radiation chemistry will adequately cover the field of "hot atom" chemistry. McPherson also asked if I would consider writing a small book on the transuranium elements. To this I replied that I do not want to make more commitments until the Plutonium Project Record is further on the road toward publication and it is known how much of the material on the transuranium elements will be cleared.

I sent a memo to Joe Hamilton, saying that I have learned that Winston Manning will entertain a request from him for three or four micrograms of Pu^{238}, which Joe wants for his tracer investigations on the physiological role of plutonium.

A letter arrived today from Gordon Leader, along with one of the papers from Volume 14B that I had requested. Gordon said that Manning will mail a couple of the other papers. He also provided me with the status of some of the other papers and concluded by saying that Manning and he have not yet finished their writing. Manning, he reported, plans soon to go into complete retirement from ordinary laboratory affairs so that he can get his writing done.

When I made the rounds of the labs, I learned that Tom (Leon O.) Morgan took and passed his prelims. His committee consisted of Axel R. Olson, Gerald E. K. Branch, William D. Gwinn, William F. Giauque, Ralph R. Hultgren, and Robert B. Brode.

Thursday, November 7, 1946

The topic for today's 10 a.m. Chem 123 lecture was fission. After the lecture I stopped in to see Geoffrey Wilkinson before going up to the hill.

W. H Sullivan wired that he has changed his Chicago visit to November 13 through 16 in order to coincide with my trip.

Another telegram arrived today, an invitation from Charles Waring of the University of Connecticut to speak at their chemistry colloquium while I am in the East. I immediately wired, "Sorry. Can't make it on this trip. Letter follows." I then dictated a letter to Waring to explain in detail about my upcoming schedule for that period of time and to say that perhaps I'll be able to make it on some future trip.

Welden Reynolds (Encyclopaedia Britannica) replied, in a letter dated November 4, that Edward Teller is interested in the chart of isotopes, not in the "Table of Isotopes," in his article on NUCLEUS for the Encyclopaedia. Reynolds then said that the present article on RADIOACTIVITY will be supplanted by two new articles: RADIOACTIVITY, NATURAL (written by Sir James Chadwick) and RADIOACTIVITY, ARTIFICIAL (or induced), which he wants me to write.

I also received a nice letter from Lila Miller Negrete, a friend from UCLA days, who wanted the procedure for obtaining C^{14} since a business acquaintance of hers, Dr. Boris Krichesky, wants to use "tagged" carbon in endocrine research. I replied today and referred her (and Dr. Krichesky) to Dr. Paul Aebersold and my article in Science magazine of June 14, 1946.

I mailed a rough draft of "Thermal Neutron Fissionability of Heavy Nuclei" to Winston Manning. In my covering letter I made a few specific comments on the paper and then asked that he send us a copy of the finished paper before it appears in the PPR, Volume 14A.

Friday, November 8, 1946

I talked with some of the fellows and then acknowledged receipt of Marty Studier's paper on the fission cross section of Np^{238} but asked him to prepare a table of the essential data for the calculation and give it to me when I am in Chicago next week.

Templeton and Howland prepared a memo, "Report on polonium, bismuth, and lead isotopes." The data in this will be used in the revision of the "Table of Isotopes."

I read a list of corrections and comments from Truman Kohman on the "Table of Isotopes." Truman also said, "I intend to send my recommendation on the use of 'nuclide' to Science or Physical Review soon. I will send you a copy. I would very much like to see it adopted in the title of the Table and Chart. If this does not seem advisable at present, I strongly recommend that 'nuclear species' be substituted for 'isotopes.'" [Kohman strongly believes that the term "isotopes" should be used only in the restrictive sense, i.e., to refer to species that all have the same atomic number and not as we do in the "Table of Isotopes."] Truman also said that he now agrees that the curie should be redefined officially as 3.7×10^{10} disintegrations per second exactly.

Edgar Westrum sent me the information I asked Darrell Osborne to get on the most reliable values of the disintegration constants of Pu^{239} and

Pu^{240}. Westrum commented that he has signed a consulting contract with Argonne, saying "They seem to value my services at 150% higher per day than does the Radiation Laboratory."

A note arrived from E. O. Wiig, in which he said that they plan to have a dinner in downtown Rochester for me since Professor Noyes will not be in town at the time of my Harrison Howe Lecture.

There was another meeting on piles today, attended by Ernest Lawrence, Wendell M. Latimer, Amos Newton, Charles H. Prescott, Jr., J. Robert Oppenheimer, Edwin M. McMillan, Iz Perlman, Glenn T. Seaborg, Robert Thornton, Emilio Segrè, Robert Serber, Spofford G. English, and Luis W. Alvarez.

Helen went to her Russian class with Lee Perlman this evening.

Saturday, November 9, 1946

I received and read Ernest Lawrence's covering letter and enclosed suggested simplified procedure concerning the allocation and custody of top secret materials, which we prepared in response to General Groves' directive of August 5. Most of the rest of the morning was spent reading reports.

Today's football game was played in Los Angeles; California was beaten by Southern California by a score of 14 to 0.

Helen's mother (Mrs. Iva B. Griggs) arrived to visit with Helen while I am out of town. At about 5:45 p.m. I left on the "City of San Francisco" for Chicago.

Sunday, November 10, 1946

En route to Chicago. Much of the trip was spent working on my writing projects, including the "Table of Isotopes."

Monday, November 11, 1946

I arrived at the Chicago Northwestern station mid-morning and took a taxi to the University of Chicago campus to visit with people in the New Chemistry Building.

Later in the day I went to the Englewood station and boarded a pullman for Schenectady.

[In Berkeley, Amos Newton sent his monthly report covering the work of the Berkeley group from October 1 to October 31, 1946, to C. E. K. Mees. Newton discussed several items from the General Information Meeting in Oak Ridge that he attended: C^{14}, element 43, and a paper on photosynthesis. Specifically about Berkeley, Newton mentioned the beginning of the operation of the 184-inch cyclotron; the status of the new radioactive lead isotopes, including the reassignment of a 68-minute lead (from 13-hour bismuth) to Pb^{204}; and the relatively unsuccessful attempts to produce protactinium metal.]

Tuesday, November 12, 1946

In Schenectady. I attended an all-day meeting of the General Electric Nucleonics Project Consultants. In attendance were consultants H. A. Bethe, W. K. Lewis, P. H. Morrison, Glenn T. Seaborg, E. P. Wigner, W. H. Zinn, guest Enrico Fermi, and members of the General Electric staff H. A. Winne, C. G. Suits, Zay Jeffries, D. C. Prince, K. H. Kingdon, W. H. Milton, Jr., A. B. Greninger, B. R. Prentice, J. J. Smith, C. W. La Pierre, and D. Cochran. General conclusions of the meeting were:

1. The proposal for a pile to be designed at Schenectady looks sound so far. Work along the lines proposed should continue.

2. This pile proposal fills a gap in the overall national program.

3. A less promising pile should not be constructed in an attempt to obtain useful power quickly by a short-cut procedure.

4. The critical experiment should be carried out at the more remote Sacandaga Road location.

5. Considerably more theoretical and experimental study is needed to settle questions of pile design.

6. It will probably be desirable to move some of the fuel rods as well as parts of the reflector to control the pile.

[In Berkeley, Iz Perlman requested authorization from Russell Ball of the Area Office for two 60-inch cyclotron bombardments: 2000 μah of deuterons on a 200 mg thallium backing plate, and 1500 μah of deuterons on 25 mg Pu^{239} on an interceptor target. Iz also gave the Chem 123 lecture at 10:10 a.m., covering the topic "piles" and announcing a midterm for December 5.]

Wednesday, November 13, 1946

I returned to Chicago by train and took a taxi from the Englewood station to the Windermere East Hotel.

[In Berkeley, Iz Perlman (and I) received a number of corrections and additions to the "Table of Isotopes" from Maurice Goldhaber.

Helen had tea with the Chemistry Department wives at Glatha Latimer's home.]

Thursday, November 14, 1946

In Chicago. At the laboratory I read a letter from Edgar Westrum containing some comments about the specific activity paper (Pu^{239} and Pu^{240}) for the Plutonium Project Record. Westrum noted that he will be in Chicago from Friday afternoon through Sunday.

I spent considerable time with W. H Sullivan. We discussed data for the "Table of Isotopes" and the chart of isotopes.

[In Berkeley, Iz Perlman gave the Chem 123 lecture and talked about accelerators.]

Friday, November 15, 1946

In Chicago. I worked with Gordon Leader, Joe Katz, Leonard Katzin, and others on the papers for the Plutonium Project Record. Al Ghiorso is also in Chicago, working at the Argonne pile on fission cross section measurements.

The October issue of The Physical Review has finally been issued and contains the "Letter" "Properties of 94(239)" by Kennedy, Seaborg, Segrè, and Wahl [70, 555 (1946)]. This article was received by the journal on May 29, 1941 but was voluntarily withheld from publication until the end of the war. It was necessary to make a few omissions, including any value for the slow neutron fission cross section of 94^{239}, to conform with declassification rules.

[In Berkeley, Mary Millard sent a telegram and a follow-up letter in my name to the Publications Manager of the American Institute of Physics, ordering reprints of this article for the Department of Chemistry, the Department of Physics, and Washington University.]

Saturday, November 16, 1946

In Chicago. I again talked with the Chicago men. Ed Westrum is spending a few days here consulting; we discussed some questions about the PPR papers and the "Table of Isotopes." Westrum will spend some time at the end of the year with us in Berkeley.

[Oregon State defeated California in Memorial Stadium today--28 to 7.]

Sunday, November 17, 1946

In Chicago. I went over the talk I will give tomorrow evening in Rochester. Later I had dinner at the home of the Royal Smiths (parents of my former secretary, Edrey Smith Albaugh, 9514 S. Damon St., Beverly Hills--a suburb of Chicago).

I left Chicago (Woodlawn station) about 10:20 p.m. via North Shore Limited to travel to Rochester.

Monday, November 18, 1946

I arrived in Rochester about 11 a.m. and was met by Edwin O. Wiig, who took me to a hotel.

The Harrison Howe Lectures are dedicated to Dr. Harrison E. Howe, a founder and charter member of the Rochester Section of the ACS and for many years editor of Industrial and Engineering Chemistry. It is the aim of the Lectures to stimulate enthusiastic interest in problems of importance to the future of chemistry and chemical engineering.

The program this evening consisted of a roast young turkey dinner and remarks by Norris Embree, Section Chairman; Kenneth C. D. Hickman,

Toastmaster; and John A. Leermakers, who introduced me and my talk, "Plutonium and Other Transuranium Elements."

Harrison Howe Lecture, November 18, 1946, left to right: John A. Leermakers, Glenn T. Seaborg, Norris Embree.

[In Berkeley, Bernadine Bertnik was hired as a junior laboratory technician.

The newly appointed members of the Atomic Energy Commission--David E. Lilienthal, Robert F. Bacher, Lewis L. Strauss, William W. Waymack, and Sumner T. Pike--visited the Radiation Laboratory today. Lawrence, Latimer, and others conferred with them.

Helen went to a cocktail party at the Claremont Hotel, to celebrate the completion of the 184-inch cyclotron. Later she attended a meeting of the Child Development Section of the Berkeley Faculty Wives Section Club; the subject tonight was "Toys."]

Tuesday, November 19, 1946

In Rochester. I visited and had a tour of the Eastman Kodak Company plant, where I talked with my friend from UCLA days, Jimmie Lu Valle.

In the afternoon I gave a seminar for the Department of Chemistry at the University of Rochester. Edwin O. Wiig and other members of the chemistry staff took me out for dinner. I left Rochester by train at about 10:15 p.m. to return to Chicago,

[In Berkeley, the Chemistry 123 lecture was given by Iz Perlman on the preparation of isotopes.]

Wednesday, November 20, 1946

I arrived in Chicago about 7 a.m. and took a taxi from the Englewood station to the Windermere East Hotel. I then went to the Argonne National Laboratory on campus where I met the newly appointed members of

the Atomic Energy Commission--Lilienthal, Strauss, Bacher, Waymack, and Pike--who were visiting the lab this morning.

Truman Kohman invited me to have dinner with him and his wife Jane this evening. Unfortunately, I was not feeling well and declined. I returned to my hotel and went to bed.

Thursday, November 21, 1946

In Chicago. At the laboratory I consulted with more of the men about papers for the Plutonium Project Record.

I left Chicago (Northwestern station) about 7:30 p.m. to return to Berkeley on the "City of San Francisco."

[In Berkeley, Iz gave the regular Chem 123 lecture; today's topic was methods of cataloging nuclear species.]

Friday, November 22, 1946

Enroute to Berkeley on the "City of San Francisco."

[In Berkeley, Burris Cunningham has left to visit Chalk River Laboratory in Canada.

Helen and Lee attended their Russian class.]

Saturday, November 23, 1946

I arrived in Berkeley about 8:30 a.m. and first took a taxi home, where I dropped my bags and checked on Helen and Peter.

At the laboratory I found an enormous stack of correspondence, most of which will have to wait until next week for answers. There has been some confusion about ordering reprints for the article, "Properties of 94(239)." We have tried ordering them without a reprint order blank, but Margaret S. Griffin, Publications Manager of the American Institute of Physics, wrote and explained that the reprint blanks were sent to Kennedy. Today I wrote Kennedy and told him that we want 300 with covers for the Chemistry Department and 150 without covers for Segrè and the Physics Department. Other letters I noted included a letter from Robert Gould of C & E News, who wrote that he and Jim Crowe have concluded that Walter Murphy (Editor of C & E News), will want to publish my Harrison Howe Lecture in his publication. Gould wants it cleared for publication for the December 25th issue.

I found an advance copy of Fortnight, sent by Kenneth Cooperrider, Associate Editor, containing an article about me. This is the second issue of the magazine--Cooperrider also sent me a copy of the first issue.

I read a thank-you note from Alexander Silverman of the University of Pittsburgh for some drawings I sent him earlier this month.

Unfortunately, Stanford defeated California, 25-6, in today's "Big Game," played in Memorial Stadium. Helen and I went to the game with Jim

and Rebecca Cason. California has had a miserable season this year under Coach Frank Wickhorst; undoubtedly he will be replaced. However, UCLA has had a successful year and will be the Pacific Coast Conference representative in the Rose Bowl.

Sunday, November 24, 1946

Part of the day was spent in continuing to look over my correspondence. I noted, for example, a letter from Charles Waring of the University of Connecticut. Waring was disappointed that I was unable to include a visit to their university on my recent trip but has invited me to address their Chemistry Colloquium any time I visit the East. Waring mentioned that he is still interested in finding a young nuclear inorganic man for their Department of Chemistry.

Monday, November 25, 1946

I read a copy of memorandum CL-WHS-10 from W. H Sullivan. The memo describes the need for and evolution of his hexagonal chart of isotopes.

I read a November 7 letter from Robert F. Gould (C & E News). Gould thanked me for the information on my talk at Rochester and then went on to express concern about Daniel Wilkes (Public Information Office, University of California). He emphasized that Wilkes should not use material written for ACS meetings in full until it has been released by the ACS. In particular, Gould said that he does not want to be scooped by Science News Letter, a publication on which Wilkes works as a part-time correspondent.

Roger J. Williams of the University of Texas sent me (November 11) a short article describing his new book, The Human Frontier.

In a November 14 letter Ray Stoughton thanked me for the information about the U^{233} sample about which he had asked. Ray also mentioned that Latimer is getting a copy of their (Clinton) monthly report.

In a notice from the Graduate Division I read that David Templeton, who has passed his prelims, has been assigned a thesis committee comprised of Seaborg, S. G. English, and E. M. McMillan. Leon Owen Morgan, who also has passed his prelims, has a committee composed of Seaborg, R. E. Connick, and R. B. Brode.

I read the covering letter, dated October 30, that Lawrence sent to Groves for the document Oppenheimer, Hamilton, and I prepared as an improvement of Groves' directive about the handling of top secret material: U^{233}, U^{235}, and Pu^{239}.

I received a copy of Robert Gould's summary of my Harrison Howe Lecture. Gould suggested that, if I have corrections or changes, I should call his Chicago office (or send him the changes).

A proof of the present article in Encyclopaedia Britannica on "Radioactivity" arrived from Welden Reynolds while I was out of town. He asked that I return it when I'm finished with it.

Leslie G. Cook wrote a long, newsy letter from Chalk River, where he serves as the head of the chemistry division. Cook suggested that March or April would be an interesting time for Iz or me to visit. He will send us their unclassified reports and let us see the declassified lists of reports. He said they plan to declassify the 4n + 1 series (except for the energies and periods of the two leading members) and send the mutilated reports to Albert C. English, now in Chicago, so that an article on this material can be written up for publication.

Tuesday, November 26, 1946

Present at today's group meeting were Asprey, Beaufait, Blaedel, Bockhop, Bradley, Covey, Eyring, Ghiorso, Goeckermann, Heppler, Hopkins, Huffman, James, Jentsch, La Chapelle, Magnusson, Miller, Morgan, Newton, O'Connor, Perlman, Prescott, Reynolds, Robinson, Seaborg, Simkin, Stewart, S. Thompson, R. Thompson, and Werner. Much of the meeting was taken up with Ghiorso's report on tentative results of the fission cross section measurements at the Argonne heavy water pile obtained during his recent (November 13 to 21) Chicago trip. For example, the fission cross section of Np^{238} is about 1500 b, using the fission cross section of Np^{237} as about 0.02 b. When one assumes 550 b for the neutron absorption cross section of Am^{241}, the fission cross section of Am^{242} is greater than or equal to 1000 b.

Perlman talked about the recent bombardment of lead with 200 Mev deuterons at the 184-inch cyclotron and the fractions isolated so far. O'Connor discussed the preliminary results on the chemical fractions isolated from a natural uranium target that was bombarded for less than 1 μah with 200 Mev deuterons, again on the 184-inch cyclotron.

Prescott described some calculations for Lawrence's heterogeneous pile. Finally, I announced that McCullough will spend full time here, as a consultant, starting in February, in order to equip an x-ray laboratory and use it for molecular structure determinations. I also mentioned Westrum's arrival date for his stay with us next month.

* * * * *

I began a discussion on the broad topic of "radiochemistry" in Chem 123 this morning, mentioning such things as the properties that can be determined by tracer techniques.

Back on the hill I read a letter from James McCullough; he wrote that his recent trip to Argonne was interesting and informative and that everyone was very helpful. He mentioned that we should have at least two of the 9 cm cameras although the x-ray unit will actually take four cameras at once. McCullough added that he will come to Berkeley again when the rest of the x-ray unit arrives.

I wrote to Quentin Van Winkle at Ohio State University to let him know that I have heard he is about to receive his Ph.D. and that we would like him to join our group here. I followed this by asking George Everson to send Van Winkle a personnel security questionnaire.

Later I read reports and journals.

Wednesday, November 27, 1946

After handling some administrative matters, I took care of some of my pending correspondence. Professor Paneth's manuscript, "The Making of the Missing Chemical Elements," (his Leeds lecture) arrived recently. Today I wrote to tell him that I think he has made a fine presentation of the subject. I also gave him the reference [Phys. Rev. 70, 555 (1946)] for the Kennedy, Seaborg, Segrè, and Wahl paper entitled "Fission Properties of 94^{239}."

Earlier this month Walter J. Murphy (Editor of Industrial and Engineering Chemistry) asked me to review the manuscript, "Measurement of Radioactive Tracers, Particularly C^{14}, S^{35}, T, and Other Longer-Lived Low Energy Activities," by W. F. Libby. My extensive travel this month prevented me from responding, and Murphy wrote again to ask for my review. Today I wrote to Murphy and said that the paper is worth publishing; I added a few suggestions and comments on the paper.

Elmer Hutchisson (Editor of Journal of Applied Physics) asked (letter of November 19) for a photograph of americium to be used in a review paper of Professor Philip Morrison. I mailed him today a photograph of the first americium isolated (as the hydroxide) in macroscopic form. I offered to send him any of the pictures in my recent Science article "The Transuranium Elements."

B. P. Geyer, Chairman of the Abstract Committee of the Vortex, wrote offering to construct an abstract of my address from my article called "The Transuranium Elements" at the California Section meeting (October 14) for publication in the Vortex. Geyer said he was suggesting this since he realizes how busy I am. In a reply today, I agreed to his proposal and offered to check it over if he wants.

D. L. Tabern (Abbott Laboratories) wrote and mentioned a successful "trial" of the Lewellen records. The main point in his letter was about ICCASP. Tabern related that a number of members of the Chicago science group have resigned because of the radical posture of the organization; he asked whether I am still a member.

I also mailed a check for $11.00 to William Ackerman, Graduate Manager, UCLA, requesting two Rose Bowl tickets.

Thursday, November 28, 1946 (Thanksgiving Day)

During the later morning and early afternoon Stan Thompson and I went to Mira Vista for a round of golf (SGT-103, GTS-106). Helen, Jeanette, Pete, and I had our Thanksgiving dinner together at home after the golf game. I arrived home later than I should have, and the dinner had passed its peak! Jeanette was very unhappy with me.

Friday, November 29, 1946

I worked on some of my writing projects and talked with various fellows for a time this morning before reading the mail.

A memo arrived from M. A. Stewart, Assistant Dean of the Graduate

Division, stating that David Templeton's thesis committee has been reconstituted as follows: I. Perlman, S. G. English, and E. M. McMillan.

Later I mailed Gordon Leader a description of the status and comments on a number of papers for the Plutonium Project Record.

At home Helen reported on Peter's activities and said that Jo Owen stopped in to see her for a few minutes. Later Helen went with Lee to the Russian class in Wheeler Hall.

Saturday, November 30, 1946

I read a letter from Dr. Vanancio Deulofeu, who was disappointed that I could not send a manuscript to Ciencie e Investigacion (my reply of October 4). He suggested sending a copy of some general lecture and my authorization to publish it in Spanish.

I wrote to Sigfred Peterson, saying that I have written statements about him for a number of positions. I said, however, if the application to Clinton Laboratories implies that he is interested in nuclear research, I want to renew my offer to him to join our group here.

I then spent some time catching up on my report reading.

Sunday, December 1, 1946

Iz Perlman and I played 18 holes of golf at Mira Vista Country Club this morning (IP-103, GTS-103).

Later I gave some thought to the article on artificial radioactivity that I have promised to prepare for the Encyclopaedia Britannica; I also worked on the Seaborg-Segrè article on the transuranium elements for Nature.

Helen's mother, who has been visiting us, left by train for her home in Los Angeles (1637 S. Van Ness Avenue).

Monday, December 2, 1946

I handled some of my correspondence before making the rounds of the labs.

I wrote to Ruth Rogers and asked her to send me the information on Sigfred Peterson's personnel card so that I can write some letters of recommendation for him. In addition, a memo went to George Everson, requesting him to send a Personnel Security Questionnaire (PSQ) to Peterson and explaining that we want to interest Peterson in joining our group. I also sent Peterson's name to Professor Charles Waring at the University of Connecticut, as a possible candidate for the position he has open.

Another letter went to Edwin O. Wiig to give him an accounting of my expenses for the Harrison Howe Lecture. Also, a corrected copy of the Harrison Howe Lecture was mailed to Robert F. Gould for publication in whichever journal of the American Chemical Society he feels is appropriate.

I wrote to Gordon Leader to ask him to delete a sentence in Chapter XXII of Volume 14A of the Plutonium Project Record. I mentioned that I shall send Chapter XXI, which I have rewritten, as soon as it is typed.

Iz sent Kay Way a photostatic copy of the "Table of Isotopes," which she requested. He told her that we hope to issue it around the first of the year.

In today's mail was a thank-you note and honorarium check from V. Boekelheide (Department of Chemistry at the University of Rochester) for the seminar I gave there on November 19.

Edgar Westrum wrote that he will arrive about 5 a.m. on December 27 and leave on January 5. He said, "This will leave at least nine days in which to make a significant experimental contribution." Westrum said he plans to send a list of the equipment needed for his experiments soon. I routed the letter to Blaedel, Eyring, Cunningham, and Perlman.

Sigfred Peterson wrote to ask me to write another letter for him--this time to C. C. Vernon at the University of Louisville. Peterson mentioned that he may visit Oakland this winter.

Tuesday, December 3, 1946

While attending a meeting this morning, I received a telephone call from AEC Commissioner Robert Bacher (in Washington, D. C.), inviting me to be a member of the nine-member statutory General Advisory Committee to the AEC. I immediately accepted. Bacher informed me that there will be about six meetings a year; the stipend will be $50 per day. I reported the news of my appointment to Ernest Lawrence.

After the Chemistry 123 lecture in which I continued lecturing on radiochemistry--adsorption techniques, radiocolloids, volatility properties, etc.--, I stopped in to see Wendell Latimer to inform him of the GAC appointment.

Back on the hill Iz and I prepared a midterm for the Chemistry 123 class.

Helen and I had dinner at "Jack's" in San Francisco with some visiting Cyanamid people--R. C. Gaugler, Robert C. Swain, and Wilbur G. Malcolm. Ernest and Molly Lawrence and Melvin and Genevieve Calvin also attended.

Wednesday, December 4, 1946

I handled some administrative matters before reading the mail, which included a note from Truman Kohman, who described his efforts to find the information I requested on radium samples and said that he will ask Ruth Rogers to mail the table next week. Truman also mentioned that early last week he and his wife Jane attended John Simpson's wedding, which was held in Owen, Wisconsin. Truman said he acted as an usher.

Al Ghiorso gave me a copy of some information on the Pu^{240} and Pu^{238} content of plutonium recovered in a soil sample from the Alamogordo bomb test (July 16, 1945), as deduced from slow neutron fission and alpha-particle emission measurements. Al is mailing this information to Art Jaffey, at Jaffey's request.

As usual, I went through the labs to talk with the men about their research.

Thursday, December 5, 1946

The Chemistry 123 class had the second midterm examination this morning.

At 12 noon there was a luncheon for members of the city of Berkeley staff in Room 12 (Conference Room) of Bldg. 8. After a few brief remarks by George Pettitt (Introduction), Luis Alvarez (Radiation Laboratory Program), Glenn Seaborg (Nuclear Chemistry), E. O. Lawrence (Welcome and Conclusion), the visitors were given a tour of the laboratory.

Miss Yvonne Crump of Oakland wrote a letter of appreciation for my lecture in the Extension Division series of talks on nuclear energy.

Iz and I spent much of the afternoon grading the midterms for Chem

(486 Michigan Avenue, Berkeley) for dinner.

Friday, December 6, 1946

I had an appointment in First Aid for a blood test and urinalysis.

Iz and I completed grading the Chem 123 midterm examination this morning. Two members of the class (not our graduate students) obtained grades of 100--P. W. Gilles and Z. Z. Hugus.

In the mail was a letter from M. Goldhaber with more information and comments for our "Table of Isotopes."

Saturday, December 7, 1946

I talked with some of the fellows, but for much of the morning I read some of the accumulated reports.

A letter arrived from Collin H. Alexander, who attended my Harrison Howe Lecture. Alexander said that he believes the announcement of the preparation of the pure compounds of americium and curium ranks in his mind with Willard Gibbs' paper on the phase rule.

This evening Helen and I went to the home of Gerhard and Nellie Rollefson (14 Bonnie Lane, Berkeley) for dinner.

Sunday, December 8, 1946

I worked for a while on an outline for the Britannica article, "Artificial Radioactivity," and the article for Nature.

Helen and I went to Kezar Stadium in San Francisco to watch the San Francisco 49ers defeat the Los Angeles Dons, 48-7. Frankie Albert made two beautiful passes to Alyn Beals for two of the 49ers' seven touchdowns.

During the evening Jo Owen stopped over to see us.

Monday, December 9, 1946

This morning I mailed Joe Kennedy a copy of the article Segrè and I are preparing for Nature (a history of the transuranium elements) and asked for his and Wahl's comments. I also told Joe about Segrè's meeting with Stuart Scott, a patent attorney, in October.

In a letter to Robert Gould, I told him about the correspondence I have had with Dr. Venancio Deulofeu, who recently wrote that he would like to publish a Spanish translation of one of my lectures. I asked Gould if it would be proper to let him publish a translation of my Harrison Howe talk.

A letter arrived from Edwin Wiig, requesting my bill for my expenses to Rochester (I have already sent this to them) and telling me how much they enjoyed my lectures.

I read a letter from Sigfred Peterson, who thanked me for my recommendations and then went on to explain his position. He likes nuclear research, but he believes that he would prefer teaching. Peterson said that he has a personal reason for not joining us in Berkeley--because of the housing shortage, he and his wife would be tempted to live with his parents, but they feel they would be happier in a more independent life. I also received from Ruth Rogers the personnel information I requested about Peterson. Ruth suggested that I talk with Roy Thompson about him.

Later I wandered through the labs to check on the research.

Tuesday, December 10, 1946

Present at this morning's group meeting were Asprey, Bockhop, Bradley, Covey, Cunningham, Bernie Fries (a visitor), Hopkins, Howland, Huffman, James, La Chapelle, Lilly, Magnusson, D. Miller, Morgan, Newton, O'Brien (patent man), O'Connor, Perlman, Prescott, Reynolds, Robinson, Seaborg, Simkin, Stewart, Templeton, R. Thompson, and Werner. Werner reported on his 500 μah bombardment of 5 mg of Pu^{239} with 22 Mev deuterons. He found a 50-hour activity that may be Am^{240} and a 12-hour activity that is probably Am^{237} or Am^{239}. James discussed his 44 Mev helium ion bombardment of Pu^{239}, in which he found two half-lives in the region of 1 to 10 days that may be Cm^{241}, Cm^{238}, or Cm^{239}.

Templeton talked about the various fractions from his bombardment of lead with ~200 Mev deuterons (184-inch cyclotron) (~0.1 μah). Roy Thompson noted that he has found activities in the zinc, copper, nickel, cobalt, iron, manganese, and chromium fractions in a sample of copper bombarded with 200 Mev deuterons. Perlman asked about the smallest number of particles that has been observed to be ejected in the bombardments with the ~200 Mev deuterons, and O'Connor said they may have observed the d,p2n reaction on U^{238}.

* * * * *

The midterm was returned and explained to the Chem 123 class this morning. Today I talked about the natural radioactivity of the light elements (Lu, Os, K, Rb, and Sm) and the four missing elements (43, 61, 85, and 87).

Much of the rest of my day was spent answering my correspondence. In answer to a form letter from Charles Coryell, I sent him a few corrections to the Journal of the American Chemical Society compendium of fission product activities produced on the Manhattan Project. I told Coryell that it is a nice compilation and I am sorry that I do not have time to look it over more critically.

On November 4 S. Reid Warren, Jr. wrote, asking about the names for elements 43, 61, 85, and 87. Today I replied and explained that the names he listed were given by people who believed that they had discovered the elements; we now know that these discoveries were in error. I then gave him the present status of the names.

Kurt Kraus and Ray Stoughton wrote on November 4 and asked me to give

two lectures on March 10 and 11 for the Clinton Laboratories training program. Today I wrote, apologizing for being so slow in replying and saying that my commitments are so heavy that I cannot accept.

Paul W. Merrill (Mount Wilson Observatory) asked, in a letter dated November 6, for information on the spectra of plutonium, americium, and curium--he would like lists of the strongest lines for comparison with stellar spectra. In my reply today I explained that, although the spectra of some of the transuranium elements have been observed, the information has not yet been declassified.

John H. Lawrence wrote me a note on November 15 and asked for an evaluation of John Gofman. In reply I wrote today that I believe Gofman is the best graduate student I have had and, further, that he is one of the two or three best young chemists that I have been associated with on the Plutonium Project. I concluded, "He would be a very good man for your group and I believe that it would be a mistake if the University of California has an opportunity to add him to its staff and does not do so." Yesterday I received a request for an evaluation of Gofman, who is applying for a fellowship, from Donald C. Balfour (Director of the Mayo Foundation of the University of Minnesota). I wrote Balfour a letter similar to the one I wrote to John Lawrence.

I also wrote a letter of recommendation to W. A. Felsing, who inquired on November 15 about David H. Templeton. (Templeton is being considered for a position at the University of Texas.) In a very positive letter I said, "Templeton is a man that we would like very much to keep in our group here and we will feel his loss keenly if he should leave us. He is an outstanding man and I can recommend him practically without reservation."

Douglas Ryan wrote on November 18 in behalf of his high school chemistry class in Harper, Kansas, to ask "What is energy?" Today I replied that I believe he is probably interested in the principle of the equivalence of mass and energy which forms the basis for the release of energy from the nucleus of the atom. I referred to Einstein's equation and said that, in actual practice, only about 1/10% of the mass of the plutonium or uranium-235 changes into energy.

My comments on the coverage of the radiations and reactions for the production of the heavy elements in the Plutonium Project Record were sent to R. S. Mullikan at Argonne.

I answered a letter of November 20 from Richard Eisenberg of Brooklyn, New York, who asked a number of questions about atomic weights, valences, and electronic structure of the actinide elements. I also referred him to my article in Science (October 25, 1946 issue).

Recently I received a request from R. C. Anderson (Chairman, Department of Chemistry, University of Texas) for an evaluation of Sigfred Peterson, who is applying for a position there. I wrote my usual letter, saying that Peterson is well qualified for research in nuclear chemistry and in inorganic chemistry. A similar letter about Peterson went to James G. Stangby at Clinton Laboratories, whose request for an evaluation arrived today, and to Professor C. C. Vernon at the University

of Louisville (to whom Peterson requested I write).

Yesterday a note arrived from Quentin Van Winkle, who said that he is interested in my job offer but has already accepted a postdoctoral appointment at Ohio State University this year. I wrote him today to say that I am glad he is interested and to suggest that he fill out the PSQ within the next few months so that clearance will not be a bottleneck.

In reply to a November 25 letter from Neil Gordon, I wrote that I am concerned about the change in time for my lecture in their "Chemical Forum" because I had planned to leave Detroit in the evening to travel to Schenectady. I added that I am also concerned about the publication to follow the lecture since it is difficult to prepare an article that encompasses new and declassifiable material although this is not serious in the lecture itself.

A few comments also went to W. H Sullivan about his "hexagonal chart." I said that I prefer the simplicity of the chart with rectangular coordinates, but I believe that a chart of the "hexagonal" type should be prepared.

At 4:15 p.m. I attended the Connick-Seaborg group meeting in Gilman Hall; after dinner I went to the Research Conference meeting.

Today's C & E News had a sketch of me on the cover because I delivered the Harrison Howe Lecture.

Chemical and Engineering News, December 10, 1946

Wednesday, December 11, 1946

I made the usual rounds of the labs to check on the research.

Dean Latimer asked me to check about getting mass spectrometer analysis of C^{13} samples for Herbert A. Young on the Davis campus. I discussed this with Burt Moyer and then wrote a memo to Latimer, saying that Moyer feels that they can analyze a limited number of samples. I suggested that he have Young contact Moyer.

I wrote to Lyle R. Dawson at the University of Kentucky and suggested, if he is still interested in men, the name of Sigfred Peterson. I wrote, "As you probably know first-hand, Peterson is a good man although his personality must certainly be rated as 'distinctive.'" [Lyle, who was a member of our Chemistry Section C-I at the Met Lab, is now the Chairman of the Chemistry Department at the University of Kentucky.]

Helen went to the tea for the chemistry wives at Mrs. Alberta Porter's home (911 Oxford, Berkeley).

Thursday, December 12, 1946

I checked in with Miss Kittredge and then began talking about the four heavy radioactive decay series to the Chemistry 123 class.

I stopped in and talked with Geoffrey Wilkinson about his research before going up to the hill.

After looking over the mail, I made the rounds of the labs to check on the research. I then worked on the Seaborg-Segrè article.

Friday, December 13, 1946

Again I made my usual tour of the labs, worked on my PPR papers and the "Table of Isotopes."

Vance Cooper telephoned to discuss an offer for employment that he received from General Electric. He mentioned that he had written a letter to me, which I have not yet received, and I said that Helen and I plan to visit Los Angeles during the holidays and hope to see him and Mary then.

Saturday, December 14, 1946

This morning I answered a November 4 letter from Wm. May Venable, a consulting engineer with Blaw-Knox Company in Pittsburgh. Venable is interested in work on the observation of the K-lines of the x-ray spectra of the transuranium elements and recent work on uranium and thorium. In my reply I said that I know of no work on the x-ray spectra of the transuranium elements although they often have the K x-rays associated with their radioactive rays; I explained that I have had no occasion to look into the literature on the x-ray spectra of uranium and thorium.

I also replied to a letter from Alice M. Bullock, dated October 16,

who asked if the University would be interested in a claim she has in the Black Hills, which seems to contain uranium. In my reply I informed her that the University is not interested in mining deposits but suggested that she contact the Bureau of Mines in San Francisco about an assay.

D. R. Hamilton wrote on December 5 that it has been decided to publish in book form only the more general lectures of the first and third days of the Princeton Conference (and thus not my lecture on the second day). He suggested that my more technical talk be published in Science with a footnote referring to the conference. Today I wrote that I would feel equally happy whether my talk is published or not since I did not prepare it with publication in mind. I then mailed Charles Coryell a copy of the paragraph about element 61, explaining the situation and asking for comments and improvements in order to make it conform better to the facts.

Barney Weissbourd and Herb Hyman replied to my query about the ICCASP. They said that a vast majority of prominent people originally associated with the organization are still members and they believe the organization continues to stand for the things it stood for when they joined. In addition, the newspaper article listing those who have resigned is misleading since it implies that everyone resigned en masse. Today I wrote Weissbourd to thank him for the information and to say that I do not plan to resign even though I am not in the midwest. I also wrote D. L. Tabern to inform him that I do not intend to resign from the ICCASP even though I cannot be active in the organization. I also told Tabern that I am pleased that the Chicago Section nominated me for the Baekeland Award.

On December 7 Robert P. Adams, Assistant Professor of English at Michigan State University, wrote to ask me the following question:

> In your present judgment, is continuous desirable social progress likely to result if scientific discoveries and inventions are in the future applied in world-wide practice as effectively as they have been since the Renaissance?

Adams wants this answer for a paper he is preparing on "The Social Responsibilities of Science in Some Renaissance Utopias and After." My reply was, "I am afraid that I must admit that your letter caught me without any very definite ideas on the question which you have posed. I have given the question some thought and have had some lively discussion with my colleagues about it, but I do not believe that I am ready to give you an answer."

I answered a December 9 letter from Francis E. Blacet (UCLA Chemistry Department), who asked for an appraisal of Edward King. I wrote that I do not know King's work first hand as a scientist but he is well thought of and that I shall try to find out more about him. I also recommended David Templeton as a possibility for UCLA. On another matter Blacet had asked about a supply of deuterium gas, and I offered to write Merle Randall, now connected with Stewart Oxygen Company, for whatever good it may do. I then mentioned that Helen and I will be in the Los Angeles area during the holiday and I shall try to see him.

In addition, I mailed Gordon Leader copies of the rewritten Chapter XXI, "The Actinide Series," for Volume 14A and noted that he should now have received from us all chapters for Volume 14A and all the papers for 14B, except one by Perlman and Seaborg, which he should receive next week.

Another letter went to Truman Kohman. I asked if he can spare a couple of milligrams of highly purified radium--we want to prepare pure radium metal to measure its structure and physical properties in order to compare them with the corresponding properties of americium metal, which should be radium-like.

Today I received the letter, begun on October 26, that Vance Cooper mentioned yesterday. He apologized for not acknowledging my letter sooner, explaining that the letter arrived as they were painting their house. He said that he appreciated my writing to Dr. Suits and that he just received a personal letter from Roy Beaton, who is enthusiastic about what General Electric seems to be doing at Hanford. Vance went on to say that, since he has heard nothing from G.E., he will limit his correspondence with Beaton to a general attitude. [I learned yesterday that Vance has now received an offer from General Electric.]

Brief notes went to Fred Albaugh and Vance Cooper to confirm our plans about visiting Los Angeles over Christmas. I gave them both my parents' telephone number and suggested to each a game of golf and a reunion of the Chicago crowd during the week of December 22.

I took Helen, Jeanette, and Peter to an open house at the laboratory for relatives of the employees. This featured a tour of the new 184-inch cyclotron. Peter was very social and smiley until he fell asleep.

Sunday, December 15, 1946

I put the finishing touches on one of the papers for Vol. 14B and then worked on the article called "Artificial Radioactivity" for the Encyclopaedia Britannica.

During the evening Helen and I left Peter with Jeanette and went to the Albany Theater to see the movie, "The Hurricane," starring Dorothy Lamour. We didn't stay for the second feature, "Barbary Coast" (Miriam Hopkins).

Monday, December 16, 1946

I took care of some routine things and talked with some of the men before looking over today's mail.

A check for the remainder of my travelling expenses and my $200 honorarium for the Harrison Howe Lecture arrived from Edwin O. Wiig, who said that he trusts C & E News will find me chosen "the man of the year."

Robert F. Gould wrote to thank me for my corrected manuscript of the Harrison Howe Lecture. Gould said that he, Murphy, and Crowe believe it should be published in Chemical and Engineering News.

A number of additions, corrections, and deletions arrived from W. H

Sullivan for our "Table of Isotopes" or, as Sullivan refers to it "Table of Nuclear Species." [Sullivan does not approve of our broader use of the term "isotope."]

I received a rejection notice from UCLA to my application for Rose Bowl tickets I mailed the material they sent me, along with the rejection notice, to J. D. McCullough in case it may be helpful in his bid for tickets.

I read a December 11 note from Leonard Katzin. Katzin said that Pete Hagemann found a lot of material on thorium chemistry that Rollefson apparently did not have when he wrote his chapter for Volume 17A (the survey volume on the production and properties of U^{233}). Leonard asked that I check with Rollefson to see if he is agreeable to having Hagemann make the necessary additions as co-author. I talked with Rollefson and today wrote Katzin to inform him that Rollefson agrees with the proposal and, in addition, will read the manuscript if Katzin wishes.

Two letters arrived from Ed Westrum, one of which included corrections to his plutonium specific activity paper for the Plutonium Project Record. Westrum said that he is making an all-out effort to complete at least three of his earliest plutonium calorimetry papers in time for the volume. In the other letter Westrum discussed the salary he expects next summer when he works with us:

> Consequently, from the laboratory's point of view, I think that my personal industrious contribution to research plus the beneficially catalytic effect my experience and direction can provide as far as the work of interest is concerned should merit $500 monthly. From my point of view, less than my academic salary would disparage my worth and would furthermore discountenance the plans in view of the additional expenses certainly involved in arranging and maintaining a temporary residence.

I wrote my usual letter of recommendation for Sigfred Peterson to John C. Bailar, Jr. at the University of Illinois, whose request arrived today.

At 10:15 a.m. Peter had his regular appointment with Dr. McAlear; Peter is progressing normally.

Tuesday, December 17, 1946

Our group meeting began on time this morning even though only five people were present: Asprey, Blaedel, Bockhop, Covey*, Cunningham, Eyring, Ghiorso, Hopkins, Huffman, James, La Chapelle*, Lilly, Lindner*, Magnusson, D. Miller, Morgan, Newton, O'Connor, Perlman, Prescott, Reynolds, Robinson, Seaborg*, Stewart, Templeton, R. Thompson*, S. Thompson, and Werner [* = present on time]. Lindner began the meeting with a talk about his recent bombardment of tin with 200 Mev deuterons. Silver, cadmium, indium, tin, and antimony fractions were separated and activity was found in all of them. He reported the detection In^{113*} grown from a tin fraction and a cadmium activity showing an x-ray decay of 7.0 hours and a beta-particle decay of 6.4 hours. R. Thompson then reported the preliminary results of the bombardment of copper with 200

Mev deuterons--a multitude of activities.

I then said that there must be a new style of nomenclature to describe the complicated reactions obtained with very high energy particles. I suggested a comma to separate the bombarding particle from the particles emitted and that all emitted particles be listed without punctuation between them.

There was then a discussion of a scheme proposed by James on the production of high energy beryllium ions from the collision of high energy alpha particles with beryllium nuclei. Templeton talked about the recent developments in the bombardments of lead with 200 Mev deuterons, and O'Connor reported on the latest results from his bombardments of uranium with 200 Mev deuterons. Blaedel described the program to be followed while Westrum is here.

* * * * *

I continued lecturing on the radioactive decay series at this morning's meeting of the Chemistry 123 class; I included a discussion of the electron structure of the elements in the actinide series.

Back on the hill I read a thank-you note, dated October 24, from Paul Lazarus for the copy of the "Table of Isotopes" I sent him.

A letter also arrived from Neil Gordon, who asked that I send him the text of my lecture for the "Chemical Forum" at Wayne University in Detroit by March 10 so that it can be mimeographed in advance.

I wrote a brief letter to Burns Chevrolet Company in Berkeley, asking to be placed on the waiting list for a new Chevrolet. I am still using Jeanette's 1939 Dodge sedan to commute to campus and the Radiation Laboratory.

At 4:15 p.m. I went to the meeting of the Connick-Seaborg groups in Room 102, Gilman Hall. After dinner I attended the Research Conference.

Wednesday, December 18, 1946

This morning I mailed Gordon Leader copies of a Seaborg and Perlman paper (22.22) for Volume 14B. In my covering letter I said that the tables need to be checked further but I want him to have the paper before he leaves (Leader is terminating at Argonne soon to become a member of the faculty at the University of Kentucky).

I made my regular rounds of the laboratories to see how the research is progressing.

John B. Condliffe of the Department of Economics sent me the following cartoon that he received recently.

Thursday, December 19, 1946

The lecture this morning in Chemistry 123 was about chemical and physical effects of high energy particles--radiation chemistry. As I often do, I stopped in and talked with Geoffrey Wilkinson before going up to the hill.

On the hill I met with Ernest Lawrence, who recently sent me a letter from Lee DuBridge, in which DuBridge promotes a nuclear laboratory for the Southern California area. I told Lawrence that I generally agree with the idea--later I returned DuBridge's letter to Lawrence, along with a memo to Lawrence confirming my comments.

I answered a letter, dated November 25, from M. G. Evans (Professor of Inorganic and Physical Chemistry, The University, Leeds, Great Britain). Evans addressed his letter to Dr. Sam Ruben, now deceased. It was referred to me since he asked for reprints on the Ruben, Kennedy, and Seaborg work on exchange reactions involving the work with radioactive iron. I explained to Evans that the work was never published since it was interrupted by war work--and now never will be completed nor published.

I read a copy of a letter Charles Coryell wrote to Professor Paneth about Paneth's paper, "The Making of the Missing Chemical Elements." Coryell offered a few suggestions on references for element 61 and then said, "I think your article is a very interesting one and plays an important role in the clarification of nomenclature and the fair distribution of credit in this exciting new field."

On Monday I received an article on accelerators here from Merritt L. Kastens (Associate Editor, Industrial and Engineering Chemistry). Kastens asked for my comments, and today I returned the manuscript along with my suggestions.

A humorous, congratulatory letter arrived from Paul Aebersold on my appointment to the GAC. Paul also sent holiday greetings to Helen and Peter.

Friday, December 20, 1946

The following telegram arrived for me this morning:

> A TWO DAY MEETING OF THE GENERAL ADVISORY COMMITTEE HAS BEEN SCHEDULED BEGINNING JANUARY THREE AT TEN A.M. COMMA NEW WAR DEPARTMENT BUILDING COMMA ROOM SIX ONE SEVEN FOUR COMMA WASHINGTON COMMA D.C. IF WE CAN ASSIST YOU IN OBTAINING TRAVEL OR HOTEL RESERVATIONS COMMA PLEASE CALL MISS REAMES OF MY OFFICE COMMA COLLECT PERIOD REPUBLIC SIX SEVEN ZERO ZERO COMMA EXTENSION SEVEN NINE ONE ONE NINE PERIOD DAVID E LILIENTHAL CHAIRMAN ATOMIC ENERGY COMMISSION

This will change our vacation plans; I asked Mary to begin immediately to work out the travel arrangements. This may be difficult because of the holiday season.

Joseph B. Work wrote from Richland, Washington, to ask me to speak to their Richland Technical Society when I visit Richland in January as a member of the G.E. Consultants Committee. I immediately wrote and accepted, suggesting the evening of January 20 as best and giving the title as "The Transuranium Elements."

I wrote to A. V. Grosse to give him my comments on Dr. Otto Kay's proposal that the ACS undertake the translation of Beilstein and Gmelin into English. I told Grosse that I have discussed the case of Beilstein with the organic chemists here who practically unanimously are opposed to the idea. I also said that I believe I favor the translation of at least some of the later volumes of Gmelin.

I requested of Rex Barton a document pass for Dr. Edgar Westrum, who will be here as a consultant from December 27 to January 5.

Wilma J. Bryan, a counting girl, terminated today.

Jo Owen visited us during the evening.

Saturday, December 21, 1946

Helen, Peter, and I took the Sante Fe to Bakersfield and then transferred to a bus for the rest of the trip to Los Angeles. Bob Engstrom, a neighbor of my parents, met us and drove us to my parents' home in South Gate. Jeanette will stay in Albany since she is now working as a clerk for the Department of Navy (housed in a temporary office complex under the grandstand seats at Golden Gate Fields in

Albany).

[In Berkeley, Lorraine Johnson, our clerk, married Richard Petch.]

Sunday, December 22, 1946

In South Gate. Peter's grandparents are quite impressed with their grandson, who is now able to sit alone.

Clayton Sheldon and I went to Rio Hondo Golf Club for 18 holes of golf (CES-108, GTS-94).

Monday, December 23, 1946

In South Gate. Mary called from Berkeley to say that she is forwarding a letter from David Lilienthal, which contains numerous forms that need to be executed by me. She said that she and Helen Bateson, travel clerk at the Radiation Laboratory, are working with Miss Reames in Lilienthal's office about my travel arrangements.

[In Berkeley, Christmas vacation begins for the University but not for the Radiation Laboratory.

A copy of our April 13, 1942 note on "Nuclear Properties of U^{233}" arrived from Hoylande D. Young. The paper was marked with deletions to make it declassifiable although it is not officially declassified.

Robert A. Lavender sent, on December 18, instructions for Kennedy, Wahl, and me to follow in order that his office can file patent applications in Great Britain and Canada for Cases S-52-A and S-52-B (basic cases on the chemical properties of plutonium).]

Today's Time and Newsweek contained announcements of the membership of the General Advisory Committee.

Atomic New Year

The five men whom President Truman appointed on Oct. 28 to the Atomic Energy Commission (NEWSWEEK, Nov. 4) had asked the Army to continue running the Manhattan District establishments while they toured the country for a firsthand look. Last week they visited the White House and emerged with the news that they would take over from the Army on Jan. 1.

Next day President Truman took further action under the Atomic Energy Act. With David E. Lilienthal as chairman, the commission consists of Lewis L. Strauss, Sumner T. Pike, William W. Waymack, and Robert F. Bacher. All are laymen except Bacher. As part-time scientific consultants to the commission Mr. Truman now appointed a General Advisory Committee consisting of four physicists, Lee A. DuBridge, J. R. Oppenheimer, I. I. Rabi, and Enrico Fermi; three chemists, James Bryant Conant, Glenn T. Seaborg, and Hood Worthington; a metallurgist, Cyril Stanley Smith, and a transportation engineer, Hartley Rowe.

The Atomic Energy Commission will receive a large assortment of laboratories and contracts for the establishment of more. These include the Hanford Engineer Works, Pasco, Wash., which besides producing plutonium is planning electric-power plants; the Clinton Laboratories at Oak Ridge, Tenn., now producing radioactive isotopes and planning a power plant; the Argonne National Laboratory in Chicago, which does basic research; the Knolls Atomic Power Laboratory, near Schenectady, N. Y., to be run by the General Electric Co., and Brookhaven National Laboratory, to be operated by a group of Eastern universities on the site of Camp Upton, Long Island.

Other new atomic-research plans:

¶ *Cyclotron.* Columbia University announced plans for a physics research center with a 300,000,000-volt cyclotron at Irvington-on-Hudson, N. Y. Work will be done there under a contract with the Office of Naval Research.

¶ *Cosmic Rays.* The Navy announced it will soon send physicists and cosmic-ray counters into the stratosphere in three AAF B-29s for research on the nature of the atom.

Last week the President:

¶ Appointed nine of the nation's top scientists and engineers as an advisory board to the five-man Atomic Energy Commission. The board: Harvard President James Bryant Conant; Dr. Lee A. DuBridge, president of the California Institute of Technology; Nobel Prize Physicists Enrico Fermi (University of Chicago) and I. I. Rabi (Columbia University); ex-Los Alamos Director J. R. Oppenheimer (University of California); Hartley Rowe, chief engineer of the United Fruit Co.; Chemistry Professor Glenn T. Seaborg (University of California); Cyril Stanley Smith, director of the University of Chicago's Institute of Metals; Hood Worthington, chemical engineer for E. I. Du Pont de Nemours & Co.

Tuesday, December 24, 1946

In South Gate. Fred Albaugh played golf with me at Rio Hondo (FWA-141, GTS-96). We talked about General Electric's plans for Hanford and his possible role there if he accepts their job offer.

Later on I looked over the following letter from David E. Lilienthal that Mary forwarded from Berkeley:

UNITED STATES
ATOMIC ENERGY COMMISSION
WASHINGTON 25, D. C.

20 December 1946

Professor Glenn T. Seaborg
University of California
Berkeley, California

Dear Professor Seaborg:

I wish to express the deep appreciation of the Commission to the response of all members of the General Advisory Committee to arrangements for the first meeting of the Committee in Washington on January 3 and 4, 1947.

Members of the Commission will meet with you at 10:00 a.m. on January 3 in the New War Department Building to assist you in planning the work and future schedules of the General Advisory Committee and to consider how the Committee can best advise on some of the long-range problems facing the Commission.

For your convenience, I am enclosing a copy of the Atomic Energy Act of 1946. The organization and functions of the General Advisory Committee are described in Section 2 (b) of the Act.

You will note in reading Section 2 (b) the fifty-dollar per diem allowance to members of the Committee while engaged in meetings and conferences, in addition to reimbursement for travel and other expenses while engaged in the work of the Committee. In order for us to make these payments, a distressing number of government forms must be executed by you pursuant to various statutory requirements. Will you, therefore, please complete the following enclosed papers and return them to us at your earliest convenience:

a. Affidavit to be executed by civil officers of the United States upon appointment to office.

b. Oath of office.

c. Personnel affidavit.

d. Affidavit respecting strikes against the Federal Government.

e. Secrecy agreement.

f. Withholding tax exemption certificate.

You will note that the first four items must be notarized and that the Secrecy Agreement must be witnessed.

May I renew my assurance that we will make every effort to assist you in obtaining travel and hotel reservations. Since accommodations will be crowded during the New Year holidays, may I suggest that if our assistance is required, you call Miss Reames of my office collect (Republic 6700 - Extension 79119) as soon as possible.

Sincerely,

David E. Lilienthal

David E. Lilienthal
Chairman, Atomic Energy Commission

7 Incls
As listed

My mother prepared our traditional Christmas Eve dinner of lutfisk, herring, saffron bread, rice pudding, etc. We exchanged and opened Christmas presents after dinner. Peter seemed to be as intrigued by the wrappings as by the gifts.

Wednesday, December 25, 1946 (Christmas)

In South Gate. Vance Cooper and I went to Rio Hondo for 18 holes of golf (VRC-113, GTS-102). We had a long discussion on the course about General Electric--Schenectady and Hanford.

Later my parents, Helen, Peter, and I had turkey and all its trimmings for Christmas dinner.

Saturday, December 26, 1946

In South Gate. Helen Bateson, our travel agent, telephoned and talked with Helen. Since neither she, Mary, nor I have heard anything about the reservations for my trip to Washington, she said she plans to wire Miss Reames. Mrs. Bateson reported that she has made reservations for my return to Berkeley. Later a telegram arrived from Marian Reames, stating that my train reservations to Washington have been sent air mail, special delivery, registered mail to South Gate.

[In Berkeley, permission arrived from Bob Gould to have my Harrison Howe Lecture reprinted (in Spanish) in a South American journal. Mary promptly mailed a copy in my name to Dr. Venancio Deulofeu, saying that since I am not sure that this is what he had in mind, he may publish it or not as he sees fit.]

Sunday, December 27, 1946

In South Gate. Today's golf game was played with my cousin Elmer Johnson, again at Rio Hondo Golf Club (EJ-108, GTS-100).

My train ticket for Berkeley to Washington and my return ticket to Berkeley, along with Pullman reservations for Berkeley to Washington and the return to Chicago, arrived from Marian Reames. Miss Reames included a Government Transportation Request for the return Pullman reservations from Chicago since she has not been able to secure these yet, in the event that I am more successful in securing this space. I also received a ticket and Pullman reservation on the San Francisco "Lark" (SP train #75) for December 29 for my return to Berkeley from Los Angeles.

Saturday, December 28, 1946

In South Gate. I again played golf with Clayton Sheldon at Rio Hondo. For nine holes the scores were Clayton-50, Glenn-45.

Helen and I left Peter with his grandparents and went to a party at the home of Vance and Mary Cooper (301 S. Lomita St., Burbank). Fred and Edrey Albaugh were also there, and we spent the evening talking about our days in Chicago.

Sunday, December 29, 1946

In South Gate. I got together with Bob and Alice Freeman (Alice is Herman Robinson's sister) for 18 holes of golf at Rio Hondo Golf Club. Phil Turner played along with us (AF-117, RF-103, GTS-102, PT-110).

Later I boarded the night train, "Lark," (Pullman) for my return to Berkeley. Helen and Peter will remain in Los Angeles in her mother's apartment (1637 S. Van Ness Ave.) until just before I return from Washington, D. C.

[Our secretary, Mary Millard, married Fred Bender today.]

Monday, December 30, 1946

I checked the status of the research of our group and then looked over my mail. The C & E News of December 25 gave the results of the recent election of the American Chemical Society. I was elected to a three-year term as Councillor-at-Large. In addition, the informal poll of the members named me as "Chemist of the Year." Normally, the cover of this issue would have had a picture of "Chemist of the Year" on it but since my picture was on an earlier issue this month, a picture of Santa Claus appeared instead.

Eugenie T. Procopio, Issuing Officer, sent me an official letter describing travel authorization for my duties as a member of the General Advisory Committee of the Atomic Energy Commission.

I read and promptly declined an invitation from W. J. Horton to speak to the Northwestern Utah Section of the American Chemical Society. I explained to Horton that too many commitments prevent me from accepting but that I would like to visit his group some time in the future.

Neil Gordon, in reply to my letter of December 10, wrote that he will arrange my lecture so that I can leave at 8:45 p.m. on March 24. He said that we can settle the matter of publication of my talk when I come but that he would appreciate a copy of the lecture for mimeographing purposes by March 24. Today I wrote him that I will proceed on the assumption that I will be able to leave for Schenectady on the 8:45 p.m. train. I agreed to send him my manuscript by the March 24 deadline.

I read a letter from Waldo E. Cohn who, with Ed Tompkins, is writing up the fission product separations work that they did with organic acid complexing agents on ion exchange columns. Cohn asked for references for other project work other than the work in Boyd's group.

Another letter was from Truman Kohman, who in addition to comments about various PPR papers, said, in response to my query, that there is no purified radium in Chicago and that all that they had was sent to Los Alamos. Kohman mentioned that he probably will not be able to visit Berkeley before next summer or fall.

Robert Littell (Reader's Digest) returned the reprints I lent him, saying "...they are beyond the knowledge of the great majority--which is a pity, as I feel real study of this whole subject is necessary on the

part of the public for its survival in the atomic age and for its political education if such survival is to be insured."

Gordon Leader sent me a copy of the present list of Volume 14B Collected Papers and described the status of the unfinished papers. He then discussed the numbering system, which he believes is logical but could be changed. Volume 14A, Leader wrote, is essentially complete except for three chapters. Leader noted that he will be at the University of Kentucky after December 29 and will be willing to help by correspondence.

I read a letter from Lyle Dawson, who thanked me for my letter about Sigfred Peterson. Dawson said that Gordon Leader will be in Kentucky on January 1 and that should take care of their needs except for two or three people with little or no experience. Lyle went on to congratulate me on my recent honors and asked that I give his regards to members of the group.

I noted a December 18 letter from J. M. Cork. Cork said that he had lithographed a text for use in his class in radioactivity and nuclear physics. Recently it was called to his attention that three items in the book refer to information that has not been declassified and one of these is on the U^{236} and Pu^{240} isotopes in which he gave reference to me. Cork apologized and said, "It would never have occurred to me that there could possibly be a desire on the part of any governing board to suppress the fundamental natural constants of the elements..."

Joe Kennedy wrote, in another letter I read today, that he and Art Wahl can find nothing objectionable in the Nature draft ("The Transuranium Elements" by Seaborg and Segrè). Joe added that Wahl's first comment was the same as his last summer--why such an article at all? Joe also asked to be placed on the reprint list for Berkeley publications in the nuclear field.

Finally, I received and read a note from Welden Reynolds (Encyclopaedia Britannica), saying that he has been unable to learn the status of Chadwick's article on natural radioactivity and asking if he could receive my article on artificial radioactivity by February. He also requested tentative deadlines for the other articles I have agreed to write for the Encyclopaedia.

Tuesday, December 31, 1946

In attendance at this morning's group meeting were Beaufait, Blaedel, Bockhop, Cunningham, Eyring, Ghiorso, Hicks, Howland, Huffman, La Chapelle, Lilly, Magnusson, Newton, Perlman, Reynolds, Robinson, Seaborg, Simkin, S. Thompson, R. Thompson, Jeanne Wheelock (with Robinson), and Westrum. R. Thompson described the multitude of activities from his bombardment of copper with 200 Mev deuterons. In the future he plans to investigate only one fraction per bombardment so that milking experiments can be performed. Perlman emphasized keeping detailed and accurate logs of bombardment data.

Westrum described his method of making melting point measurements on small quantities (0.1-1 mg). After testing with pure magnesium and pure

zinc, he measured the melting point of neptunium metal as 639-641°C. Howland described the mercury and gold fractions from his bombardment of lead with 200 Mev deuterons. Perlman then led a discussion on the method of writing nuclear reactions.

Finally Cunningham announced that he plans to bombard Am^{241} with 32 Mev helium ions in an attempt to find element 97.

* * * * *

Yesterday I received a letter from Karl K. Darrow, who asked for the name of a person to invite to speak at an April meeting of the American Philosophical Society on the subject of the present and potential uses of radioactive oxygen. I spoke with several people about this--McMillan, Hamilton, Perlman, and others--and today I wrote Darrow that we doubt such a paper is worthwhile in view of the short (2 minute) half-life of O^{15}. I suggested that a more interesting paper could be given on the uses of separated stable isotopes of oxygen.

The following telegram arrived for me today from Bernard M. Baruch:

> I AM GIVING A DINNER IN HONOR OF THE OUTGOING AND THE INCOMING MEMBERS OF THE UNITED NATIONAL ATOMIC ENERGY COMMISSION. IT WILL GIVE ME GREAT PLEASURE TO HAVE YOU PRESENT AT THE LOUIS SHERRY RESTAURANT 300 PARK AVENUE AT SEVEN THIRTY PM FRIDAY JANUARY THIRD INFORMAL OR DINNER DRESS

I spent New Year's Eve aboard the "City of San Francisco" en route to Chicago.

Wednesday, January 1, 1947

Today I travelled to Chicago aboard the "City of San Francisco." My usual routine for such train journeys is to catch up on some of my reading or to work on my writing projects. Today, however, I spent some time in the club car listening to the Rose Bowl game between UCLA and the University of Illinois. UCLA was badly beaten, 45 to 14.

Thursday, January 2, 1947

In Chicago. After arriving in Chicago, I went out to Argonne Laboratory at the University of Chicago to consult with some of the men there. Later I caught the "Capitol Limited" bound for Washington.

Friday, January 3, 1947

In Washington. I arrived in Washington at 9 a.m. in ample time for the 10 a.m. GAC meeting in Room 5136, New War Department Building. Present at this meeting were General Advisory Committee members Dr. James B. Conant, Hartley Rowe, Professor Enrico Fermi, Professor I. I. Rabi, Hood Worthington, Professor Cyril S. Smith, and I. Professor J. Robert Oppenheimer, delayed by travel and weather conditions, arrived at 10:30 a.m. Saturday while Dr. Lee A. DuBridge arrived after adjournment. Members of the Atomic Energy Commission present were David E. Lilienthal, Sumner T. Pike, Lewis L. Strauss, and William W. Waymack. Dr. Robert F. Bacher, also delayed by travel and weather conditions, arrived at 4 p.m., Friday. Carroll L. Wilson, General Manager of the Commission, was also present. Professor Oppenheimer was chosen as Chairman (with Hartley Rowe as temporary chairman in Oppenheimer's absence). Lilienthal presented each member of the General Advisory Committee with an official Presidential appointment and then passed out a sheet describing some of the tasks facing the Commission.

THE WHITE HOUSE
WASHINGTON

December 12, 1946

My dear Professor Seaborg:

I am pleased that you find it possible to arrange your schedule of activities so as to accept membership on the General Advisory Committee to the Atomic Energy Commission. This is an opportunity for public service of the highest character and I am particularly appreciative of the spirit of cooperation which prompts you to accept an appointment which gives your government the benefit of your knowledge and experience.

Very sincerely yours,

Harry S. Truman

Professor Glenn T. Seaborg,
University of California,
Berkeley,
California.

(COPY) Jan. 3 1947

TO THE MEMBERS OF THE GENERAL ADVISORY COMMITTEE:

The members of the Commission have felt that it would be inappropriate to attempt to provide a detailed agenda for the first meeting of the General Advisory Committee. The Commission will look continuously to the General Advisory Committee for advice and assistance "on scientific and technical matters relating to materials, production and research and development". The methods by which this advice can best be given are in themselves topics for discussion which it is hoped can be thoroughly explored at this first meeting of the Committee.

The Commission has, however, considered some problems on which the advice of the General Advisory Committee is requested at this time. As a basis for discussion at the meeting, the Commission has concluded that the following general problems should be brought to the attention of the General Advisory Committee.

(1) One of the pressing decisions of the Commission is the selection of a Director of the Division of Research. The Commission's concept of the functions of this division will be outlined to the meeting by its General Manager, Carroll L. Wilson. It is hoped that specific suggestions as to candidates for this position can be made by the General Advisory Committee before the adjournment of its first meeting.

(2) An immediate task facing the Commission is the preparation of a statement of the existing research and development program resulting from the former Manhattan District's research contracts and a review and evaluation of that program. In order to initiate work on this task, the Commission will need the assistance of the principal universities and industrial concerns which are research contractors. It is hoped that the General Advisory Committee can recommend to the Commission the names of persons from some of the contractors' laboratories who might be brought to Washington for this purpose.

(3) A decision has been made by the Commission to establish a laboratory at Brookhaven and for its construction and operation by Associated Universities, Inc., a non-profit corporation formed by a group of eastern universities. By transfer, the Commission has acquired a contract providing for the construction of a research laboratory at Schenectady, New York, by the General Electric Company. The Commission would like the General Advisory Committee to consider the need for the establishment of these two laboratories in close proximity, and assuming the need for both laboratories, to consider a reasonable division of research and development programs between them. In particular, the Commission desires an expression of opinion from the General Advisory Committee as to whether in view of the present state of scientific information and technical developments, the research program at

(COPY)

the General Electric Laboratory should be one of fundamental investigation or should be more specifically related to industrial and commercial applications of atomic energy.

(4) It is the Commission's hope that affirmative and stimulating relationships can be developed and maintained between the Commission and its research contractors. The General Advisory Committee's suggestions for the establishment of such relationships would be helpful to the Commission. It is suggested that it might be advisable to appoint a special committee composed of business and scientific representatives of university and other laboratories or foundations to study and report on this problem.

(5) At the time of the appointment of the General Advisory Committee, the Commission informed the President that it would probably appoint other advisory committees in the fields of medicine and biology, geology and mining, and the social sciences. The Commission desires the advice of the General Advisory Committee on the need for and functions of such advisory groups and specific suggestions for appointments to them.

(6) The Commission desires the advice of the General Advisory Committee on the best approach to a study of the extent of contamination and possible corrective measures at the Hanford Engineer Works.

(7) An explanation of certain operating changes at Oak Ridge, adopted by the Manhattan District with the approval of the Commission in December, will be made to the General Advisory Committee. The Commission in approving the changes relied on a report of a committee of experts to the Manhattan District stating that with suitable safeguards the new operations could be conducted with reasonable safety. In view of the importance of the problem, however, the Commission desires suggestions from the General Advisory Committee for a review of the safety report.

David E. Lilienthal
Chairman

Lilienthal then discussed the functions and role of the General Advisory Committee and then described the Commission's organization and general nature of its work to date. Carroll Wilson explained the role of the Division of Research as a staff rather than a line operating division. In the ensuing discussion it was agreed that the Committee should advise the Commission on major policy and program determinations and on the selection of technical consultants. Among the subjects suggested by individual members of the GAC for future consideration and advice by the Committee were the desirability of concentrating on the immediate construction of a reactor for the production of electric power; a re-examination of the raw materials situation with emphasis on new techniques for the discovery of sources of materials and on the more efficient processing of such materials; the proper relationship of the development and production of atomic weapons to other research and production programs; the recovery of certain potentially valuable elements now lost in the production of fissionable material (which I suggested) and more effort on studies relating to the use of thorium as fissionable material; the proper balance between freedom in research laboratories to explore the boundaries of scientific knowledge and positive direction by the Commission to research work which has reached a concrete stage; the conservation of uranium through re-examination of current means of production of fissionable material at Hanford and Oak Ridge; and the desirability of continuing the wartime emphasis on the engineering aspects of the development of new materials for use in reactors and other processes.

After lunch (12:30 to 1:30 p.m.), the Committee considered candidates for the position of Director of Research. There was a discussion about the General Electric laboratory at Schenectady (set in an industrial framework) and Brookhaven (academic personnel). Advisory committees were also discussed. I then talked about administrative problems: (1) delay in the publication of the Manhattan Project's technical series; (2) difficulties involved with the employment of students as part-time employees; (3) the unfortunate necessity for all employees to be cleared by the FBI; (4) problems with publications because of policies of the Patent Office of the Manhattan District; and (5) problems with admittance passes. The session adjourned at 5:30 p.m.

I am staying at the Hotel Statler (16th and K Streets) during this meeting.

Saturday, January 4, 1947

In Washington. The GAC meeting reconvened at 9:30 a.m. The following resolution (Conant moved and Fermi seconded) was adopted: the Chairman of the GAC (Oppenheimer), with the help of subcommittees and staff of the Commission, will report on existing status of and future plans for (a) research and development, (b) materials, and (c) production. There was a general discussion on the conduct and mechanics of the activities of the Committee, meeting dates, etc. The following names were suggested for the position of Director of Research: Philip Morrison, Louis A. Turner, John A. Wheeler, Norman Hilberry, J. R. Zacharias, L. R. Hafstad, J. B. Fisk, M. Z. Fineman, Charles L. Critchfield, Norman F. Ramsey, Frederick Seitz, Jr., and William A. Fowler. A subcommittee of Fermi, Rabi, Smith, Seaborg, DuBridge, and the

Chairman ex-officio, was appointed to recommend to the Commission names for the position.

The following advisory panel was recommended to consult with the Commission on the selection of a Director of Engineering: Warren K. Lewis, Earl P. Stevenson, P. C. Keith, Thomas H. Chilton, and Hood Worthington. Then a subcommittee consisting of Smith, Seaborg, and Worthington was appointed to suggest to the GAC names for members of a subcommittee on Geology and Mining. The meeting adjourned at 12 noon.

I called Helen in Los Angeles to check on how things are with her and Pete and to tell her a bit about the meetings I have been attending. Naturally, I complained about UCLA's ignominious loss to the University of Illinois in the Rose Bowl game.

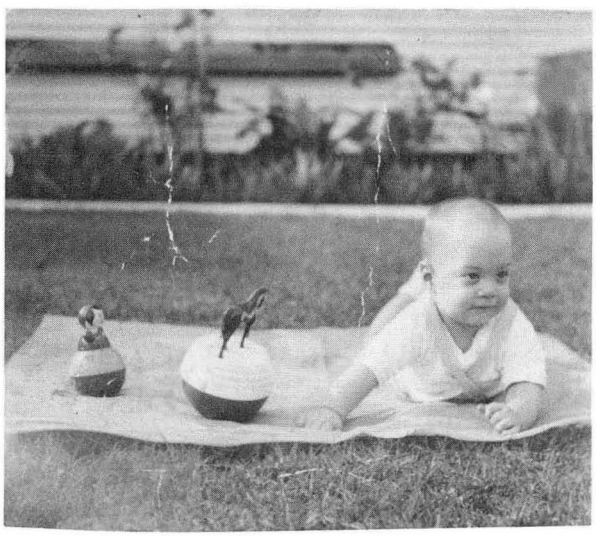
Sunday, January 5, 1947

I arrived in Chicago on the "Capitol Limited." I called Joe Katz and made arrangements to meet him at the New Chem Building on the University of Chicago campus. We then spent some time working on the PPR papers.

Later in the day I caught the "City of San Francisco" bound for Berkeley.

[In Berkeley, Burris Cunningham is scheduled to go to Clinton Laboratories for a visit while in Los Angeles Helen took pictures of Peter and his maternal grandmother.]

Peter and Helen's mother, Mrs. Iva Griggs, 1637 S. Van Ness Ave., Los Angeles, January 5, 1947



Peter Seaborg
January 5, 1947

Monday, January 6, 1947

Enroute to Berkeley. Part of my trip was spent working on the draft of my article, "Artificial Radioactivity," for the Encyclopaedia Britannica. Helen and Peter are scheduled to return home from Los Angeles by train today. [In Washington, Oppenheimer's brief report (attached) on the 1st Meeting of the GAC was sent to Carroll Wilson.]

Tuesday, January 7, 1947

Helen and Peter met me when I arrived in Berkeley (Berkeley Southern Pacific Station) this morning. I was especially pleased to see that Pete recognized me. I then went to the laboratory and spent some time talking with Perlman and looking over my mail.

Amos Newton's report to the Eastman Kodak Company of the Berkeley chemistry group's research program for the period November 1 to December 1, 1946, has been issued. Newton covered the bombardments on the 184-inch cyclotron, the neutron beam distribution from the 184-inch cyclotron, excitation curves for alpha particle and deuteron reactions, particle plate studies, and organic compounds of C^{14}.

I read a letter from Ray Stoughton, in which he said that he and Kurt Kraus are disappointed that I probably will not be able to come to Clinton Laboratories to give the lectures. He said they would like me to come any time to be scheduled as an extra speaker. Stoughton also mentioned that they have a committee (George Boyd, Kay Way, Harold C. Schweinler, Paul W. Levy, William H Sullivan, and Stoughton) collecting new nuclear data and issuing it as monthly memos. He asked about including our results mentioned in the minutes of our meetings.

Wednesday, January 8, 1947

I received and read the revisions to my Princeton talk that Charles Coryell made. Charles congratulated me on my appointment to the GAC.

Then I talked with some of the fellows in the labs about the research.

Thursday, January 9, 1947

The Chemistry 123 lecture was given in Room 219, Gilman Hall at 10:10 a.m. Before going up to the hill, I talked with Geoffrey Wilkinson about his research, which seems to be progressing well.

I read a memorandum from Perlman to E. O. Lawrence about the chemistry program. Iz sent a copy of the outline I prepared in April 1946 while I was still in Chicago and said that all of the items except No. 13 (Isotope Separation by Solvent Extraction) concern work that is either active or anticipated in the foreseeable future. Perlman said the problem in No. 13 is worth pursuing but we do not anticipate any work on this soon. Perlman mentioned that the work on No. 10 (Identification of Reactions and Products Obtained as the Result of Use of New Acceleration Machines) has assumed a more important part of our program than anticipated when the program was drawn up. The topics listed are: 1. Nuclear Properties of Transuranium Elements; 2. Chemistry of Americium;

January 6, 1947

MEMORANDUM FOR CARROLL L. WILSON

At its first meeting the General Advisory Committee to the Atomic Energy Commission took the following decisions:

1. That the comprehensive report on the state of the Atomic Energy projects be prepared for the Committee on the basis of information provided by the Commission;

2. That the Advisory Committee obtain the services of a qualified physicist as its secretary. A panel of the Committee recommended Philip Morse, John Wheeler and John Manley in that order as suitable candidates for secretary.

Since the initial and essential work of preparing the report must necessarily fall with the Commission itself and since any of the proposed secretaries would be satisfactory to the Committee, and to its chairman, I should like to leave with you both the matter of preparing material for the report and of obtaining the services of a satisfactory secretary who might well assist you in the collection of information as one of his first duties.

Dr. J. Robert Oppenheimer

3. Chemistry of Curium; 4. Search for Transcurium Elements; 5. Chemistry of Plutonium; 6. Chemistry of Neptunium; 7. Chemistry of Protactinium; 8. Spectroscopic Investigations; 9. Nuclear Investigation in Region of Atomic Number 80-85; 10. Identification of Reactions and Products Obtained as the Result of Use of New Acceleration Machines; 11. Search for Transuranium Elements in Nature; 12. Basic Chemistry of the Rare Earth Elements; 13. Isotope Separation by Solvent Extraction; 14. Special Separations.

At 4:15 p.m. I went to the meeting of the Seaborg and Connick groups in Gilman Hall. I also attended the Research Conference meeting in Room 102, Gilman Hall at 7:30 p.m.

Friday, January 10, 1947

After taking care of some administrative work, I looked at my accumulated mail.

I read a copy of a memorandum from Kenneth Priestley to Captain W. J. Lee. The document relates primarily to pass mechanics for employees' admission to Building 4.

Herbert A. Young at the Davis campus has a program in progress to study the properties of the halides and volatile compounds of chromium, molybdenum, and tungsten. In order that the Atomic Energy Commission support and receive reports on this work, Professor Latimer has made arrangements with me to coordinate Young's program with ours. Today I read two memos from Donald Cooksey, describing the arrangements, to Russell Ball in the Area Engineer's Office and to Herb Young at UC, Davis.

Dean Latimer also made arrangements for me to talk with two graduate students who want to do their graduate research with us in nuclear chemistry: Harry G. Hicks and Kenneth Street, Jr. Both men received their B.S. degrees from Berkeley in 1943 before entering the armed services. After discussing various possibilities for research, I explained about the necessity for a clearance before they can work on the hill, arranged for them to fill out PSQ's with George Everson, and then introduced them to Geoffrey Wilkinson, with whom they can begin their research without a clearance. Hicks has been doing well in Chem 123 this semester, and Street will take the course this coming spring semester. Wilkinson is presently investigating the products from the deuteron bombardment of platinum and iridium at the 60-inch cyclotron.

Saturday, January 11, 1947

I read and talked with Iz--since we share a cramped office, we can easily consult with each other on administrative and research matters. I then made my usual tour of the laboratories.

In the afternoon I played golf with Bill Dauben and Professor G. Ernest Gibson at Mira Vista (WD-115, GEG-109, GTS-112).

Sunday, January 12, 1947

I played with Peter and then worked for a number of hours on

"Artificial Radioactivity" for the Encyclopaedia Britannica.

In the afternoon Dorr and Louise Etzler visited Helen, Peter, Jeanette, and me.

Monday, January 13, 1947

I checked on the research before looking at the mail that arrived this morning.

Recently I replied to a letter from K. K. Darrow, who asked for a speaker to talk on the uses of radioactive oxygen isotopes at a meeting of the American Philosophical Society. In my reply I suggested a talk on the use of separated oxygen isotopes might be more significant (because of the short half-life of O^{15}) and asked Darrow whether the speaker's expenses would be paid. In a response that I read today, Darrow agreed with my suggestion for that topic and said he would ask L. P. Eisenhart, by carbon of his letter, about the travelling expenses.

V. Deulofeu (Argentina) thanked me for the copy of my Harrison Howe Lecture, which I sent him with permission to translate it for his journal, Ciencia e Investigacion. He explained that it is their policy not to publish papers that have already appeared in other journals; however, he has made a long review of it that will be published under the heading, "Recent Research."

I received another copy of Fortnight from O. D. Keep, Editor and Publisher.

Dr. Earl R. Miller (our health safety officer) sent a memo to Ernest Lawrence, with a carbon to me, describing changes needed in our inadequate hood system in Building 4.

Tuesday, January 14, 1947

Present at this morning's meeting were Asprey, Beaufait, Blaedel, Covey, Eyring, Garden, Ghiorso, Goeckermann, Huffman, James, La Chapelle, Lilly, Magnusson, Morgan, Newton, O'Connor, Perlman, Reynolds, Robinson, Seaborg, Searls, S. Thompson, Wheelock, and Werner. Garden announced that the health group has submitted a set of rules about labeling and storage of active materials to Lawrence. Miss Searls, a new member of Nelson Garden's radiation protection group, is concerned about the amounts of active materials left exposed in the laboratories. This, it was decided, is because the building is not finished.

Reynolds discussed his method of sintering uranium oxide-molybdenum layers, under consideration for use as fuel elements in Lawrence's power pile. I told the group that the present status of a pile at Berkeley is unknown, and I doubt that one will be built here.

Blaedel talked about Westrum's unsuccessful attempt, during his recent visit, to measure the melting point of americium. He also told about Westrum's thoughts on producing protactinium metal.

Morgan described two bombardments of Pu^{239} with 20 Mev deuterons in

order to find short-lived and characterize activities. Tentatively, the assignments are Am^{240} for the 50-hour activity and Am^{239} for the 12-hour activity. He also looked for neptunium activities in these bombardments.

Goeckermann then talked about his bombardment of antimony with 200 Mev deuterons; his interpretation has just begun. Finally, Perlman reported on a lead bombardment with 200 Mev deuterons; a 2-minute alpha activity was found in original target material and a 9-minute, a 27-minute, and very little of a 90-minute alpha activity in the bismuth fraction. On bombardment with 100 Mev deuterons, there was none of the 2-minute activity and only a small amount of the 9-minute activity.

* * * * *

At 10:10 a.m. I gave the Chem 123 lecture.

Back on the hill I answered a letter I received yesterday from Lewis L. Strauss of the AEC, who said that he wanted to ask me the significance of my recommendation that a serious attempt be made to recover presently wasted protactinium, ionium, americium, and neptunium from reprocessing wastes from plutonium production reactors. I said I shall talk with him after the next [GAC] meeting because I believe it is important to recover these and there is already a great deal of experimental background in recovery methods existing within the atomic energy laboratories.

In a note to Marian Reames in David Lilienthal's office, I said that I have asked, through Mr. Trowbridge, that their office help me obtain reservations for the trip to and from Washington next month. I explained that I have learned that a colleague [Albert Ghiorso] will be returning from Chicago to Berkeley about the time I will be returning; I asked that she try to get a bedroom for both of us on the "City of San Francisco" on February 4 so that we can travel together.

I mailed Professor Cyril S. Smith a list of 36 members of the National Academy of Sciences who are in the geology branch. This is in connection with our assignment from the GAC to suggest members for a subcommittee on Geology and Mining. I also explained that I have not yet talked with Professor Adolf Pabst here about the men.

I wrote to J. B. Work that the meeting of the General Electric Consultants Committee at Hanford has been postponed until February 26 and 27, and therefore I cannot give the talk I promised on January 20. I suggested the night of February 26 if it does not conflict with the as yet unscheduled agenda for the meeting.

In the late afternoon I went to the meeting of the Connick-Seaborg groups, and in the evening I attended the Research Conference.

Wednesday, January 15, 1947

After taking care of some administrative matters, I looked over the mail.

Alexander Silverman (University of Pittsburgh) enclosed with a letter dated January 10, a reprint of his article on the early history of

radioactivity in America.

I received a January 10 letter from C. G. Suits (General Electric). Suits has received approval from the District for a consulting contract for me that will make it possible for them to reimburse me; he will forward it when it is prepared.

Earl R. Miller asked me, in a memo I received yesterday, for the names of people working with more than ten micrograms of plutonium, saying they plan to have the urine of these people examined in order to determined whether they have inhaled or gotten some of the material into their bodies. In my reply today I said,

> Before submitting a list of persons who should be considered for this, I wish to call to your attention the fact that the general problem of safeguarding against all dangerous alpha emitters is the more pertinent consideration. For some of these, such as americium and curium, no methods of urinalysis exist at present as far as I know. Of course steps should be taken to remedy this situation, and I hope to discuss this matter with you in the near future.

Thursday, January 16, 1947

I went over my lecture notes for Chem 123 and then gave the lecture at 10:10 a.m. in Room 219, Gilman Hall. Today's lecture included a discussion of the Szilard-Chalmers reaction. I also reminded the class of next Tuesday's midterm examination.

After checking in the Department office, I went to the hill and worked on my talk to Alpha Chi Sigma for tomorrow night.

Friday, January 17, 1947

This morning I talked with some of the fellows and then answered some of my mail.

In response to a letter, dated December 16, from Alice V. Neil (Librarian, Research Laboratory, General Electric Company), I explained that our supply of the reprints of the article, "Artificial Radioactivity," has been long exhausted. However, I mailed her a copy of a rather crude reissue of the article. I also lent her my original reprint in case the Nucleonics Project of the company desires to reproduce the article in quantity.

Another reply went to Beryl Gilman (Technical and Scientific Division, United Office and Professional Workers of America, New York), who asked me on December 20 to serve as a member of the Board of Contributing Editors. I explained that I must decline because of my extremely heavy schedule.

In reply to another note from Neil Gordon (January 8th), I verified that I shall send the manuscript by March 10.

R. C. Gibbs wired, asking me to attend a National Research Council Advisory Conference on Nuclear Science in New York on January 29. I

replied by night letter, "AM INTERESTED IN PLANNED ADVISORY CONFERENCE BUT VERY PROBABLY CANNOT ATTEND THIS TIME. I CAN LEARN RESULTS FROM MCMILLAN IF HE ATTENDS. OTHERWISE SHOULD I ASK GHIORSO OF OUR LABORATORY TO SIT IN FOR ME. HE WILL BE IN NEW YORK TO ATTEND MEETING OF COMMITTEE ON RADIOACTIVITY AT THAT TIME".

W. H Sullivan sent a request for aid with his hexagonal chart--one or two reprints of all pertinent work in the field from me and my colleagues and corrections which we have entered in the presently revised "Table of Nuclei." Sullivan also asked for verification of all the private communications. In my reply I suggested that he asked for a slightly infinite amount of help. I told him that I have made arrangements for the reprints to be sent, that I shall send him a copy of the private communications for him to check, and suggested that he obtain the corrections from a copy of our table that will be sent to Kohman.

I wrote a brief note to Darrell Osborne, who will visit our laboratory at the end of next week, to suggest that he airmail his V-2 form to W. S. Bigelow (Rad Lab Personnel Department) since the administrative offices are not open on Saturday morning when he plans to arrive.

John G. Conway, the spectroscopist from the University of Pittsburgh whom we have located, began work today with our group at a salary of $320 per month.

John Conway

This evening Helen and I attended the dinner meeting of the San Francisco Professional Chapter of Alpha Chi Sigma at the Claremont Hotel. I gave the after-dinner talk on the subject, "Atomic Energy." Later Helen and I danced in the Garden Room along with the Dorr Etzlers and the Richard Faulls.

Saturday, January 18, 1947

In today's mail I received two copies of the sketch of me that appeared on the cover of the December 10th issue of Chemical and Engineering News. Jim Crowe asked that I keep one and autograph the other for their collection.

Iz and I spent some time preparing a midterm for the Chemistry 123 class.

When I got home, Helen reported that Jo Owen had lunch at our home with her.

Sunday, January 19, 1947

As I seem to be doing every Sunday, I worked on my article, "Artificial Radioactivity," for the Encyclopaedia Britannica.

Later, in the afternoon Helen and I went to the Hildebrands' home (500 Coventry Road, Kensington) for tea while Jeanette babysat with Peter.

Monday, January 20, 1947

Much of my morning was spent on my correspondence. I answered a couple of letters (January 2 and 5) from Luther Arnold, a Metallurgical Laboratory colleague, who is now working for the Arthur D. Little Company in Boston. Arnold is dissatisfied with the opportunities there and asked about Tracerlab, Inc. In my reply I said that I believe that there is a great future for the use of tracers in industrial research but I doubt that someone in this business can eventually afford to operate a small pile for the manufacture of tracers (as he suggested). I said that I believe instruments put out by Tracerlab, Inc., compare favorably with instruments put out by other companies. I also suggested that Arnold visit us and talk things over with the gang out here.

In a reply to a letter from Edgar S. Gordon (University of Wisconsin), who wrote to ask me to participate in a symposium on the use of isotopes in biological research in Madison next September, I accepted but stated that, because of commitments, it would be safer not to schedule my paper for September 10th, the first day of the symposium.

I answered a letter from Miss Edith Mitchell (Harris and Ewing Photographers, Washington), saying that I shall plan to come in for a sitting on one of my future visits to Washington.

Charles P. Smyth, in a letter dated January 13, asked that I stop in Princeton on one of my trips East and give a lecture to the Chemistry Department. In today's reply I explained that I shall be near Princeton in March and could speak on the afternoon of March 27 or possibly in the early evening.

I wrote to A. N. Stevens (Lilly Research Laboratories) to explain that I cannot speak at the student meeting of the Indiana Section of the American Chemical Society on April 25 and 26 because of teaching commitments here. However, I added, that I could talk on Friday, April 18, after the Atlantic City meeting which is being held during the week of April 14, but this would probably do them no good.

In reply to a letter from Ray Stoughton that I received last month, I said that I checked with Joe Hamilton who told me that the helium ions produced in the 60-inch cyclotron are probably closer to 40 Mev than to 44 Mev; however, there is some uncertainty. I also asked Ray, who receives copies of the minutes of our research group, not to quote results from the minutes.

Another letter went to Hazel W. Severy (Santa Barbara College) who asked on December 12th for an appraisal of Walter J. Blaedel. I said, "My opinion of him is very high both as a scientist and as a potential

teacher. He is one of the key members of our laboratory here, and we shall be disappointed if it turns out that we must lose him."

I answered a letter, dated November 15, from Leslie G. Cook. I asked him for a copy of their two, now declassified, reports on the 4n + 1 series and said that I shall be glad to help whenever I can with respect to their publications as he requested. In addition, I thanked Les for the invitation to visit next spring although I do not know whether we shall be able to do so.

A note went to Fred Albaugh, asking for the full name and address of Farkas, whom I want to recommend to Karl Darrow as a speaker on stable oxygen isotopes. I noted that I received a telegram from Vance Cooper, and I wonder whether the visit of the General Electric representatives has been postponed indefinitely. I said, "Unfortunately the meeting of our Consultants Committee in Hanford has been postponed until February 26 and 27 so that I am not certain whether I should try to do anything about this matter before that time."

I also replied to a query from Henry B. Hass that was directed to the members of the Committee on Foreign Compendia of the ACS. I said that I think it would be a good idea for our Committee to meet with Professor G. Malcolm Dyson and the Nomenclature Committee of the Organic Division.

Tuesday, January 21, 1947

Our Chemistry 123 class had its third midterm this morning.

Back on the hill I took care of some correspondence: In response to a letter of December 21, requesting an evaluation of Leonard I. Katzin, today I wrote to Dr. M. Burneice Larson (The Medical Bureau, Palmolive Building, Chicago). I described Katzin's background, education, and personality.

On January 7 George Watt wrote about his frustrating attempts to obtain a copy of the Kohman-Jaffey "Counting Manual." Watt also asked if I am in a position to learn whether General Electric would be interested in supporting some of the work he wants to do at the University of Texas since he has, so far, been unable to get support from the Manhattan District. In my reply today I said that I shall try to feel out Guy Suits or Zay Jeffries at the next meeting of the General Electric Nucleonics Consultants Committee about the possibility of supporting his work. I suggested that it might be better to contact Carroll Wilson when the Atomic Energy Commission gets underway. I added that we are sorry that he succeeded in leading Roy Thompson astray (Thompson is returning to Texas) after he obtained such a good start here.

I returned a questionnaire about Melvin Gerstein to H. L. Patterson (Fairchild Engine and Airplane Corporation, Oak Ridge). I wrote that, although I did not know him well, I remember him as a good scientist (in our group at the Metallurgical Laboratory) and would re-employ him if I were in a position where I needed a man of his type.

A letter also went to Professor Paneth to ask if he could give me a reference (names and date only) to the original work on the applications

of radioactive tracers to chemical problems. I suggested the names of Hevesy and Paneth around 1910 or 1912, and I asked for his verification.

To Willard Valentine, Editor of Science, I wrote that I see that he has decided to publish my paper, "Applications of Artificial Radioactive Tracers to Chemistry and Medicine," (Princeton address). I included a corrected copy of the manuscript with revisions to improve its accuracy.

Another letter went to Lyle Jensen, whom I asked if he would be interested in an instructorship here this coming fall, noting that we are still interested in setting up an x-ray crystal structure program.

I also wrote to John Lewellen to ask if he has corrected versions of his brochure of the Lewellen Club Productions' products since one of the men in administration here is interested in the possibility of using the products to educate the visitors.

Another note went to L. H. James, in answer to a letter dated last August, in which I explained that we realize the difficulties in the pronunciation of americium (we use a soft "c") and curium (we pronounce as "k") but the group concurred on these names.

I wrote to Vance Cooper to ask about the postponement of the visit to Los Angeles of the General Electric interviewers. I said, "I am not quite sure whether to advise you to press for direct negotiations or to advise you to wait until after our Consultants' meeting. What do you think? Maybe the latter is still the best bet."

Finally, a telegram arrived today from R. C. Gibbs, asking that Ghiorso attend his Advisory Conference on Nuclear Science and suggesting that I discuss with Ghiorso the subject of the part chemistry can play in the NRC program.

In the late afternoon I went down to the meeting of the Connick-Seaborg groups; I also attended the Research Conference at 7:30 p.m.

Wednesday, January 22, 1947

Iz and I spent the morning grading the Chemistry 123 midterm exams.

In today's mail was a copy of Darrell Osborne's letter to W. S. Bigelow, requesting that the passes for his visit this coming weekend be prepared in advance.

I mailed to Truman Kohman the copy of the "Table of Isotopes" with the latest additions added to it. In addition, I thanked Truman for the chart of isotopes but said that some parts seem to be missing.

In the afternoon Stan and I went to Tilden Park for 18 holes of golf (SGT-95, GTS-99).

Thursday, January 23, 1947

The Chemistry 123 lecture was given at 10:10 a.m. The midterms were

returned and explained. This is the last session of the class.

Back on the hill I read a copy of a letter from Ralph James to Art Jaffey. James explained that the reason he did not write up the experiment in which the half-life of Pu^{238} was determined by observing its growth from Cm^{242} was that the chemical yield of Pu^{238} was not known and the calculated value was thus subject to considerable error. James gave Jaffey the details of the calculation.

In the late afternoon Helen and I went to the Lawrences' home (111 Tamalpais Road) for a cocktail party in honor of Governor Earl Warren.

Friday, January 24, 1947

I received and noted Amos Newton's report covering the work of the Berkeley chemistry group for the period December 1 to December 31, 1946. Newton described the recent bombardments on the 184-inch cyclotron: lead, copper, tin, and antimony. He included a discussion of the type of convention to be employed for writing these nuclear reactions. Newton described our method for measuring the melting points of milligram amounts of metals melting below 1000°C. The melting point of neptunium metal was determined by this method to be 639-641°C. Finally Newton reported on the nuclear particle plate program.

Iz and I both received copies of a "Table of Non-Fission Product Nuclei" from W. H Sullivan. Sullivan included an explanatory letter and asked for corrections and/or additions by the fifth of February.

I read a letter from Carroll Wilson, who stated that they have obtained the reservations I requested for the February GAC meeting trip. Wilson suggested that, in the future if I desire, I can make travel arrangements through their Berkeley office. Wilson also included some details about compensation and payment of expenses for members of the GAC; we will receive the $50 per day compensation for days spent in travel, as well as days spent at the meetings and for days we spend on GAC business at any other time.

Saturday, January 25, 1947

After spending the morning at the laboratory where Iz and I prepared a final examination for the Chem 123 class, Iz, Stan Thompson, Tom Morgan, and I had lunch at Larry Blake's on Telegraph Avenue and then went to Mira Vista Country Club for 18 holes of golf (SGT-93, GTS-111, IP-103, LOM-115). In match play, with two points per hole (low ball and low total), Stan and I beat Iz and Tom, 7 and 4.

Darrell Osborne spent the evening at our Washington Street home with Helen, Jeanette and me.

Sunday, January 26, 1947

I was amused by Peter for a while and then again worked on my Britannica article.

Melvin and Genevieve Calvin visited during the afternoon, and Darrell

Osborne had dinner with us.

Monday, January 27, 1947

I checked on the research before taking care of some of my mail.

I answered a December 18 letter from W. R. Kanne of General Electric in Schenectady, who asked for my criticism of an experiment that they plan to do. In my reply I explained that the delay was caused by the letter arriving at my Chemistry Department office during my absence and then being put away. I said that, in the future, it will be safer to use my Radiation Laboratory address. I then discussed the experiment, which involves the measurement of the neutron capture cross section of Pu^{239}, which I explained was feasible but difficult. I suggested an alternative type of experiment and said that I shall discuss it with him on my next visit to Schenectady.

Earlier this month Nell A. Parkinson (C & E News) wrote and asked me to review Cosmic Radiation, edited by Werner Heisenberg (Second Edition). I agreed, and today I mailed to her my review. I said in part, "This is undoubtedly the best book on cosmic rays which is available at the present time." I also reported, "Although in the main this is a review work, it contains some results which have not been published elsewhere. In particular, there are the theoretical investigations of Moliere on the large showers, the calculations of Flügge on the neutron distribution in the atmosphere and some work of Heisenberg on a simplified cascade theory, which fall into this category."

John Willard sent me a letter that the University of Wisconsin received from a Bert Brock in San Francisco, requesting an analysis of some radioactive substance he has found. Willard suggested that I might want one of our men to talk with Brock.

I also read a letter from W. H Sullivan who enclosed a declassified letter to Professor M. L. Pool (Ohio State) about the radioisotopes of rhodium. Sullivan explained that the letter was sent in order to bring to the attention of persons working in the field of artificial radioactivity, information so that unnecessary duplication of effort can be avoided.

A letter arrived from Carroll Wilson who explained that, in preparation for the February 2 meeting of the GAC, a report from a meeting of Laboratory Directors (January 16, 17, and 18) is being sent to me, part from A. V. Peterson of Oak Ridge and part from N. E. Bradbury of Los Alamos. The information in the report will serve as a basis for considerations of Research and Development activities of the Commission. Today I received the part of the report from A. V. Peterson.

I wired R. C. Gibbs stating, "Ghiorso will attend advisory conference for McMillan and me."

Tuesday, January 28, 1947

Present at today's meeting of our group were: Asprey, Beaufait, Blaedel, Bockhop, Bradley, Conway, Covey, Cunningham, Garden, Hopkins,

Howland, Huffman, James, La Chapelle, Lilly, Miller, Morgan, Newton, O'Brien, O'Connor, Osborne, Perlman, Prescott, Reynolds, Seaborg, Stewart, Templeton, and Wheelock. Cunningham reported on the work of Miller, Hopkins, and Cunningham on the bombardment of arsenic with 200 Mev deuterons. The following elements were separated with added carrier: selenium, arsenic, germanium, gallium, zinc, copper, nickel, cobalt, iron, and manganese with about 70% of the gross Geiger activity in the gallium, about 10% in the selenium, and about 10% in the germanium fraction.

Perlman reported that the bombardment of bismuth with 200 Mev deuterons apparently results in the following reaction: $Bi^{209}(d,\mu^-)At^{211}$. Templeton said that the At^{211} was easy to identify because of its distinctive decay properties. They have ruled out the formation of At^{211} by deuteron bombardment of heavy element impurities in the bismuth, and they think it is unlikely that the At^{211} was formed by some helium ions present in the cyclotron. I suggested other opportunities with materials with which we are working that may allow us to observe d,μ^- and α,μ^- reactions.

Blaedel talked about his attempts to produce protactinium metal. Then, John Conway announced that a Baird spectrograph is being set up in the Chemistry Building (on campus) and should be working in about two weeks. I added that McCullough will be set up in Gilman Hall in a few weeks with an x-ray spectrograph and will be here all of the coming semester. Garden talked about the planned "hot" lab next door to Bldg. 4 with 6-inch lead walls and the new periscope he has designed.

* * * * *

On the hill I replied to a note from K. H. Kingdon (General Electric, Schenectady) to inform him that I plan to attend the next meeting of the Consultants for the General Electric Nucleonics Project at Hanford on February 25 and 26. I then wrote to J. B. Work to explain that the dates of the meeting have been changed to February 25 and 26 and that it would be safer to schedule my talk for the evening of February 25.

Another note went to Mel Silverman (Lewellen Club Productions). I informed him that element 43 now has the name technetium, symbol Tc; element 85 has the name astatine, symbol At; and element 87 has the name francium, symbol Fa. These elements, I said, should be listed on his table with these names and symbols.

This morning Helen had an appointment with Dr. Borson, who confirmed Helen's speculation that she is pregnant.

Wednesday, January 29, 1947

I checked on the research and then took care of some correspondence.

I replied to a January 23 request from George D. Lobingier of Westinghouse Electric Corporation for an appraisal of Leon O. Morgan. I said, "I rate Morgan as a first-rate scientist. He has a quite good understanding of inorganic and nuclear chemistry as well as a good experimental ability in these fields..... He is an industrious research

man and is always willing to do his part or more than his part in any cooperative undertaking in the laboratory."

A similar letter about Paul O'Connor also went to Lobingier. I said, "I rate O'Connor as a very good scientist. He has a quite good understanding of inorganic and nuclear chemistry as well as good experimental ability in these fields. ... He is an unusually industrious person and is very stable in his analysis and his approach to his problems. I feel that he has a very pleasant personality and that he will always get along well with his associates."

I mailed my comments on Sullivan's "Table of Non-Fission Product Nuclei" to him, including a few new references and some information on work of Burton Moyer and co-workers here that contradicts some of the Clinton work. Finally, I mentioned that I shall be in Chicago on February 1 and 4 enroute to and from Washington.

Today's mail brought a thank-you note from Ray Stoughton for the information I sent him recently on the alpha-particle energy he requested. Ray explained that the recent data quoted (by their radioisotope compilation group) as private communication from G. T. Seaborg came from me via Sullivan and not from the minutes of our meetings.

Another letter arrived from Luther Arnold. Arnold said that the Tracerlab proposition has been laid aside because Tracerlab's financial backers have advised caution on venturing into the radioisotope production aspect of the field. Arnold also said he is now considering some chances to go into small enterprises as he has decided that there are monetary as well as personal advantages in working for oneself or in a very small company even though there are greater risks.

I received notification from Frank W. Test that patent application S-3155, "Basic Peroxide Precipitation Method of Separating Plutonium from Contaminants," by Glenn T. Seaborg and Isadore Perlman was filed on November 26, 1946.

The January 4, 1947, issue of Nature contains two articles of particular interest. One, by D. R. Corson, K. R. Mackenzie, and E. Segrè, refers to their discovery of element 85 and suggests the name astatine, symbol At. The other, by C. Perrier and E. Segrè, refers to their discovery of element 43 and suggests the name technetium, symbol Tc.

Thursday, January 30, 1947

The following final examination for Chemistry 123 was given to the class today:

Final Examination
Chemistry 123
January 30, 1947

Masses: n 1.0090
H 1.0081
He 4.0039
Li^7 7.0181

1.

Answer the following in a word, a few words, or a short sentence:

(6 pts.) (a) What two properties of a carrier does one consider in selecting a carrier to specifically co-precipitate tracer amounts of another element?

(2 pts.) (b) What is the principal chemical property causing radio-colloid formation?

(2 pts.) (c) It is well known that if barium sulfate is precipitated from a solution of potassium nitrate, some of the potassium nitrate is carried. What is the probable mechanism for carrying?

(2 pts.) (d) Zirconium activity is much more poorly carried from solution with a variety of carrying agents if hydrofluoric acid is present. Explain.

(6 pts.) (e) What are three desirable nuclear properties for giving a maximum amount of useful activity for tracer work if the activity is prepared by the n, γ reaction? (Consider both target and product nuclei.)

(2 pts.) (f) Why would one expect no nuclear reactions if uranium is bombarded with 8 Mev helium ions?

(2 pts.) (g) Why is it possible to count alpha-particles in the presence of considerable numbers of beta-particles in a suitably designed counter?

(2 pts.) (h) A beta-particle and an X-ray may have the same penetrability through lead. How could they be distinguished by absorption methods?

(4 pts.) (i) What type of accelerator should first produce: (1) 1000 Mev protons? (2) 1000 Mev X-rays?

(2 pts.) (j) What specific result of the fission process accounts for most of the heat generated in a pile?

2.

(15 pts) Calculate (a) the weight and (b) the initial number of curies of 6.8-day beta-emitting U^{237} necessary in order to give by its complete decay one microgram of

2,250,000-year Np^{237}. (c) What is the alpha disintegration rate of this amount of Np^{237}? (Credit: (a) 3, (b) 6, (c) 6.)

3.

(10 pts) For $_3Li^7$ give or calculate the following:

Mass number
Mass
Mass defect
Packing fraction
Binding energy

4.

(15 pts) Calculate the weight of $_{43}Tc^{99}$ which is formed as the result of the irradiation of one kilogram of natural uranium with slow neutrons at an average flux of 10^{15} neutrons/cm^2/sec for a period of 100 days. The following literature values may be used: slow neutron fission cross-section for natural uranium, 3×10^{-24} cm^2; fission yield and half-life of Tc^{99}, 6.2% and 4×10^6 yr., respectively.

5.

The element with atomic number Z has only the one stable isotope, Z^A.

(12 pts) (a) List four ways by which the isotope $(Z+1)^{A+1}$ presumably could be made from Z^A. (Writing the reactions will be sufficient.)

(3 pts) (b) What can you say about the number of neutrons and protons in the isotope Z^A?

6.

(10 pts) (a) Using the following data calculate the average binding energy of neutrons in thorium. Upper energy limit of $UX_1(Th^{234})$ and $UX_2(Pa^{234})$ beta-particles, 0.3 and 2.3 Mev respectively (with no gamma-rays accompanying these beta-particles); energy of U^{238} alpha-particles, 4.7 Mev.

(5 pts) (b) It should be noted that the value obtained in part (a) is an average value for the isotopes Th^{231}, Th^{232}, Th^{233}, and Th^{234}. Explain how the individual thresholds for the γ,n reaction would differ among these four isotopes.

Later I read a letter from Fred Albaugh, who in addition to giving me the information I requested about Bert Farkas, said he was confused about General Electric. Fred enclosed a letter from Giddings, who suggested a visit to Schenectady (as opposed to an interview in Los Angeles), and said that the only way he could get to Schenectady would be to use his 1947 vacation. He concluded by mentioning that he has bought some used golf clubs and two used bags [implying that he can equip me for a golf game with him].

Since I have not yet heard from L. P. Eisenhart about travelling expenses for the speaker for the American Philosophical Society, I wrote to Karl Darrow to suggest the name of Dr. Adalbert Farkas of Philadelphia (whose full name and address I just learned from Fred Albaugh) as an expert in the use of separated stable isotopes in research. I said that he may be the best man irrespective of geography.

I also wrote a memo to Professor Leonard B. Loeb (UC Professor of Physics and chairman of the appointment committee) to say that I favor the appointment of Dr. Theodore Vermeulen to an associate professorship of chemical engineering, saying that Vermeulen is particularly valuable because he is a good physical chemist as well as a good chemical engineer. [Ted Vermeulen, who has a Ph.D. from UCLA and is presently working for the Shell Development Company in Emeryville, would be the key man in leading the program in Chemical Engineering in the Chemistry Department on campus and would be associated with us in the Radiation Laboratory.]

Theodore Vermeulen

A. N. Stevens of the Indiana Section of the ACS wrote, expressing regrets that I cannot speak to their meeting in April and suggesting that, since I will be in Cleveland in March, that I stop in Indianapolis before or after that engagement. In my answer today I said I can stop in Indianapolis on the evening of Thursday, March 20, and speak then to their section.

At 5:30 p.m. I left Berkeley aboard the "City of San Francisco" bound for Washington.

Friday, January 31, 1947

En route to Chicago. Much of my trip was spent reading.

[In Berkeley, Dag Dreher and son Jerry visited Helen and Peter during the afternoon.]

Saturday, February 1, 1947

In Chicago. When I arrived in Chicago, I went out to the New Chemistry Building on the campus of the University of Chicago and talked with Winston Manning, Joe Katz, and others. Later I caught the "Capitol Limited" bound for Washington.

[In Berkeley, Jo Owen visited Helen during the afternoon.]

Sunday, February 2, 1947

In Washington. My train arrived at 9 a.m., and I immediately went to the New War Department Building for the GAC meeting, which began a little before 10 a.m. In addition to the nine GAC members (J. Robert Oppenheimer, James B. Conant, Lee A. DuBridge, Hartley Rowe, Enrico Fermi, Glenn T. Seaborg, Hood Worthington, Cyril S. Smith, I. I. Rabi), three Commissioners (David E. Lilienthal, Robert F. Bacher, William W. Waymack), General Manager Carroll Wilson, James B. Fisk (Director of the Division of Research), Colonel James McCormack (Director of Military Application), John H. Manley (Secretary to the GAC), and Military Liaison Committee members (General Lewis H. Brereton, Rear Admiral William S. Parsons), along with the security officer Captain W. A. Blair and the stenographer Miss E. M. Dashiell were present.

Chairman Oppenheimer explained that he asked Chairman Lilienthal to supply the committee with information about the nuclear weapons stockpile and the production rates. Manley, Blair, and Dashiell were not present while Bacher gave us this information. Oppenheimer then led us into a discussion on reports on research and development available to us, especially the so-called "Panel Report" (proposals for research and development in the field of atomic energy, dated September 28, 1945). He stated this covered the research and development from the Manhattan District at the time the report was written. He summarized the situation reported as that no real exploration of new weapons has taken place. Referring to reactors, Oppenheimer said that Zinn's report (Report on Research and Development, February 1, 1947, Section II-1) states that there has been essentially no new reactor built and no comprehensive reactor program organized. Oppenheimer suggested a recess (10:30 to 11:30 a.m.) so that we could read the reports.

When we reassembled, Oppenheimer asked for comments about the testing of atomic weapons; Admiral Parsons emphasized the need for testing the presently stockpile types since they have not been subject to any testing. Everyone agreed that some testing should be made. DuBridge stressed that thorough and adequate preparation is needed since testing is costly in terms of personnel, time, and money and will involve physical and political dangers. We also discussed some of the details of the weapons and concluded that work toward major increases in energy release was important. We adjourned at 12:30 p.m.

The GAC reconvened at 1:30 p.m. in Executive Session, with Manley and the stenographer present, to discuss reactors. Oppenheimer summarized the need for reactor development as follows: to advance the international aspects of atomic energy through the demonstration of its peaceful utility, to affect public opinion in a similar fashion in this

country, and to provide sufficient fissionable material so that questions of allocation become relatively unimportant. The time scale of this effort, he said, should be to obtain some power in the order of a year or two and then an increased power program in five years. Fermi differed from this point of view, saying that nuclear weapons are more important at this time in view of the international situation and that our country should not risk loss of strength in the field of weapon production, development, and research. He stated that it is more important to make Los Alamos healthy again than to develop nuclear reactors. After some discussion, we generally agreed that weapons are a first priority. This then led to an examination of the Los Alamos situation: lack of strength in theoretical matters, the quality of the present direction of the laboratory, the degree of achievement of the past year, the merits of a different location, the community problems existing there, and the possibility of stimulation of Los Alamos by a directive to develop a reactor or to concentrate on thermonuclear explosives.

Finally, we talked about the function of the General Advisory Committee, particularly whether the members should simply advise the Commission using information supplied by the Commission or whether it should be more of a working group by obtaining information itself; we tended toward the latter view.

After a 15-minute recess, we met with McCormack, Parsons, Bacher, and Fisk to summarize our discussions. The GAC stated that we view the weapon situation as vital and an important and priority job of the Commission. Additional emphasis on thermonuclear weapons over that suggested in the Panel Report should be now given, we said; this may serve to strengthen Los Alamos and to improve its performance with regard to present weapons. We also stated that more work is required on the initiator problem, and we stressed we agreed with the Panel Report that weapons tests should be carried out. We agreed to assist in the recruitment of personnel for the AEC's program. The session was adjourned at 5:30 p.m.

I am staying at the Hotel Statler and, after dinner, I called Helen and learned that she, Jeanette, and Peter visited Muir Woods during the afternoon.

Monday, February 3, 1947

In Washington. The GAC convened at 9:15 a.m. with all members present, plus Manley and Louise Johnson (stenographer). Oppenheimer summarized yesterday's conclusions, saying that it seemed to him that the heart of the problem was reached with surprising speed and that is that the making of atomic weapons is something to which we are now committed, much as we dislike it. He reiterated our comments that the GAC should help as much as we can and there may be a time when a group of us should spend a period of time (as long as six weeks) at Los Alamos in order to help. Oppenheimer stated that we probably do not want to recommend the removal of Los Alamos to another site but we should point out to the Commission (1) at some time a group of us might be willing to go to Los Alamos to live and work for a period of time, (2) that the position of technical director may be easier to fill than that of the theoretical position, (3) that the "super" may not be the best way to revitalize Los

Alamos--a reactor program might provide the proper incentive. We discussed the summary, and Rabi pointed out that he believes the various Commission laboratories are not pulling together.

We then talked about reactors, and Oppenheimer pointed out four things to keep in mind: (1) that we need more plutonium and more plant than we have, (2) that we need a research reactor with high flux to permit entrance into fields now only marginally accessible, (3) that we must look into the long-range aspects of power breeders, probably those using plutonium, and (4) that real atomic power should be produced as quickly as possible. Fermi suggested the following order: (1) an improved version of Hanford, (2) a breeder power unit of the Zinn type should be built as soon as possible, and (3) a high flux reactor should be built as soon as possible. Oppenheimer commented that he would like an even higher priority given to a reactor of the 10,000 kw useful power range. We also talked about the Clinton situation and the present state of the Daniels' high temperature pile, both of which are unsatisfactory. The session adjourned at 12:15 p.m.

Our afternoon meeting opened as an Executive Session at 2 p.m. with Manley and Lucille Ross (stenographer) present. We talked about how to transmit the results of our meetings and agreed that Oppenheimer, as our chairman, will transmit a letter to the Chairman of the AEC, replying to the specific questions raised by Lilienthal in regard to the military applications of atomic energy. Oppenheimer then appointed the following subcommittees: Weapons - Conant (chairman), Rowe, Rabi, Fermi; Reactors--Smith (chairman), Fermi, Worthington; Research--DuBridge (chairman), Rabi, Conant, Seaborg. Oppenheimer will be an ex-officio member of each committee. We also decided to question the policy of holding one Hanford production plant in stand-by condition in order to guarantee the production of adequate amounts of polonium for use as initiators. This Executive Session was adjourned at 4:30 p.m. At 4:45 p.m. Lilienthal, Bacher, Pike, Waymack, Strauss, Fisk, Wilson, and McCormack joined us to hear the results of our executive sessions. I left shortly thereafter in order to take a cab to the train station to catch my train, the "Capitol Limited," at 5:30 p.m., for Chicago.

Oppenheimer's report to Lilienthal, dated February 4, follows:

UNITED STATES
ATOMIC ENERGY COMMISSION
WASHINGTON 25, D. C.

February 4, 1947

~~TOP SECRET~~
~~By Authority of the U. S. Atomic Energy Commission~~
~~Per: [illegible] Date Aug. 6 1947~~
~~Document No. [illegible]~~

Atomic Energy Commission,
Washington, D. C.

Dear Mr. Lilienthal:

During the second meeting of the General Advisory Committee February 2-3, 1947, members of the Committee had an opportunity to become somewhat more familiar with the technical problems facing the Commission. For the most part, we were aware that long and careful study and a far more critical evaluation of the technical facts would be required before we could helpfully advise the Atomic Energy Commission.

To this end we have created three subcommittees. The first, on reactors, has as members Dr. Fermi, Mr. Worthington, and Dr. Smith as Chairman. The second, on weapons, has as members Dr. Fermi, Dr. Rabi, Mr. Rowe, and Dr. Conant as Chairman. The third, on general problems of research not specific to weapon or reactor development, has as members Dr. Conant, Dr. Rabi, Dr. Seaborg, and Dr. DuBridge as Chairman. The Chairman of the General Advisory Committee is ex-officio member of all subcommittees. We would hope that these subcommittees might on occasion be directly useful to the Atomic Energy Commission on specific problems.

In studying the items on the Agenda, with which you are familiar, we were able to reach certain conclusions which, because of their urgency and the unanimity with which we could endorse them, we would like to transmit to you at this time. We are aware that there are problems of equal or greater importance on which we could reach no conclusion, and we are aware that there may be facts both of technical matters and of general policy, knowledge of which might have altered our views. Nevertheless, we have attempted in these recommendations to take into account such known limitations of facilities, personnel, organization, and technical difficulty as were available to us.

CLASSIFICATION CANCELLED

CLASSIFICATION CANCELLED with deletions
~~OR CHANGED TO~~
BY AUTHORITY OF A B Siebert, OC, DOE
BY memo DATE 4/19/90

I. The first matters concern atomic weapons and an answer to the letter of the Chairman of the Atomic Energy Commission in which we were asked to evaluate the recommendations of the so-called Panel Report, and of the actual state of affairs in the light of these recommendations. There are two matters in which we find that our present views differ from those of the Panel Report. One is that in that Report, insufficient emphasis was given to the need for improving the design of initiators for implosion weapons. This deserves particular attention because of its bearing on the effectiveness of the stockpile of weapons and on the operation of the Hanford reactors.

The second point where we would wish to revise the Panel recommendations is this: In those recommendations it was suggested that work on thermo-nuclear explosives be pursued in a leisurely and rather scholarly manner, lest it interfere with the many other pressing developments in atomic energy. The encouraging results of preliminary study in this field, the existence of suggestive new proposals for thermo-nuclear explosives, but above all, our conviction that, properly understood, the pursuit of thermo-nuclear problems would strengthen rather than weaken the quality of work on atomic weapons, would make us now wish to assign a higher urgency to this work.

Strictly speaking, none of the problems outlined in the Panel Report can be regarded as solved. It is probable that the present [DELETED] and composite core designs do constitute a solution, but to establish this, there will have to be direct and significant tests of performance. These tests in turn, if they are to be significant and economical of material and effort, must be based on a more thorough understanding of the behavior of atomic explosives than exists at this time. We therefore wish to couple a recommendation for a test program of new atomic weapons with a prior and stronger recommendation for a fundamental study of the theory of atomic explosives. We would also wish to point out that neither the alpha phase plutonium problem nor the problem of improving explosive components is quite far enough along to warrant adopting present models as satisfactory.

Much of our discussion centered about the question of the methods by which the work at Los Alamos could be improved. We have no single radical suggestion which will achieve this. We are confident that an essential part of the problem lies in strengthening theoretical work at Los Alamos Laboratory, and that

this requires adding to the staff at least one of a very small number of suitably qualified theoretical physicists. We have, however, the following general suggestions, some of which will no doubt prove impractical, but some of which we believe are deserving of further exploration, and all of which are intended to contribute to the strength of our weapons development program.

(1) That the Advisory Committee attempt to establish among colleagues a fundamental understanding of the need for successful weapon development at this time and attempt to overcome the widely prevalent aversion among qualified scientists to work in this field.

(2) That the possibility of supplementing the work of the Director at Los Alamos by bringing there a man of recognized standing in physics who could bring style and inspiration to the work of the Laboratory, should be explored along with, but not independently of the recruiting of theoretical physicists.

(3) That the development of the thermo-nuclear program be undertaken not in competition with, but as a stimulation to improvement in the standards of laboratory work.

(4) That it be accepted as policy to test, after appropriate theoretical study, essentially new models of atomic weapons.

(5) That the Director of Los Alamos be encouraged to take advantage of any help he may obtain from the men responsibly associated with that project during the war.

(6) That to the full extent possible, the scientific resources of the Commission and its personnel be made available to aid the theoretical work at Los Alamos.

(7) That should it appear, as the Committee believes unlikely, that a strong group of men interested and qualified in the development of atomic weapons will be more willing to undertake this development at a site other than Los Alamos, this course should not be discarded as too costly or too dangerous.

(8) That all steps need to be taken, including the payment of high salary, the skillful use of visitors, and the improvement of administrative machinery for the Los Alamos community, to establish that it is the policy of the Commission that the work at Los Alamos has a priority second to none.

II. With regard to reactors, there are only two matters which are sufficiently clear to us. Of all reactor development programs, the one which appears to us most ready for construction, most likely to lead to results in a short time, and, significantly, embodying the soundest principles of power and breeding, is the fast neutron reactor designed by Mr. Zinn's group at the Argonne Laboratory. In the light of the great importance of rapid progress in this field, and of the fact that this reactor appears a sound prototype for the "main line" development, and without prejudice to a later decision on other reactors, we recommend to the Commission that it authorize construction of the Argonne reactor at the earliest possible time.

The above statement has been made without a critical examination of the technical reports available to us; of all our members, only Mr. Fermi has been close enough to this work to suggest that these reports are substantially sound. The Subcommittee on Reactors would be prepared, if the Commission so desired, to make a critical evaluation of the soundness of the Argonne reactor. Such an evaluation must, of course, precede an authorization to construct.

The second point on reactors is this: We are aware of the apparent importance of having the Hanford plant available for operation to produce polonium for initiators. We believe, however, that a re-examination of the reasons for holding a Hanford plant in stand-by condition may very probably reveal that this is not a wise course in the light of our great need of active material. In this connection the Committee is of the opinion that for the long-range power breeder program, plutonium will turn out to be the key substance. For these reasons, we think that it would be sound policy to raise plutonium production in comparison to U-235 production to a ratio at least equal to, and possibly greater than, the optimal ratio for weapons. For these reasons, we recommend the construction of plants of Hanford type, improved in the light of experience, and with greater attention to the conservation of the raw materials. The scale of these new plants should provide the balance of production indicated above.

III. The last matter to be covered in this report is relatively very much simpler. Deuterium and tritium are very useful in small amounts for experimental purposes. We would like to see them made available, as they are not at present, where they are needed. We realize that in the case of tritium,

a decision involving security will have to be made. We believe that withholding this material from qualified laboratories may also constitute an unwise security policy in that it calls attention to our preoccupation with it. In the case of Carbon 14 , we believe that the present high cost is a deterrent to its wider use. We would suggest that the whole problem of charges made for isotopes be re-examined, with the hope that a more effective application of them will result.

The fragmentary character of these suggestions reflects of course our very limited knowledge of the problems. We hope that this will improve with time.

J R Oppenheimer

J. R. Oppenheimer
Chairman

for the General Advisory Committee

Tuesday, February 4, 1947

In Chicago. I went out to New Chem at the University of Chicago. Among the people I saw was Roy Beaton who was visiting. I took the opportunity to talk with him about possible positions for Vance Cooper and Fred Albaugh with General Electric at Hanford. Art Jaffey told me that his long-time decay measurement on Pu^{238}, an experiment that he set up several years ago, now shows a half-life of 90 years. On the other hand, on the basis of its growth from the decay of Np^{238}, the half-life is 77 years. I also called W. H Sullivan to discuss a number of items, including the "Table of Isotopes."

Later Al Ghiorso, who has been working here in Chicago for a few days on the measurement of thermal neutron fission cross sections of cyclotron-produced and other isotopes, and I boarded the train, the "City of San Francisco," bound for Berkeley.

Wednesday, February 5, 1947

Enroute to Berkeley. Al Ghiorso and I went over his data from his thermal neutron fission measurements during our trip.

[In Berkeley, Iz Perlman sent a request for two 60-inch cyclotron bombardments to Russell Ball: 1200 μah helium ion bombardment of an entire backing plate of thorium and a 2000 μah helium ion bombardment of an entire backing plate of uranium.]

Thursday, February 6, 1947

Al and I arrived in Berkeley at 12:30 p.m. (late) and immediately went to the laboratory.

In my correspondence I found a telegram from Joe Kennedy "WOULD APPRECIATE BY AIR MAIL LIST OF DECLASSIFIED TRANSURANIUM AND TRANSTHORIUM AND FOUR N PLUS ONE NUCLEI WITH HALFLIVES RADIATIONS AND FORMATION REACTION KNOW MOST OF THESE BUT UNCERTAIN OF CLASSIFICATION STATUS THANKS." I immediately mailed Joe a reprint and suggested that he read my Harrison Howe Lecture in the current issue of C & E News. I then said that the existence of U^{233} is declassified and a short note by Seaborg, Gofman, and Stoughton will appear within a month or so, but we are not yet permitted to mention half-life or type of radiation. However, the Canadians have obtained permission for oral presentation for similar work on the decay products of U^{233}; we hope to publish our work simultaneously with their work.

I noted that patent application S-1145, "Method of Separating Uranium, Plutonium, and Fission Products by Bromination and Distillation" by Arthur H. Jaffey and Glenn T. Seaborg, was filed on January 8.

I also read a letter from William Meggers, who questioned me about the symbol for francium--I had given him the symbol Fa in a letter I wrote on October 8. Meggers read Professor Paneth's article in the January 4th issue of Nature that contained the statement that Mlle. Perey proposes the symbol Fr.

Lyle Jensen wrote on January 28 that he would be interested in al instructorship at Berkeley with the opportunity for research in the field of x-ray diffraction and crystal structure. Jensen said that the work with Professor Johnston at Ohio State is most interesting but it probably would be possible to leave the post next September.

A long letter arrived from Bob Campbell of Life. Campbell and Fritz Goro, with the backing of Dennis Flanagan, want to do a picture story on the Radiation Laboratory, similar to the one they did last year on the "Plutonium Laboratory"; he posed a number of questions about the projects going on here.

Friday, February 7, 1947

Iz and I have turned in the grades for the fall semester of Chemistry 123: Miss A. M. Anderson, A; L. B. Asprey, A; D. R. Bomberger, A; K. Chivukula, A; M. Chun, B; J. G. Claypool, C; J. W. Cobble, B; C. C.

Delwiche, B; E. L. Dobson, A; A. K. Dunlop, B; R. C. Feber, B; S. P. Gessel, B; P. W. Gilles, A; D. L. Grunes, C; J. H. Guill, C; Miss C. R. Hawkins, C; H. G. Hicks, A; H. H. Hopkins, A; R. House, B; Z. Z. Hugus, A; W. T. Jackson, C; J. L. Jungnickel, B; Mrs. L. S. Kelly, A; R. R. Kitson, F; J. H. Klinger, B; J. R. Lai, C; E. H. Lambert, B; H. C. Lanz, B; Mrs. H. M. L. Maas, B; Miss H. J. Maxwell, C; D. R. Miller, A; T. W. Newton, A; D. A. Orth, A; M. E. Rozen, C; R. K. Sheline, A; D. C. Stewart, A; P. R. Stinchfield, C; B. Suverkrop, C; M. W. Taft, B; L. A. Walker, B; A. D. Webb, B; J. W. Weigl, B; A. R. Weiss, B; W. Weltner, B; R. E. Weston, B; R. W. Wheatcroft, B; B. Williams, B; Miss I. M. Wilsey, C; D. M. Woodbury, C.

I mailed the deleted note, "Nuclear Properties of U^{233}: A New Fissionable Isotope of Uranium" by G. T. Seaborg, J. W. Gofman, and R. W. Stoughton to Professor John T. Tate for publication in The Physical Review:

> The bombardment of thorium with slow neutrons produces Th^{233} (by the reaction $Th^{232}(n,\gamma)Th^{233}$), which emits beta particles and has a half life of 23.5 minutes. The daughter of Th^{233} is the 27.4-day beta-emitting Pa^{233} which in turn decays to U^{233}. We have measured the radioactive and the fission properties of U^{233}. Our measurements in a sample of U^{233} weighing 3.8 micrograms show that this isotope undergoes fission with neutrons. The same result was obtained in a check experiment with another sample of U^{233} weighing 0.8 microgram.

In the covering letter I said, "I believe that the note itself together with the explanatory footnote is probably self-explanatory. It has been submitted for clearance and has been approved for publication in its present form."

I also answered a letter that arrived yesterday from A. N. Stevens of the Indiana Section, who is most pleased that I agreed to talk to them and asked for a biographical sketch, photographs, and the title of my talk for publication in The Accelerator, the publication of the Mid-West Cooperating Sections of the ACS. I sent him the requested materials, suggested the title, "Plutonium and Nuclear Energy," and said that I plan to arrive in Indianapolis on the "Carolina Special" at 5:05 p.m. on March 20.

I read a letter from W. D. Leech (Chairman, Faraday Committee in Pasadena), who invited me to give the 1947 Twentieth Annual Faraday Lecture to a group of young students on March 27. The Lecture, he said, is presently sponsored by the Pasadena Board of Education, the Science faculty, the John Muir Junior College, the Pasadena Junior College, and special members of the faculty of the California Institute of Technology and is patterned after the Christmas Juvenile Lectures carried on by the Royal Society in London. The lecture seemed very worthwhile so I called Leech. I explained that I am unable to speak on March 27, but I shall be able to do so in April. I suggested April 28, 29 or 30.

I wrote to Professor A. J. Dempster at the University of Chicago and explained that we are attempting to include the best information on abundances of the stable isotopes in our new "Table of Isotopes." I

asked that he check report B-6.281.1 by A. E. Cameron and J. R. White of the Clinton Engineer Works, which apparently includes new work on the abundances of the isotopes of some 24 elements. I explained that we want to know if the work is sound. I also mentioned that we hope to take advantage of his offer to have one of our men work in his laboratory.

I made the rounds of the laboratories and brought myself up to date on the progress of the research.

Saturday, February 8, 1947

This morning I replied to a letter dated January 22 from Alden H. Emery (Secretary of the ACS) and said that I shall be glad to serve as a member of the American Chemical Society Advisory Committee to the Chemical Corps.

A. Pabst (Department of Geological Sciences, UC) sent me a list of names of eminent geologists and a list of references about natural radioactivity and geology, along with comments. I requested these of Pabst in connection with my work on the GAC.

I received a note from V. Deulofeu who said that he is sending me a copy of the February issue of Ciencia e Investigacion, which contains his résumé of my Harrison Howe lecture. Deulofeu said that he is sorry he did not publish the complete paper.

On February 8, Miss L. Belle Voegelein (Washington, D. C.), who is editing a work, wrote to ask if the names americium and curium are definitely accepted as names for element 95 and 96. I answered today and said that, although the names are technically regarded as suggestions, I believe that she is safe to use them in editing work.

A note had arrived from Charles P. Smyth, who said he is delighted that I can speak on March 27 in Princeton, and will schedule the talk at 4:30 p.m. so that I can catch an early evening train to Washington.

I mailed Truman Kohman information on the new reactions obtained with the 184-inch cyclotron. I said that, although it is declassifiable, it is still to be regarded as secret.

After lunch Stan Thompson and I went to Mira Vista for a game of golf with Professor G. E. Gibson and his son Charlie (SGT-102, GTS-103, CG-116, GEG-107). Playing for two points per hole (low ball and low total) in match play and using a complicated handicapping system, Ernest Gibson and I beat Stan and Charlie by 12 and 8.

Helen and Jeanette went to Oakland to see the Gondoliers in a Gilbert and Sullivan operetta during the evening. Flammarian (Flam) Barbachano was in the cast.

Sunday, February 9, 1947

Part of my day was spent putting the finishing touches on our paper on the 4n + 1 decay series (daughters of U^{233}).

I also read the article by Merritt L. Kastens in the January 27th issue of C & E News on "Accelerators under Construction at Berkeley." [I had looked over and criticized this article for Kastens while he was preparing it.]

During the evening Helen and I left Peter with Jeanette and went to see "Holiday in Mexico." The movie played at the Oaks Theater (1875 Solano Avenue) and starred Walter Pidgeon, Jose Iturbi, Roddy McDowell, and Jane Powell.

Monday, February 10, 1947

I visited the various labs to talk with the men about their research.

My Harrison Howe lecture, "Plutonium and Other Transuranium Elements," is appearing in today's issue (February 10th) of Chemical and Engineering News. Included in the article is a picture of americium hydroxide; this was the first macroscopic quantity of americium, isolated by Burris Cunningham at the Metallurgical Laboratory [C & E News 25, 358 (1947)].

I mailed Leonard Katzin the following carbon copy of our paper on the 4n + 1 series and said that I shall send it in for declassification when I have their corrections and/or ok:

The 4n + 1 Radioactive Series:
The Decay Products of U^{235}

By F. Hagemann[2], L. I. Katzin[2], M. H. Studier[2], A. Ghiorso[4] and G. T. Seaborg[4]

Metallurgical Laboratory, University of Chicago[1]

(1) Work performed under auspices of Manhattan District.

During 1944-1946 we studied the chain of decay products of U^{235}, the new isotope of uranium which was first separated and examined (1941-42) by Seaborg, Gofman and Stoughton[3].

(3) G. T. Seaborg, J. W. Gofman and R. W. Stoughton, Phys. Rev. 71, in press (1947).

These decay products, which constitute a substantial fraction of the entire missing, 4n + 1, radioactive series are listed, together with their radioactive properties in the following table:

Isotope	Type of Radiation	Half-life	Energy of Radiation (Mev)
$_{90}Th^{231}$	α	7×10^{8} yr.	4.85
$_{88}Ra^{231}$	β^-	14.8 days	~0.2
$_{89}Ac^{231}$	α	10.0 days	5.80
$_{87}Fr^{231}$	α	4.8 min.	6.30
$_{85}At^{217}$	α	0.018 sec.	7.00
$_{83}Bi^{213}$	β^- (96%) α (4%)	47 min.	~1.2 (β^-) 6.0 (α)
$_{84}Po^{213}$	α	very short	8.30
($_{81}Tl^{209}$)	(β^-)	?	?
$_{82}Pb^{209}$	β^-	3.3 hr.	0.7
$_{83}Bi^{209}$	stable		

The radioactivity of the Tl^{209} has not yet actually been observed; its existence in the chain is inferred from the partial alpha-decay of Bi^{215}. The isotope Pb^{209} has been previously reported, as a result of its production by the d,p[6], n,γ[7] and n,p[7] reactions.

(6) R. L. Thornton and J. M. Cork, Phys. Rev. 51, 383 (1937)

(7) W. Maurer and W. Ramm, Zeits. f. Physik 119, 602 (1942)

A number of the preceding members of this 4n + 1 radioactive series have been previously reported as follows:

$$_{94}Pu^{241} \xrightarrow{\beta^-} {}_{95}Am^{241} \xrightarrow{\alpha} {}_{93}Np^{237} \xrightarrow{\alpha} {}_{91}Pa^{233} \xrightarrow{\beta^-} U^{233}$$

The 27.4-day Pa^{233} was first reported by Meitner, Strassmann and Hahn[8], and the doubts as to this isotopic assignment

(8) L. Meitner, F. Strassmann and O. Hahn, Zeits. f. Physik 109, 538 (1938)

which later arose as a result of the discovery of fission were cleared up by the work of v. Grosse, Booth and Dunning[9] and Seaborg, Gofman and Kennedy[10]. The 2.25×10^6-year Np^{237}

(9) A. v. Grosse, E. T. Booth and J. R. Dunning, Phys. Rev. 59, 322 (1941)

(10) G. T. Seaborg, J. W. Gofman and R. W. Stoughton, Phys. Rev. 59, 321 (1941)

was first identified by Wahl and Seaborg[11] while Pu^{241} and

(11) A. C. Wahl and G. T. Seaborg, reported in Chem. Eng. News 23, 2190 (1945)

500-year Am^{241} were first reported by Seaborg, James and Morgan[12].

(12) G. T. Seaborg, R. A. James and L. O. Morgan, reported in Science 104, 379 (1946) and Chem. Eng. News 25 358 (1947)

Also of interest are two previously reported beta-emitting radioactive isotopes, 23-minute Th^{233}(15) from the reaction $Th^{232}(n,\gamma)$ and 7-day U^{237}(14,15) from the reaction $U^{238}(n,2n)$, which may be referred to as "collateral" members of the series.

(13) E. Fermi, E. Amaldi, C. D'Agostino, F. Rasetti and E. Segrè, Proc. Roy. Soc. A146, 483 (1934)

(14) Y. Nishina, T. Yasaki, H. Ezoe, K. Kimura and M. Ikawa, Phys. Rev. 57, 1182 (1940)

(15) E. M. McMillan, Phys. Rev. 58, 178 (1940)

As the name for the 4n + 1 radioactive decay family we suggest "neptunium" series or family; thus the longest-lived member would give its name to the family in a manner similar to the naming of the uranium and thorium decay series.

Another, independent study of the decay products of U^{233} was carried on simultaneously by A. C. English et al. of the Division of Atomic Energy of the National Research Council of Canada, which has resulted in essentially similar findings, and this work is being published at this time in another place.

(a) Present address, Argonne National Laboratory, Chicago.

(b) Present address, Institute of Nuclear Studies, University of Chicago.

(c) Present address, Radiation Laboratory, University of California, Berkeley.

Tuesday, February 11, 1947

Present at this morning's group meeting were Asprey, Ball, Beaufait, Blaedel, Bockhop, Bradley, Conway, Covey, Cunningham, Eyring, Garden, Ghiorso, Goeckermann, Hicks, Hopkins, Howland, Huffman, La Chapelle, Lilly, Lindner, McCullough, Magnusson, Morgan, Newton, Nolan, O'Brien, O'Connor, Perlman, Prescott, Reynolds, Robinson, Seaborg, Templeton, Thompson, Werner, and Wheelock. Perlman announced that telephone calls are taking up too much of the secretaries' time and said each worker will be responsible for answering calls on his extension.

Templeton asked for a remedy for lack of transportation to the 184-inch cyclotron area on Friday, Saturday, and Sunday nights. He then discussed his bombardment of bismuth with various energies of deuterons (30 to 200 Mev), followed by separation of a fraction probably containing polonium, astatine, and mercury by heating the original target and collecting the volatile material on a cold plate, and investigating the alpha particle radiation in the pulse analyzer. At all energies there were comparable yields of At^{211} and its daughter Po^{211}. This constancy of yield now makes uncertain the mode of formation of At^{211} (at first it was thought to be formed by mesotron ejection). He found short-lived alpha-particle activities in this volatile fraction at the highest energies--half-lives of 10 minutes, 22 minutes, and 60 minutes (5.9 Mev alpha particles) and 2 hours (5.2 and 5.9 Mev), 1 day (5.2-5.3 Mev) and at lower energies he found 2 hours (5.2 and 5.9 Mev), 1 day (5.2-5.3 Mev) and greater than 1 day (5.1, 5.3, 5.4 Mev). To test for fission he sought bromine fission products; Br^{82} became undetectable below 50 Mev.

Hopkins talked about the bombardment of arsenic (atomic number 33) with 200 Mev deuterons. He looked for yields in several fractions from $_{21}Sc$ to $_{34}Se$. No activity was found in scandium; the lowest atomic number in which activity was detected was 25 (manganese). Miller reported on the bombardment of copper with 200 Mev deuterons with emphasis on the gallium activities. These could be formed by "mesotron ejection" reactions unless they are due to helium-ion impurity in the deuteron beam. Observation of 70-minute Ga^{68} suggests the presence of helium ions since it could be produced only by the $Cu^{65}(\alpha,n)$ reaction.

Ghiorso reported on the thermal fissionability measurements made recently in Chicago: incorrect decay of Cm^{240} fissionability casts doubt on the 16,000 barn cross section; Cm^{242} was again found to have a cross section of 40 b; the cross section of Pu^{238} (not precise) is 13 b. I reported, on the basis of information from Jaffey during my recent visit to Chicago, that his decay measurements show a half-life of 90 years for Pu^{238}. James estimates a value of 115 years from his yield in milking Pu^{238} from the decay of Cm^{242}.

Stan Thompson described the plans for the search for element 97. He will bombard 1 mg of Am^{241} on Thursday with helium ions in the 60-inch cyclotron and use the predicted quadrivalent state of element 97 to aid in the chemical separation. In his three previous bombardments with helium ions he used 10 micrograms of Am^{241} about a year ago, 2 mg several months ago, and 1 mg more recently. In the last bombardment the Am^{241} was lost in the cyclotron; this time the americium will be covered with a

2 mil platinum foil.

Lindner talked about his inconclusive bombardment of antimony with 200 Mev deuterons, in which he sought evidence of mesotron ejection reactions by looking for radioactive iodine products; Goeckermann reported that his 200 Mev deuteron bombardment of antimony to look for Te^{118} should be repeated. Finally, McCullough said that the efficiency of his x-ray equipment has been demonstrated by determining the lattice constants of NaCl, which agree within 0.1% of the accepted values (the camera is apparently working well).

* * * * *

I stopped in the Department of Chemistry office to talk with my colleagues and Miss Kittredge before going up to the hill.

Today I received a very detailed reply to my letter of January 21 to Professor Paneth about the first users of radioactive tracers. Paneth said I am correct in the names--Hevesy and Paneth--but the date was 1913. Paneth gave me some of the background of the work and is sending me three reprints of the original work.

Alice V. Neil (Librarian at General Electric in Schenectady) returned the file copy of my article, "Artificial Radioactivity," which I sent her last month, saying that the Nucleonics Project does not feel the costs warrant duplication at this time.

I promptly replied to a letter I received from Vance Cooper today. Cooper said that he agreed with my suggestion that he let the situation with General Electric lie dormant until after my trip to Hanford later in the month but he just received a wire from Hanford that two of their personnel men will be in the Los Angeles area the week of February 10 to 15 and, if he is interested, he should receive them. In addition, Vance said he had just received a $40 raise from Union Oil, which complicates the question of leaving the company. In my reply I mentioned that I saw Roy Beaton in Chicago and Beaton suggested to me that Cooper might head up either the Design Group, the Experimental Planning Group, or Operations Group. I emphasized to Vance that one can't consider Beaton's statements as binding commitments. I also mentioned that I spoke to President L. A. DuBridge of Cal Tech about him and Fred as possibilities for the proposed Southern California National Regional Nuclear Laboratory; I will write him and Fred about this with copies to DuBridge.

Wednesday, February 12, 1947

After making the rounds of the labs in Building 4 to check on the research, I went down to my Gilman Hall office and worked over my notes for Chemistry 123. I also saw Geoffrey Wilkinson and discussed his work and the progress of our new graduate students, Hicks and Street.

Helen went to Mrs. Hildebrand's home (500 Coventry Rd., Kensington) for the regular tea for the chemistry wives.

Thursday, February 13, 1947

I checked on the research and then took care of some correspondence.

In a memo to George Everson, I requested that Manfred Lindner, one of our graduate students, be placed on the payroll on a half-time basis (the regular graduate student school year rate).

Another memo went to Donald Cooksey about the paper BC-31 (a paper by Howland, Templeton, and Perlman), whose declassification was refused. I pointed out that the mention of Po^{210} was the only classified item and this sentence could be deleted without changing the sense of the paper.

In a memorandum to Professor Raymond T. Birge (Chairman, Department of Physics, UC), I highly recommended William J. Knox for a teaching assistantship. I wrote, "I think he might be particularly valuable in physics because he will possess that rare combination of good training in both of the fields, physics and chemistry, which, if properly applied, should present an unusual opportunity for good work." [Knox is switching from chemistry to physics for his Ph.D.]

A thank-you note went to Art Jaffey for the copies of his absorption curves; I added that we shall send him the Berkeley curves he requested.

Two copies of a photograph of me went to R. E. Dunbar (North Dakota Agricultural College), who requested them for the use of the Chemistry Club of the school.

In reply to a January 30 letter from W. H. Dunkak of Merrill Lynch, Pierce, Fenner, and Beane, I referred Dunkak, who asked about the commercial applications of atomic energy, to the Department of State's summary, "The International Control of Atomic Energy--Scientific Information Transmitted to the United Nations Atomic Energy Commission."

Today I received a carbon of a memorandum from Joe Hamilton to the Area Engineer, requesting a joint 60-inch cyclotron bombardment (his group and ours) for 2000 μah of deuterons on a U^{235} target. Our group will use our fraction for the study of Np^{235} radiations while his group will use the Np^{235} as a tracer for metabolic studies.

This evening Helen and I saw a preview of a movie on the atomic bomb, "The Beginning or the End."

Friday, February 14, 1947

Part of the day was spent going over material for a radio program that I am going to record tomorrow.

E. M. Crane (D. Van Nostrand Company) telephoned me and discussed the preparation of a section on the actinide elements for Sneed's proposed book, Comprehensive Inorganic Chemistry. In spite of my limited time, I agreed to do so since the material for such a section is very restricted.

Young Peter received a Valentine's Day card from his grandparents (my mother and father) today.

Saturday, February 15, 1947

I spent the morning at the laboratory, mainly reading reports. As usual, Stan Thompson and I had lunch at Larry Blake's (2367 Telegraph).

Later I went to Radio Station KRE (West Berkeley) and recorded a talk, "Scientific Background for Atomic Energy." This will be broadcast over station KFWB in Los Angeles. I explained the difference between nuclear and chemical energy, Einstein's relation $E = mc^2$, and the potential role of U^{235}, plutonium, and U^{233} as nuclear fuels, all derived, directly or indirectly, from uranium.

Sunday, February 16, 1947

I spent the day reading and with Helen, Pete, and Jeanette--it has been a rather miserable week weatherwise, but today it cleared enough so that the four of us went for a short ride in Jeanette's car.

Monday, February 17, 1947

I checked on the research and then looked at the mail--Monday's mail is usually quite heavy.

William H Sullivan wrote that he is scheduling a meeting of the Subcommittee on Nuclear Constants in Chicago on March 22. Sullivan thanked me for my corrections to the table of Non-Fission Product Nuclei and mentioned that Seymour Katcoff was quite critical about his referencing system. He asked for opinions on this subject.

Paul Aebersold sent me, because of my membership on the GAC, a copy of a "Summary Report on the Radioisotope Distribution Program," a draft of "General Recommendations for the Radioisotope Distribution Program," and two short publicity releases about radioisotopes. He wrote that I may find the latter useful for preparing popular lectures and asked to be placed on our mailing lists for articles or lecture notes. Paul, who is working with the radioisotopes distribution group at Oak Ridge, concluded with some personal remarks.

A. N. Stevens acknowledged the publicity material I sent him for my talk to the Indiana Section of the ACS and said that he will meet my train on March 20.

I read a letter from Joe Kennedy thanking me for the references I sent him recently. Joe said the news about the 4n + 1 decay series has leaked to St. Louis but, in view of my comments, he left the information out of his seminar report. Joe also mentioned that he will be in Oak Ridge during the week of February 24 and will try to see Stuart Scott about our applications for compensation for our plutonium patents. Joe then asked my opinion of Reid's long-lived (56-day) iodine in the recent Physical Review; he thinks Reid's isotopic assignment is probably wrong.

A. J. Dempster answered my recent query about the work of A. E. Cameron. He said that he would give great weight to Cameron's work for Cameron is a precise worker. Dempster then reported that he discussed the values in Cameron's report with Mark Inghram and gave me the values they prefer for the abundances of the stable isotopes as follows: Li (Hoff-Lu); Mg (Cameron); Si (Ney or Inghram); K (Nier); Ca (Nier); Cr (probably Cameron); Fe (Cameron); Ni (Cameron); Cu (Cameron); Se (Cameron); Sr (Nier or Cameron); Zr (Cameron); Mo (Williams and Yuster, Cameron when complete); Ag (Cameron); Cd (Cameron or Nier); In (Cameron); Sn (Cameron); Sb (Cameron); Te (Cameron); W (Inghram); Th (Nier or Cameron); Pb (Nier). Dempster also mentioned that they hope to have a series of accurate measurements for the rare earth elements in a few months.

A letter arrived from Ruth Rogers, saying that she has mailed me a collection of material that I requested. Ruth commented that she has had no luck finding anybody who knows of references about the sandwich foil for gamma counting about which I inquired when I was in Chicago recently.

I received a couple of comments from Leonard Katzin about the 4n + 1 series paper that I mailed him last Monday. I then wrote Hoylande Young and explained that the Canadians have obtained declassification of their work on the U^{233} decay products. I asked that she have the summary of our work, which I enclosed, declassified so that we can publish it at a time nearly simultaneous with the publication of the Canadian work. I also sent Katzin a correction for Chapter XXII, PPR, Volume 14A.

My day was interrupted for a while this morning. Helen had an appointment with Dr. McAlear for a check-up for Peter, who received his first diptheria immunization. Shortly after 10:15 a.m., I received a phone call from Dr. McAlear's nurse who reported that Helen was threatening to abort and that she had called Dr. Borson and an ambulance. I immediately went to Dr. McAlear's office (American Trust Building on Shattuck Avenue) and took charge of Peter while Helen went to Alta Bates Hospital. Jeanette met me in the doctor's office and took Peter home. I then went to the hospital and found that, fortunately, Helen seems to be doing all right. When I arrived home after work, Peter seemed more pleased than usual to see me; he obviously is missing his mother. Helen spent the night in the hospital.

Tuesday, February 18, 1947

At this morning's meeting the following people were present: Asprey, Ball, Beaufait, Blaedel, Bockhop, Conway, Covey, Eyring, Fries (guest from Cal Research), Ghiorso, Hicks, Hopkins, Howland, James, La Chapelle, Lilly, Lindner, McCullough, Magnusson, Miller, Moore, Morgan, Newton, O'Connor, Perlman, Prescott, Robinson, Reynolds, Seaborg, Stewart, Templeton, and Wheelock. Hopkins reported on two bombardments of arsenic with 200 Mev deuterons. Of the 36 activities found, approximately two-thirds are positron emitters. Hopkins described the chemistry, devised by Miller and Cunningham, used for the two bombardments.

Magnusson then outlined his plans to study the chemistry of Np(V), and then Morgan reported on his latest experiments on the 12-hour and 50-hour americium isotopes, in this case formed by bombarding Pu^{239} with

16.4 Mev deuterons. Next he plans to use 12 Mev deuterons in order to obtain information on isotopic assignments.

La Chapelle described his plans for studying the dry chemistry of neptunium. He will attempt to prepare $NpCl_5$ and oxides with higher oxidation states than plus four (NpO_2). I suggested he try to prepare compounds such as NpO_2Cl by means of wet chemistry.

Templeton told about his preliminary results of bombarding lead with 400 Mev helium ions; he observed products in the range of atomic number 33 (arsenic) to 56 (barium), which indicate the fission of lead has been observed for the first time. I said a survey of fissionable elements will be more profitable now than a detailed study of any one element. I suggested spot testing for fission in elements such as platinum, gold, and mercury; a review of the bombardment of lead with 200 Mev deuterons where fission has not yet been observed; and investigations with lower energy helium ions on lead to find the threshold for fission.

* * * * *

In today's mail I received from Robert Gould (C & E News) copies of all the photographs of me in their files. I requested these a few months ago.

Later I went to Alta Bates Hospital to see Helen. Dr. Borson reported that she is doing all right but needs to rest.

Wednesday, February 19, 1947

At the lab I worked on some routine matters, including reading some reports, and then looked over the mail.

I acknowledged a letter from L. P. Eisenhart who just informed me that he asked Hugh S. Taylor to contribute a paper on the use of separated stable isotopes of oxygen to an upcoming meeting of the American Philosophical Society, saying that his choice was a good one.

Joe Katz wrote that he is interested in obtaining ditto masters or eight copies (for declassification purposes), of a number of early Berkeley reports. I promptly forwarded Joe's letter to Bob Connick on campus and asked if he could handle it.

Later I worked in my office in Gilman Hall on the lecture notes for Chemistry 123. I also stopped in Geoffrey Wilkinson's lab to check on the research.

Helen came home from the hospital with orders to rest.

Thursday, February 20, 1947

After the usual tour of the labs in Building 4, I looked over the mail--this included the following useful list of the members of a joint Congressional Committee on Atomic Energy. I also worked for a while on part of the material that will form the basis of the talks I am scheduled to give next month.

C
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The following members have been appointed to a joint Congressional Committee on atomic energy, required by law, to make a continuing study of the Atomic Energy Commission and report to Congress from time to time on its findings:

SENATE:

Arthur Vandenberg	(R. - Mich.)
Bourke B. Hickenlooper	(R. - Iowa)
Eugene D. Milliken	(R. - Colo.)
William F. Knowland	(R. - Calif.)
John R. Bricker	(R. - Ohio)
Brien McMahon	(D. - Conn.)
Richard B. Russell	(D. - Ga.)
Edwin C. Johnson	(D. - Colo.)
Tom Connally	(D. - Texas)

HOUSE OF REPRESENTATIVES:

Sterling Cole	(R. - N. Y.)
Charles H. Elston	(R. - Ohio)
Carl Hinshaw	(R. - Calif.)
Van Zandt	(R. - Pa.)
Patterson	(R. - Conn.)
R. E. Thomanson	(D, - Texas)
Carl Durham	(D. - N. C.)
Chet Holifield	(D. - Calif.)
Melvin Price	(D. - Ill.)

The Atomic Energy Commission has appointed Colonel James McCormack as Director of the Division of Military Application. Colonel McCormack is a graduate of the United States Military Academy Class of 1932, was selected as a Rhodes scholar from the State of Louisiana, received a degree from the University of Oxford, England, and later received a degree from the Massachusetts Institute of Technology. During the war, Colonel McCormack served first on the War Department General Staff, dealing particularly with research and development matters, and then overseas in General Bradley's headquarters in Europe.

Friday, February 21, 1947

This morning I dictated a number of letters to Mary Bender.

I replied to an invitation, dated January 15, from Professor J. Timmermans (Solvay International Institute of Chemistry, Université Libre, Brussels) to attend the 7th International Council of Chemistry to be held in September; the subject will be isotopes in pure chemistry and their application to biological chemistry. Timmermans asked me to speak on "Discourse on the Results Obtained by the Method of Mass Spectra." I declined the invitation because of other commitments and said that Professor Hugh S. Taylor at Princeton is more qualified to speak on the applications of stable isotopes to chemical problems. For a speaker on photosynthesis, I suggested Professor Melvin Calvin.

In a letter to Colin G. Fink (Secretary of The Electrochemical Society, New York), I declined his invitation of February 11 to speak at a symposium on "Nuclear Reactions" at their Boston Congress in October, explaining that I have previous commitments and suggesting Charles Coryell (I sent a blind copy to Coryell) as a man well qualified both to speak and to make suggestions about other suitable participants.

Helen and I received a February 16 letter from Bertrand Goldschmidt, who is presently staying at the Barbizon Plaza Hotel in New York as an advisor for the French Atomic U.N. delegation. Bert announced that he is getting married soon to an English war widow, Naomi Reinach, whom he met in Paris after his return from Bikini last summer. I congratulated Bert in my reply and said that I possibly may be in New York on March 27 and would like to see him. I added that nine-month old Peter now weighs about 20 pounds and is full of energy.

I answered the February 1 letter from Bob Campbell about the proposed picture story on the Radiation Laboratory in Life magazine and told him that Dan Wilkes (Public Information Office of the University of California) will get in touch with him. Wilkes, I said, is enthusiastic but there are certain administrative questions.

Another letter went to Lyle Jensen. I explained that Ed Orlemann reminded me that, at the Metallurgical Laboratory, he (Jensen) did not feel that he could work on Saturdays because of his affiliation with the Seventh Day Adventist religion. I said I was remiss in not mentioning that it is the policy of the department that all members must be available to perform duties on any day of the week.

I sent Ray Stoughton our declassified note on U^{233}, explaining that it was subjected to rather heavy censorship but I believe it is worth publishing.

A copy of the manuscript that will form the basis for my talk, "Chemistry of Transuranium Elements," before the Wayne University Chemical Forum was mailed to Neil Gordon. In addition, I gave Gordon details of my travel reservations.

Iz showed me the memo he sent to Russell Ball (Area Engineer's office) requesting four 60-inch cyclotron bombardments: 500 μah of

deuterons on 10 g bismuth on a backing plate, 50 μah helium ions on 10 g lead on a backing plate, 1000 μah deuterons on 25 mg Pu^{239} on an interceptor, 500 μah helium ions on 2 mg Am^{241} on an interceptor.

Westrum sent me copies of his current correspondence with Eyring and Blaedel about their thermodynamic data and protactinium metal production, something I asked him to do. He also mentioned that one surprising result of experiments on melting points was that americium welded itself to tungsten electrodes at temperatures of about 950°C. Another observation was that americium wets tantalum and forms a good bond. This may provide a means of handling a reasonable quantity as target material for bombardments on a tantalum interceptor without soldering.

Saturday, February 22, 1947

Earlier this week I received a letter from Vance Cooper who announced that he accepted the offer of the General Electric representatives to return to Hanford. As promised, I wrote him today, with a carbon copy to Lee DuBridge, to tell him of the tentative plans for the establishment of a National Nuclear Energy Laboratory in the Southern California area. I said the development is probably some years in the future but that he should get in touch with Lee DuBridge. Nuclear work at Hanford, I said, should increase his eventual value to the Southern California organization.

Additionally, I wrote an identical letter to Fred Albaugh to tell him about the proposed new laboratory.

I mailed a review of the Pauli book, Meson Theory of Nuclear Forces, to T. K. Cleveland, Editor, The Vortex (publication of California Section, ACS). I reported that "this book is only of limited interest to chemists and perhaps you may even decide not to publish it in The Vortex for this reason."

In his last letter Joe Kennedy asked my opinion on some work by Allen F. Reid and Albert S. Keston in a recent issue of The Physical Review. I agreed with Joe that the 56-day activity they assigned to I^{129} was misassigned and should probably be assigned to I^{125}. I also questioned some of the rest of the work. I said, "I agree with you that this would be an interesting problem for you people to look into."

James Grady asked me, in a letter dated February 17, for newsworthy material on my upcoming Indiana Section talk. Today I reported that I have obtained clearance for no new material but am drawing on my previous talks. I said, "If under such conditions you think there might be something newsworthy in the talk and you wish to make a press release, I would be glad, in view of the vagueness, to check it for appropriateness ahead of time."

A telegram arrived today from Professor J. Timmermans, whose letter I answered yesterday: "MAY I HAVE YOUR ANSWER TO LETTER OF JANUARY 15 FOR NEXT CONSUL INTERNATIONAL CHIMIE SOLVAY BRUSSELS SEPTEMBER 47". I immediately wired back: "SORRY CAN'T ACCEPT INVITATION. AIRMAIL LETTER LEFT YESTERDAY."

Sunday, February 23, 1947

I spent some time with Peter and also worked on some of my writing projects.

Jo Owen visited during the afternoon, and then, at 5:52 p.m., I left Berkeley on a train bound for Hanford.

Monday, February 24, 1947

Enroute to Richland. When I arrived in Portland, I transferred to a train to Pendleton. There I was met by a driver from Hanford and driven to Richland. I will stay at the Guest House during my visit.

Tuesday, February 25, 1947

In Richland. I attended a General Electric Nucleonics Project Consultants meeting in the downstairs lounge of the Transient Quarters. Present, in addition to me, were consultants Walter H. Zinn, Hood Worthington, Ernest O. Lawrence, Warren K. Lewis, and a number of General Electric Schenectady and Hanford people including Harry E. Winne, C. Guy Suits, K. H. Kingdon, D. H. Lauder, A. B. Greninger, O. H. Greager, W. Kelly Woods, Paul F. Gast, Roy Beaton, Herbert M. Parker and C. W. J. Wende. The meeting was presided over by Greninger.

Kelly Woods and then Paul Gast talked on the problem of expansion and stored energy in the graphite of the Hanford production piles. They also discussed the blistering of the uranium slugs and pile technology in general.

Beaton described the program of research, semiworks testing, and scale up to production of the Redox Process. The semiworks tests are scheduled to be finished by February 1, 1948, the pilot plant should be operating by October 1, 1948, and the full scale production plant should be operating by October 1, 1949, at a capacity of 1.5 tons of uranium slugs per day.

Greager spoke on the operating status of the Bismuth Phosphate Process. Absorption of I^{131} to prevent its escape presents a problem, as does waste disposal, in general, which costs $1 per gallon.

Herb Parker reported on health problems. Some Richlanders, he said, have about 0.01 microcuries of fission-product beta-particle emitters (Zr, Cb?) in a day's output of urine. All animals tested have some beta-particle activity.

Wende reported on the facilities in the production piles for special irradiations.

After lunch a summary meeting of the consultants with Winne, Suits, Kingdon, Woods, Greager, Lauder, and Greninger was held in Lauder's office. Concern was expressed about the need to set up a priority system to govern the competition for the space for special irradiations in the piles. Lawrence suggested that a sodium-cooled unit be set up in one of the three-inch test holes in order to obtain needed information for

future sodium-cooled piles, such as corrosion of iron by sodium. Zinn agreed that very large (1,000,000 kw or larger) piles of the future might be of the sodium-cooled graphite type. Lawrence urged that the three-year time scale for the Redox Process be speeded up. Winne emphasized that the three most immediate problems for General Electric, as trustees of Hanford, are (1) gas decontamination, (2) Redox Process, and (3) emergency pile design (because of the graphite problem). I agreed that the Redox Process must move along as fast as possible, even at the expense of the Chelate Process. I suggested that the Argonne National Laboratory support work for the development of the Redox Process; Greninger asked Zinn if this was possible, and Zinn said he believes it is but he will have to discuss it with his people.

At 8:30 p.m. I talked to the Richland Technical Society on "The Transuranium Elements." The meeting was held in the Conference Room of Kadlec Hospital.

[In Berkeley, the first session of Chemistry 123 was taught by Iz Perlman at 10:10 a.m. in Room 219, Gilman Hall. Members of the class this semester are Alfred D. Abshire, Anubhai H. Amin, Francesco Asaro, Alice D. Awtrey, Victor P. Bond, James A. Bassham, Robert S. Boydston, LeRoy A. Bromley, John E. Coles, Robert A. Conard, George L. Cunningham, San Hormasji Dalal, Lorel Lu Daus, Milton W. Davis, Russell K. Edwards, LeRoy Eyring, Ernest E. Faerber, George J. Fritz, Robert H. Goeckermann, Harold E. Granquist, Thomas E. Hicks, Allan S. Hirsch, Donald F. Howard, Betty P. J. Hurd, Richard Koch, Theodore J. La Chapelle, Edward J. Leshan, Manfred Lindner, Lawrence B. Magnusson, Roy D. Maxwell, Marian M. Maxwell, Robert A. Mitchell, William C. Orr, Robert L. Oswalt, Monroe A. Postrel, Roberta F. Pratt, Edward E. Reinhart, Barney Rubin, Daniel Sam, Edith M. Seadden, William B. Schwabacher, Kenneth Street, Stephan D. Thornton, Richard J. Weiss, Louis B. Werner, Mark L. Wilkins, John C. Wingard, and James M. Witt. Also sitting in on the class are Robert L. Cochran, Albert Ghiorso, Carl L. McGinnis, and Edwin L. Zebroski.

Dag Dreher and young Jerry visited Helen during the afternoon.]

Wednesday, February 26, 1947

In Richland. The members of the committee of the General Electric Nucleonics Project Consultants spent the day touring Hanford production facilities and conferring individually with various members of the General Electric Schenectady and Hanford staff.

In the afternoon I talked with O. H. Greager about Fred Albaugh as a possible employee, Albaugh's type of position, and his salary. I also talked with some General Electric people about possible support for George Watt's research, but I received no positive response.

Lawrence and I boarded Union Pacific train No. 19, leaving Kennewick at 10:10 p.m.

Thursday, February 27, 1947

En route to Berkeley. Lawrence and I arrived in Portland at 7:30 a.m. and then took UAL Flight 592 from Portland at 8:30 a.m. We arrived

in Sacramento at 12:47 p.m. and were met and driven to Berkeley by a Rad Lab driver. By switching to air travel, we arrived home from this trip a day earlier than scheduled.

Iz gave the Chem 123 lecture, which consisted mainly of definition of terms, this morning. We have decided to assign problem sets this semester, and Iz assigned a set due next Tuesday; Harry Hicks will be our reader for the course.

Amos Newton's monthly report to C. E. K. Mees (January 1 to January 31) was issued today. Newton described the synthesis of 1,2,5,6 dibenzanthracene. Then he discussed the fission in lead and bismuth, the bombardment of arsenic with 200 Mev deuterons, and the unusual reactions with high energy particles.

In my stack of mail I noted a letter from Walter J. Murphy (Editor, Industrial & Engineering Chemistry) about the manuscript by Yankwich and associates, "Correcting for Absorption of Weak Beta Particles in Thick Samples: General Method for Use in Tracer Work." The paper, Murphy said, does not yet meet the criticism of Dr. Frederick Henriques, one of the referees. Murphy asked me to serve as an arbitrator and give my opinion of the present version of the paper.

I read a letter from Charles Allen Thomas, President-Elect of the American Chemical Society. Charlie said that, at his request, Alden Emery extended him the pleasure of notifying me that the 1947 American Chemical Society Award in Pure Chemistry has been awarded to me. This will be announced at the spring meeting of the ACS and will be presented at the fall meeting. He emphasized, however, that this information is still confidential.

There was some correspondence between Kasimir Fajans and Aristid V. Grosse about the name of element 91, which I looked over . Fajans originally suggested the name "brevium" but seems, in this correspondence, not to have strong feelings about that name. He regards the name "brevium" as applying only to the isotope UX_2, which he discovered, and thinks the name "protactinium" for the long-lived isotope (Pa^{231}) should more appropriately serve as the name for element 91. Fajans suggested that Grosse correspond with Paneth.

Friday, February 28, 1947

This morning I called Fred Albaugh and told him about my conversation with Oswald H. Greager and the type of position that Greager has in mind for him at Hanford. Fred is interested in the job there but said that he has agreed to visit Schenectady soon about a position there. I then wrote Greager and suggested that it might be worthwhile for him to go to Los Angeles to talk with Albaugh. I gave Greager Fred's phone number and mentioned the Schenectady trip.

George A. W. Boehm (Radio Editor, ACS News Service) invited me on February 24 to appear on a "Science Forum" radio broadcast on April 16 in Schenectady. I answered today that I would be pleased to appear but said that I am not certain that I can appear on a Wednesday. I described my up-coming schedule to Boehm.

At home Helen told me that Katherine Owen, Jo's sister, dropped in to see her for a few minutes today.

Saturday, March 1, 1947

Most of my morning was spent in Building 4, where I made the usual rounds of the laboratories to check on the research, and read.

Roy Heath, who is visiting the West Coast for Wyandotte, and I had lunch together at Larry Blake's. He told me about a consulting arrangement he is trying to work out between John Willard, now at the University of Wisconsin in Madison, and Wyandotte.

Sunday, March 2, 1947

I played with Peter, read for a while, and then worked on the abstract-summary for my talk on March 21 at Western Reserve University.

Today is Helen's birthday, and her mother phoned to wish her a happy birthday. As a present I gave her my usual box of chocolates, and Pete demonstrated his newly discovered creeping ability.

Monday, March 3, 1947

After talking with some of the fellows, I looked over the morning mail. I read a note from Margaret S. Griffin (Publications Manager, American Institute of Physics), who said the manuscript, "Nuclear Properties of U^{233}: A New Fissionable Isotope of Uranium," by J. W. Gofman, R. W. Stoughton, and G. T. Seaborg will appear as a "Letter to the Editor" in the March 15th issue of The Physical Review.

I also received a letter from Neil Gordon, verifying the arrangements for my talk at Wayne University on March 20.

I received and read a memo from Earl R. Miller to E. O. Lawrence. Miller is concerned about the hazards in fighting fires in areas of radioactivity; he made some suggestions for minimizing or eliminating the danger.

A lengthy letter went to Edward Shapiro at Clinton Laboratories, who is interested in a position with us but questioned the permanency of the job. I have discussed the matter with Latimer and Lawrence and learned that, since the Radiation Laboratory is a part of the University of California, the only really permanent positions are those held by the staff of the University of California itself. In my reply letter I communicated this information to Shapiro and said, "In view of this I believe that you will want to reconsider whether you would want to come out here under these conditions."

Another letter went to Simon Freed at Clinton Laboratories. Burris Cunningham talked with Freed when he (Burris) visited there in January about joint research projects on the spectroscopy of the heavy and transuranium elements. In my letter I explained that Professor Rollefson and our new man John Conway are just now remodeling a room for their spectrographic equipment and feel that it is too early for a profitable visit for Freed.

To George Watt I wrote that, as a result of my meeting in Hanford, I

believe that a subcontract with General Electric is not feasible and that his best bet is to negotiate with the Atomic Energy Commission. However, I do not know how the AEC will handle small subcontracts. I told Watt that we will accept applications for graduate work here from his outstanding seniors since we think so much of his Texas chemists.

In addition, I wrote John Willard to ask for applications to graduate school from some of his best seniors.

I briefly looked over my notes for the Chem 123 lecture tomorrow.

Helen is having more difficulties with her pregnancy and is again staying in bed; however, Peter learned to sit up from his stomach, and this is entertaining.

Tuesday, March 4, 1947

In attendance at this morning's group meeting were Asprey, Blaedel, Conway, Covey, Cunningham, Eyring, Garden, Ghiorso, Hopkins, Howland, Huffman, James, La Chapelle, Lilly, Lindner, Magnusson, McCullough, Miller, Morgan, Newton, O'Connor, O'Brien, Orlemann, Perlman, Prescott, Robinson, Seaborg, Templeton, Thompson, and Werner. Templeton reported on his bombardment of bismuth with 400 Mev helium ions, in which he found fission product activities in the chemically separated gallium, bromine, strontium, yttrium, molybdenum, ruthenium, tellurium, iodine, barium, and cerium fractions. There was a discussion of the distribution curve, which seems to be symmetric with a peak at mass 100. Templeton and I then talked about the need to know the cross section for the formation of At^{211} in order to determine whether it is possible that it is formed by 30-40 Mev secondary alpha particles. I suggested that our next experiments in this region should be bombardments of thallium, platinum, and gold with 400 Mev helium ions in order to look for fission; also we should bombard bismuth with 150 Mev neutrons and look for fission.

McCullough corrected information reported two weeks ago: protactinium metal has the same structure as thorium metal; however, this may be complicated by the presence of PaO or PaC. He also discussed his work on neptunium and americium metals.

O'Connor reported on his bombardment of uranium with 200 Mev deuterons (no alpha particles due to Pu^{234} were found) and his bombardment of thorium with 400 Mev helium ions (no alpha particles due to neptunium or plutonium were found). He looked for light fission products but found no activity in the iron fraction.

Then Cunningham announced that Ed Tompkins (Clinton) is coming out to help us set up ion exchange columns, which they have used for the separation of rare earths. This is expected to be a good method to separate actinide elements. Finally, Hopkins talked about the improvements being made in the chemical separation of arsenic from gallium, germanium, and selenium for use in his bombardments of arsenic.

* * * * *

At 10:10 a.m. I collected the problem set assigned last Thursday to

the Chemistry 123 class and then gave the lecture, covering such things as spin, magnetic moment, quadrupole moment, etc.

Today's mail brought a reply arrived from Lyle Jensen to my letter of February 21. Jensen said that, because of his religious convictions, it is not possible for him to work on Saturday and thus must decline our offer of an instructorship. He offered to help try to find someone for our position when he attends the Crystallographic Society meeting.

Unfortunately, I developed a migraine headache during the evening and could do no reading.

Wednesday, March 5, 1947

(Edla) Jane C. Boggs began working as a technician in our counting room today.

Among my phone calls was one to Peter Yankwich on campus to discuss some of the criticisms of the reviewer (Dr. Frederick Henriques) of his article for Industrial and Engineering News. I offered a suggestion that may handle the criticisms.

I then went around to some of the labs and talked with some of my graduate students--Dan Miller, Paul O'Connor, Stan Thompson, Larry Magnusson--about the progress of their research.

Later in the afternoon I did some report reading.

Thursday, March 6, 1947

This morning I looked over my notes for Chemistry 123 in my Gilman Hall office. Then, at the 10:10 a.m. lecture, I returned Tuesday's problem set, explained the problems, and then gave the class a lecture. Among the topics covered were the Bohr model of the nucleus and the Fermi theory of beta decay.

As usual I stopped in the Department of Chemistry office after the lecture to speak with various colleagues and Miss Kittredge. I arranged with Professor Gibson and Bill Dauben to go to Mira Vista Country Club after lunch for 18 holes of golf (GEG-103, WGD-108, GTS-96) and conversation.

Friday, March 7, 1947

After dealing with some administrative matters and phone calls, I talked with some of the fellows and then looked over the mail: In a letter dated February 28, W. H Sullivan stated that the meeting of the Subcommittee on Nuclear Constants will be held on Saturday, March 22, at 9 a.m. in Jones Laboratory, Room 208 (University of Chicago). Sullivan also gave a few new points of data for our "Table of Isotopes."

I received a note from W. D. Leech of the Pasadena City Schools, verifying the April 29 date for my Faraday Lecture; the talk will be at 8 p.m. and will be preceded by a dinner at 6 p.m.

James Grady (ACS News Service) sent me a press release about my upcoming talk to the Indiana Section. Today I wrote and asked him to delete the first three or four paragraphs--since I am now a member of the GAC, I do not want to make any statements that might be interpreted as official policy of the AEC. The material about commercial applications of atomic energy, I wrote, is all right.

I returned the contract sent to me by D. E. Chambers (General Electric, Schenectady) and asked that it be redrawn and made out to me directly.

Helen reported, when I got home, that Katherine Owen and her mother came by today to give her some jewelry that Jo had made for Helen's birthday--Jo is ill.

Saturday, March 8, 1947

At my office I dictated a reply to a February 21 letter from M. C. Sneed (University of Minnesota) about the volumes, Comprehensive Inorganic Chemistry, to be published by D. Van Nostrand Company. I explained that, although I have little time to devote to this, I agree to prepare the section on the plutonium family because there is very limited open literature material available now. In answer to his question about preparing a chapter on the entire actinide series, I said the only way I could do this is to use material to be prepared by him on thorium and uranium and then cover the other elements, adding some general statements on the actinide series.

Another letter I answered came from Beryl Gilman of Technical America, who asked for an article on plutonium that would be understandable to technical workers, from architects to engineers. In my response I said that I would like to write such an article but, because of my travel schedule, it will be several months before I can prepare it.

Ed Lingafelter (University of Washington) wrote on February 24 to suggest a possible candidate for our x-ray diffraction position. Unfortunately, the man he suggested has vision problems. In my reply I explained that we intend to have all of our future work carried on by University of California staff members and I do not feel the man he suggested would fill the bill as a member of the faculty.

Professor George Glockler (State University of Iowa), who is writing a general chemistry book, recently asked about the name of element 61. In my reply today I said that my only connection with this is of one who is interested in seeing that an authentic name is given to it. I said, "I believe that it is fairly well established now that the naming will be done by L. E. Glendening and J. A. Marinsky of MIT, now recognized as the discoverers." I suggested that Glockler write to them.

A telegram arrived from Welden Reynolds (Encyclopaedia Britannica), saying, "CHADWICK'S PAPER ON NATURAL RADIOACTIVITY JUST ARRIVED. WOULD YOU LIKE AN OUTLINE OF TOPICS?" I immediately wrote and asked for both an outline and a copy of the article, saying that I should be able to finish my article by some time in May. About the other articles (on the transuranium elements and the three or four elements immediately

preceding neptunium) that I am scheduled to write, I informed Reynolds that the new work on these elements has not yet been published and probably will not be for another year. There is, I noted, general agreement that the work should not be published elsewhere before appearing in the Plutonium Project Record. I then asked Reynolds to send me copies of the present articles on uranium and thorium and also those on actinium and protactinium if such exist.

I sent a note to Charles A. Thomas (President-Elect of the ACS) to say that I am pleased to learn I have been awarded the 1947 ACS Award in Pure Chemistry.

Another note went to W. R. Kanne to inform him that, as a consultant to General Electric, I shall be in Schenectady on March 25.

In a memo to Don Cooksey, I pointed out that, since there are so few men working on the Davis research program, it is not desirable to have monthly reports on that program. However, I said, we shall keep in fairly close touch with Herb Young.

Sunday, March 9, 1947

Part of my day was spent drafting a talk, "Scientific Background for Atomic Energy and its Control," to be recorded next Saturday for broadcast on radio station KQW.

I tried to help entertain Peter during the day so that Helen could get some rest.

Monday, March 10, 1947

As usual on Monday, there was quite a bit of mail. Albert C. English (formerly of the Chalk River Laboratory and now at Northwestern University) wrote to give me the status of the Canadian paper (A. C. English, E. P. Hincks, T. E. Cranshaw, and J. A. Harvey) on the 4n + 1 series. English explained that they expect to have a letter ready in a few weeks but a major publication will not be finished for some time. He asked for a summary of the work that we will get declassified.

A letter arrived from John Willard, in which he announced that the University of Wisconsin will have openings for new instructors and assistant professors next year. He is also interested in good graduate students. In a postscript, John noted that he just received my letter inviting applications for graduate students.

George A. W. Boehm (Radio Editor, ACS News Service) wrote to explain that the March 26 date of the "Science Forum" radio program is already filled and said they will hold open the date of April 23. Boehm suggested I write to Caleb Paine of General Electric directly. I then received a telegram from Caleb Paine stating that March 26 is still open and asking if I can take part in this broadcast. I wired back, "ACCEPT MARCH 26TH DATE PROVIDED CAN MAKE 8:18 PM TRAIN TO NEW YORK. DO I NEED TO KNOW MORE ABOUT EXPECTED CONTENT OF TALK?"

I wrote to Oliver Grummitt (Western Reserve University) to send him

the abstract-summary of my talk, "The Transuranium Elements," for the March 21 "Frontiers in Chemistry" series and to give him my arrival time in Cleveland. I requested a slide projector for the talk and asked for a hotel reservation.

I answered Walter J. Murphy's letter about the Yankwich manuscript and suggested one essential change that I believe will handle many of the criticisms of the reviewer.

Iz reported he asked Russell Ball for authorization for 2500 μah of helium ions on a thorium backing plate on the 60-inch cyclotron.

Other activities today included some work on the notes for tomorrow's Chemistry 123 lecture and discussions about the research.

Tuesday, March 11, 1947

Present at this morning's group meeting were Asprey, Blaedel, Connick, Conway, Covey, Cunningham, Eyring, Garden, Ghiorso, Goeckermann, Hopkins, Huffman, La Chapelle, Lilly, Lindner, Magnusson, McCullough, Miller, Newton, O'Connor, Prescott, Reynolds, Robinson, Seaborg, Stewart, Templeton, Thompson, and Werner. Reynolds described the mass spectrograph that he is designing for use in identifying isotopes produced in bombardments.

O'Connor announced that work has just begun on the fission of uranium with 400 Mev helium ions, which indicates that it is symmetric (high yields of silver and tellurium) with high yields in the low mass wing (gallium and bromine). I pointed out that the yields include all elements from uranium itself down as low as manganese (atomic number 25). Templeton said he performed a volatilization experiment and found a high yield of At^{211} (31 mass units down)! Lindner mentioned his experiments to look for fission of tin with 400 Mev helium ions; the results seem to be negative so far.

Eyring reported on the alterations they are making to Westrum's microcalorimeter to be used for heats of reaction of transuranium elements.

Then Ghiorso talked about his Chicago work on the fissionability of Pu^{238} and Cm^{242}. Two separate measurements of the slow neutron fission cross section of Pu^{238} gave values of 23 and 38 barns that, when corrected for the presence of Pu^{239}, lead to a fission cross section of 4 barns for Pu^{238}. The results may indicate a large neutron capture cross section (500-1000 barns) for Cm^{242}; deduction of a fission cross section for Cm^{242} is complicated by the probable presence of highly fissionable Cm^{243}.

There were several additional reports on work with the 184-inch cyclotron: Goeckermann reported additional positive identification of symmetric fission of Tl^{205} with 400 Mev helium ions. Newton described his rough determination of an excitation function for thorium fission with helium ions (from 400 Mev down to the end of the range of the helium ions). Hopkins reported on the yield of gallium, germanium, arsenic, and selenium from 200 Mev deuterons on arsenic; and Miller described the

relative yields of a large range of products from the bombardment of copper with 200 Mev deuterons.

* * * * *

At 10:10 a.m. I gave the Chem 123 lecture in Room 219, Gilman Hall. After discussing the general process of beta emission, I went on to talk about radioactive decay laws. I also gave the class another problem to work for Thursday.

Up on the hill I found that a telegram had arrived from Caleb Paine stating that I easily can make the 8:18 p.m. train to New York following the "Science Forum" radio broadcast on March 26th. He gave me a few pointers for my 15-minute broadcast. I then wired K. H. Kingdon (General Electric) to inform him that I shall be in Schenectady on both March 25 and March 26.

Edward R. Tompkins of Clinton Laboratories has arrived in Berkeley in order to work with our group, particularly Burris Cunningham, for about a month. We are particularly interested in Tompkins' expertise in the use of ion exchange resins to separate rare earth elements from each other. We want to apply this method to our separation of tripositive actinide elements from each other.

I attended both the Connick-Seaborg groups meeting in the afternoon and the Chemistry Research Conference in the evening.

Wednesday, March 12, 1947

This morning I received a telegram from Welden Reynolds asking how long I want to keep the Chadwick article, "Natural Radioactivity." They have only one copy and want to begin processing it immediately. By return wire I said that I will settle for an outline of the article and perhaps see the article when I am in Chicago on March 22 and 23. A telegram also arrived from Oliver Grummitt stating that my abstract for the "Frontiers in Chemistry" talk is satisfactory and that he has reserved a room for me in the Hotel Cleveland.

After checking the status of some of the experiments in Building 4, I worked over my lecture notes for Chemistry 123 and did some reading.

Helen told me when I got home that Rebecca Cason visited her this afternoon.

Thursday, March 13, 1947

I checked in with Miss Kittredge and various colleagues and then looked over my lecture notes in my Gilman Hall office--I gave the lecture to the Chemistry 123 class. Today's topics included the Fermi relationship, the Wigner nuclei, and the Dirac relativistic equation for the electron. I assigned another problem for next Thursday.

After speaking with Geoffrey Wilkinson, I went to my hill office. A revised draft of the explanatory material for our "Chart of Nuclides" arrived from Truman Kohman. He included comments on a number of the new

entries and said he is employing the notation Cu^{65} d* to indicate a reaction involving the emission of many particles. Such reactions, he said, might be called "star reactions" by analogy with the terminology used when similar reactions are observed in photographic emulsions.

After lunch Professor Gibson and I went to Mira Vista and played 13 holes of golf. My extrapolated score for 18 holes was 97.

Friday, March 14, 1947

I took care of some administrative matters and then looked at the morning mail, which included a letter from Josef Mattauch in Switzerland thanking me for sending the reprints he requested. Mattauch said that he noted that I am a sponsor for the Committee for Foreign Correspondence of the Federation of American Scientists and added that he is interested in their deserving activities--he has written for their publications. He then asked to be put on a list for receiving University of California reprints and stated that he is sending me, under separate cover, a set of reprints from his institute.

Oliver Grummitt wrote to describe the audience for the "Frontiers in Chemistry" series and to again ask for an outline. (However, I mailed him the abstract last Monday, and he responded by telegram on Wednesday.)

Charles P. Smyth sent me some train timetables to help me plan my Princeton trip (March 27). I promptly wrote to thank him for them and to state that I shall arrive in Princeton at 3:15 p.m. I also added that I should like to have dinner with him, but I believe I should leave Princeton Junction for Washington at 8:08 p.m.

I mailed Joe Katz three copies of Paper 22.3 ["The Neptunium (4n + 1) Radioactive Family" by G. T. Seaborg] of Volume 14B of the Plutonium Project Record. Katz, Winston Manning, and I are Associate Editors for this volume. I also mentioned to Katz that I shall see him in Chicago on March 22.

Another note went to John Lewellen to inform him that I will not be in Chicago on March 23 but that I probably shall be able to see him there on Sunday, April 20.

Also arriving in today's mail was a schedule from Kenneth Kingdon (General Electric) of my consultations and talks during my visit to Schenectady on March 25 and 26 .

Helen had an appointment to see Dr. Borson yesterday that she had to cancel because she was not feeling well. Today Dr. Borson came to the house to see her and gave her instructions to continue with the bed rest.

Saturday, March 15, 1947

I spent the morning and some of the afternoon in my office and the laboratories in Building 4. Today I particularly wanted to confer with Burris Cunningham and Ed Tompkins about their work on the separation of americium and curium using ion exchange resins.

As we often do on Saturday, Stan Thompson, Tom Morgan, and I went to Larry Blake's for lunch.

Later I recorded a talk, "Scientific Background for Atomic Energy and Its Control," which will be broadcast over radio station KQW next Sunday at 9:30 a.m. I described the Baruch proposal to the United Nations: "The plan is to set up an international Atomic Development Authority, which would control all uranium and thorium mines and all the refineries for making uranium and thorium metals out of the ores. By controlling these, the international agency can make sure no individual nation has the raw materials to use in making atomic bombs. ...In this way, by directly managing all the processes which deal with dangerous materials, the international Atomic Development Authority would be able to make sure that no nation could make atomic bombs secretly."

Sunday, March 16, 1947

I entertained Peter (or more truthfully, he entertained me) and then spent some time today reading and correcting the typed draft copy of my article, "Artificial Radioactivity," for the Encyclopaedia Britannica. Additionally, I worked on my 15-minute talk, "The New Elements," that I will present on March 26 on radio station WGY in Schenectady.

Al Ghiorso and his young daughter Kris, who was particularly interested in seeing Peter, stopped over for a visit. Later Jo Owen dropped in to see us.

Monday, March 17, 1947

Iz and I discussed some administrative matters; then I looked over my mail. I read a letter from Ray Stoughton, who noted that the security censorship didn't leave much information in our U^{233} note. Ray asked for 20 reprints. He then said that he believes that all the men who wanted to do graduate work and whom he would recommend have gone to universities already, but if he finds any in the relatively near future he'll let us know.

A note arrived from Ralph E. Lapp, who is now with the Research and Development Division of the War Department General Staff, stating that he hopes to see me when he visits the Radiation Laboratory on March 19 and 20. [Unfortunately, I shall be out of town on those days.]

Alden Emery informed me that the first announcement of the 1947 ACS Award in Pure Chemistry (financed by Alpha Chi Sigma) will be made at the ACS meeting in Atlantic City in April but the award will be presented at the New York meeting in September, at which time I, as the recipient, will be expected to deliver an address.

M. C. Sneed agreed to the proposals I made in my letter to him of March 8 about the coverage of the articles I am to write for his Comprehensive Inorganic Chemistry. However, he did not agree with my comment that my contributions will not be worth an honorarium.

Boris Pregel sent me a copy of a pamphlet, "Formules de Constitution Nucléaire et Normalization des Corps Simples," written by an acquaintance

of his, Mr. Hèly. Pregel asked for my comments.

I looked over a report, "Mass Spectrograph Design for Determining Mass Assignments of Radioactive Isotopes," by Fred Reynolds. Reynolds described the Chicago machine and work he did while visiting there with Inghram and Hayden.

Pete had an appointment with Dr. McAlear at 1:45 p.m. and received his first whooping cough immunization. He now weighs 20 1/4 pounds and is 28 3/4 inches tall.

Tuesday, March 18, 1947

At today's group meeting the following people were present: Blaedel, Cunningham, Eyring, Garden, Goeckermann, Hopkins, Huffman, James, La Chapelle, Lilly, Magnusson, McCullough, Miller, Morgan, Newton, O'Connor, Perlman, Robinson, Seaborg, Stewart, Templeton, Thompson, and Werner. There were several announcements: a 1 mg Co^{60} source has been received from Oak Ridge (5.3-year gamma emitter, equivalent in intensity to 1 mg radium, to be used by Segrè to measure its decay rate with his new balanced circuit); the chemical storeroom should be used as little as possible; and a description of the health physics setup was given. It was also reported that the construction of Building 5 and the hot lab is being held up because of lack of laborers for digging foundations and laying of pipes. In response to a remark of Morgan that the disposal of waste solutions is not being taken care of and to a question from Perlman, Garden said that short-lived activity, when operations are in a region where they will not interfere with other people, can be dumped down the drain; however, it collects cumulatively in drains and stops in the first 400 feet of pipe. He suggested that long-lived materials be put aside and taken out to sea.

James described the bombardment of Pu^{239} with 40 Mev helium ions. The expected 17-hour Am^{242} was not seen in the chemical fraction isolated by fluoride and fluosilicate cycles.

Goeckermann talked about the results of the bombardment of bismuth, lead, thallium, and platinum with 400 Mev helium ions to search for fission by chemical separations of fission products and his calculated fission yields. The cross section for bismuth is highest, those for lead and thallium about equal, and that for platinum, if indeed it does undergo fission, is 10 times less than for lead and thallium. I said there is a need for a term to describe reactions where 5-20 particles are knocked out. Perlman said McMillan has suggested calling them "star reactions." I offered "splintering," Perlman "chipping," Eyring "depletion," McCullough "fragmentation," Hopkins "particular reaction," and Morgan "neutronogenetic reactions."

Werner then reported that the separation of americium and curium was a "notable flop." I noted that one of the reasons for obtaining a pure curium sample is to bombard it in the 60-inch cyclotron.

O'Connor described the fission products from the bombardment of uranium with 400 Mev helium ions. He reported his relative yields of chemically separated products from gallium to barium and described his

separation by volatilization of At^{211}, Po^{210} and Po^{208} by heating a portion of the original target.

Finally, McCullough talked and showed his plot of the lattice constants and densities for heavy metals (atomic numbers 90-94 inclusive) with face-centered cubic structure; however, it is not certain that the values for protactinium and neptunium might not correspond to PaO and NpO.

* * * * *

The Chemistry 123 lecture was given at 10:10 a.m. and a midterm was announced for March 27. Among the topics discussed today were nuclear isomerism and the measurement of beta particles and gamma rays.

On the hill I attempted to clear my desk of pending correspondence, preparatory to my coming trip.

I answered a March 4 letter from E. B. Christiansen (a friend from Met Lab days) from the University of Idaho, who asked about obtaining some more radioactive material for use in fluid flow experiments. Joe Hamilton, I told Christiansen, will ship some deuteron-bombarded copper containing the 250-day Zn^{65} isotope. I then suggested that, if this material shows promise for his experiments, he request more radioactive material from Paul C. Aebersold, Isotopes Branch, Oak Ridge.

In addition to the unclassified letter I received from Stoughton yesterday, I also received a secret letter asking about the assignments of bismuth, lead, and polonium isotopes. I answered today and said there will be full reports within a month and gave him brief reasons for our assignments. I also told him, in reply to his query, that we have been using the value of some 10^4 b as the cross section for the slow neutron destruction (n,γ and fission) of Pu^{241}.

In a memo to E. E. Coleman I requested that he expedite the installation of a home telephone for B. B. Cunningham.

A brief acknowledgement went to Truman Kohman for the draft of the explanatory material for the "Chart of Nuclides" I received last week. I said I agree in general with it although Perlman and I probably shall use the title, "Table of Isotopes," for our compilation. (Truman is adamant about using the term "nuclide" for the broad application.)

I declined a March 13 invitation from Léon Lortie to give one of the main addresses on June 10 at the 1947 Conference of the Chemical Institute of Canada at Banff, explaining that I have previous commitments.

I also acknowledged the outline for Chadwick's article on natural radioactivity from Welden Reynolds (it arrived yesterday) and told him I shall spend Saturday, March 22, at Argonne and will try to call him (in case I should want to see the entire Chadwick article).

In a memorandum to Don Cooksey, I informed him that I shall give a talk, "Chemical Identification of Transmutations Induced with the 184-inch Cyclotron," at the Chicago Information Meeting in April. I told Cooksey, who requested this information for Lawrence, that I shall be

leaving Chicago on the night of April 22 and therefore cannot speak on April 23.

I also wrote a note to K. H. Kingdon (Schenectady) that the schedule he suggested in his letter of March 12 seems satisfactory. I informed him that I shall discuss the latest results on the identification of nuclear reactions produced by the 184-inch cyclotron in my classified talk on March 25. The general subject of the talk on the following day should be "The Transuranium Elements."

A copy of the text for my talk on March 26 on radio station WGY was mailed to Caleb Paine in Schenectady.

The following letter arrived from Edgar Westrum:

> Recently I discovered that I am blessed with a spring vacation (April 3rd, et seq.) and unless some equipment that I have need for my work here arrives before then--I would be able to devote about 11 full days of research at Berkeley to the dry chemistry cause--should this prove agreeable and advantageous to everyone concerned. This will be nearly three more days than was available at Christmas and with fewer holidays, and less chance of Walt (Blaedel) producing another child, it might be a useful trip.

Westrum also announced that he will be in Cleveland for my talk this week and then will go to Chicago. I called Ken Priestley and made arrangements for a consulting visit in April for Westrum and then wired him: "ARRANGED TRIP HERE FOR YOU IN APRIL. SEE YOU IN CLEVELAND. AM SPENDING FRIDAY AT HOTEL CLEVELAND. ALSO AM GOING TO CHICAGO ON FRIDAY MIDNIGHT TRAIN NYC 89."

At 5:46 p.m. I left on the "City of San Francisco" (Roomette 10, Car 1023) bound for Chicago. Shortly after the train got underway I remembered that I had forgotten to send a report to Oppenheimer, so I prepared the following telegram and sent it to Mary Bender when we stopped in Sacramento: "SEND COPY PRODUCTION REPORT TO OPPENHEIMER INCLUDING CONCLUSIONS AND RECOMMENDATIONS, SEND TO HIS UNIVERSITY CALIFORNIA OFFICE."

Wednesday, March 19, 1947

Enroute to Chicago on the "City of San Francisco." During part of the trip, I went over the material for the talks I have coming up.

Thursday, March 20, 1947

I arrived in Chicago at about 10:45 a.m. At 1 p.m. I caught the NYC train, the "Carolina Special" (seat in Section 4, Car 465), for Indianapolis. A. N. Stevens (Editor of Research Today) met my train when it arrived in Indianapolis at about 5 p.m. The dinner for the Indiana Section of the ACS was held at 6:30 p.m. in the Warren Hotel with about 100 members present, and at 8 p.m. I addressed the group on "Plutonium and Nuclear Energy." Afterwards I was taken back to the train station, where I took the 11 p.m. overnight NYC train, "Cleveland Special," (Lower 6, Car 4200).

[In Berkeley. Iz Perlman gave the Chem 123 lecture and collected the problem set.]

Friday, March 21, 1947

In Cleveland. My train arrived at about 7 a.m., and I went to the Hotel Cleveland, where a room was reserved. After resting for a while and going over my talks for the "Frontiers in Chemistry" series, I visited the Harshaw Chemical Company. There I was shown the enormous single crystals (weights of the order of pounds) that they produce by the process of solidifying under a thermal gradient. The crystals of AgCl, LiCl, NaCl, etc. are clear and glasslike, and their applications are increasing.

Later Edgar Westrum, who came down from Ann Arbor for the talks, stopped by my hotel room to discuss with me his plans to spend his spring vacation in Berkeley.

I gave two talks--one at 5:15 p.m. and, after a quick dinner with the group, another at 7:45 p.m.--in the Physics Laboratory Lecture Room, Western Reserve campus, on "The Transuranium Elements." The first lecture was devoted to a description of the discovery and methods of production of the known isotopes of the transuranium elements. I said in part:

> There are now known the four transuranium elements: neptunium (atomic number 93); plutonium (atomic number 94); americium (atomic number 95); and curium (atomic number 96). There are six radioactive isotopes of neptunium, three of plutonium, and two each of americium and curium. These are produced variously by irradiation of the available isotopes in this region with neutrons, deuterons, and helium ions. The neutron irradiations are now preferably performed by using the uranium piles, while the high energy charged particles are produced in the cyclotron. Several of these isotopes can be produced in more than one way. Of particular interest are the three isotopes Np^{237}, Pu^{239}, and Am^{241} because these are long-lived and have all been isolated in weighable amounts. Thus, it has been possible to study the chemical properties of these three elements by the methods of macroscopic chemistry in order to augment the information which was first obtained by the tracer technique. The isotope Pu^{239} is, of course, of the major practical importance because this is the famous source of nuclear energy. This isotope is available in larger amounts than the others as a result of the successful development of the uranium pile for its relatively large-scale production. Fortunately, it is also true that the isotope Np^{237} is formed as a by-product of Pu^{239}, and it is therefore also available in significant amounts.
>
> The availability of tracer isotopes of the four transuranium elements and the three above-mentioned isotopes that exist in weighable amounts has made it possible to learn a great deal about the chemical properties of the transuranium elements. This has led to the view that a transition series is present among the heaviest elements in which the 5f electron shell is being filled. Apparently this series begins with thorium, in the same sense that the rare earth series

begins with cerium, and the half-way point is reached with curium (element 96), where seven 5f electrons are obtained.

In my second lecture I described the early chemical work done in connection with the Plutonium Project in order to develop the chemical procedures used for the extraction from uranium and fission products. Particular emphasis was placed upon the interesting work 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. I said:

> In addition to the need for work with pure plutonium in connection with the separation process, it was necessary to determine a number of the physical and chemical properties of the dry salts of plutonium and of plutonium metal. Therefore the investigation on the ultramicrochemical scale had to encompass this field also.

I included a description of the techniques and instruments used in this extremely small-scale of chemical investigation and illustrated the talk with a number of slides picturing the type of equipment and some of the early plutonium compounds that were isolated. The honorarium for these lectures is $125.

At nearly midnight Westrum and I boarded the NYC train, "The Forest City," for Chicago. (I occupied Lower 5 in Car 891.)

Saturday, March 22, 1947

In Chicago. I arrived at the Englewood Station at about 7 a.m. and took a taxi to the Hotel Windemere East where I checked in. After breakfast I took another taxi to the University of Chicago campus in order to attend a 9 a.m. meeting in Room 208, Jones Laboratory, of the Subcommittee on Nuclear Constants of the Committee on Radioactivity of the National Research Council. Members present were W. H Sullivan (Chairman), T. P. Kohman (Secretary), G. T. Seaborg, K. T. Bainbridge, H. H. Goldsmith, B. T. Feld, D. E. Hull, and L. F. Curtiss. It was agreed that the function of the group is the compilation, evaluation, presentation, and publication of experimental data on the nuclear properties of stable and radioactive nuclear species, and on spontaneous and induced nuclear reactions. Sullivan reported on his chart of nuclear species being prepared at Clinton Laboratories; it utilizes a triangular coordinate system with each nuclear species represented by a hexagon. Bainbridge accepted responsibility for compiling a summary of reported values of natural isotopic abundances and a compilation of atomic masses. Perlman and I are keeping our table of isotopes up to date. Feld is compiling nuclear spins, magnetic moments, etc. Goldsmith is summarizing neutron cross sections. Martin Deutsch has another subcommittee, whose function is to improve disintegration schemes. Sullivan described an effort at Clinton Laboratories to compile an exhaustive summary of nuclear data, etc. He also proposed monographs on important nuclear species. There was also a discussion of the problems in presentation of data and publication and distribution. I was assigned specifically to the area of nuclear constants for the subcommittee.

After the meeting on nuclear constants, I dropped in at the meeting of the Subcommittee on Radiochemistry that was being held in Room 316, Eckhart Hall. Present were C. D. Coryell, W. E. Cohn, R. B. Duffield, G. Friedlander, C. S. Garner, J. W. Irvine, Jr., S. Katcoff, J. D. Kurbatov, W. F. Libby, W. M. Manning, R. W. Spence, N. Sugarman, and A. C. Wahl. L. F. Curtiss, A. L. Turkevich, A. M. Brues, and I were present during part of the meeting. While I was present, there was a discussion of the need for declassification of publication of Project work. Sugarman and I pointed out that much of this has been done for the Fission Products Volume of the Plutonium Project Record. In a discussion of the need for a good textbook in radiochemistry, I reported that Iz Perlman and I are collaborating on a book aimed at the graduate student level.

In a free moment during the day I called Welden Reynolds of Encyclopaedia Britannica and told him that I have a rough draft of my article, "Artificial Radioactivity," prepared and nearly ready to send to him. At the end of the day I took a taxi back to the Windemere East Hotel and had a quiet dinner.

Sunday, March 23, 1947

I took a taxi from the Chicago Windemere East Hotel to the New Chemistry Building and spent the morning talking with Joe Katz about the PPR.

After lunch I went by taxi to the Woodlawn Station, where I caught the 1:30 p.m. NYC train, "The Wolverine" (Seat 9, Car 87), bound for Detroit. I had dinner on the train and arrived in Detroit at about 7:30 p.m. Neil Gordon met me and drove me to my hotel--The Wardell-Sheraton.

Monday, March 24, 1947

In Detroit. I met with Neil Gordon, organizer of Wayne University's "Chemical Forum," who told me about the future plans for their chemistry department.

In the late afternoon I talked at the "Chemical Forum" on the subject, "Chemistry of Transuranium Elements." I summarized my talk with the following:

> As of today, the periodic system consists of 96 known, identified elements--that is, there is now positively known at least one isotope, stable or radioactive, for each of the elements from atomic number 1 to atomic number 96, inclusive. The evidence points to an atomic structure for the heaviest elements--that is, those elements with atomic number greater than 88--corresponding to a transition series in which the 5f shell of electrons is being filled. This series differs in chemical properties from the rare earth series (the 14 elements of atomic number 58 to 71, inclusive, following lanthanum) in which the 4f shell of electrons is being filled in that the first members of this heavy series are much more readily oxidized to oxidation states greater than III. As the atomic numbers of the elements in this series increase, the lower oxidation states, and particularly the III state, increase in stability. The first 5f electron may appear in thorium and the stable configuration

consisting of seven 5f electrons comes with curium.

Neil Gordon arranged the time of my talk so that, after a dinner with him and other hosts, I was easily able to catch the NYC train, "The Knickerbocker," (Lower 3, Car 3241) at 8:45 p.m., bound for Schenectady.

Tuesday, March 25, 1947

In Schenectady. I arrived in Schenectady at 7:30 a.m. and went by taxi to the Hotel Van Curler. After freshening up and having breakfast, I took a cab to the Research Laboratory of General Electric Company and consulted with the men there about their atomic power project. Later in the day I gave a classified talk on the latest results on the identification of nuclear reactions produced by the 184-inch cyclotron. (Athough this material is not yet all declassified, it is non-secret in nature and will be readily declassifiable.) A number of General Electric people had dinner with me.

[In Berkeley. There was a regular group meeting conducted by Iz Perlman. In attendance were Blaedel, Cunningham, Eyring, Goeckermann, Howland, Huffman, James, La Chapelle, Lilly, Lindner, McCullough, Miller, Morgan, O'Connor, Perlman, Robinson, Templeton, Thompson, Tompkins, and Werner. Lindner reported on a bombardment of tin with 400 Mev helium ions; he found no bromine activity but may have found 600 c/m of manganese and 100 c/m of copper.

Cunningham described a separation, effected with the help of Ed Tompkins, of a 33% americium-67% curium solution with a 20 cm Nalcite resin column eluted with 0.25 M citrate solution at pH 3.27. Roughly, 30% of the curium was purified ten fold with respect to americium and about 30% of the americium was similarly purified with respect to curium. Next the americium-lanthanum separation will be tried.

Blaedel reported on his density measurements of neptunium metal by the capillary method; his value of 19.2, in contrast to McCullough's value of 14.7, indicates that McCullough is making x-ray diffraction measurements on a thin layer of NpO and not neptunium metal. Blaedel also commented on his methods of melting point measurements. Since americium wets the tungsten jaws of the apparatus (described a few months ago by Westrum), the jaws were constructed of quartz--the result with silver was still 200° too high. Blaedel then talked about another method of measuring the melting point by following the change in electrical resistance of a thin film of the metal.

* * * * *

Iz gave the Chemistry 123 lecture at 10:10 a.m. in Room 219, Gilman Hall.]

Wednesday, March 26, 1947

In Schenectady. I again spent the day at the Research Laboratory of the General Electric Company. Dr. W. R. Kanne, with whom I have corresponded, and I had a lengthy conversation about some experiments he wants to do to determine the cross section for the n,γ reaction in

Pu^{239} with neutrons of energies above the thermal range. I promised to send him drawings of the containers we use for pile irradiations.

Then, at 3:30 p.m., I gave an unclassified general talk on "The Transuranium Elements."

Some of the General Electric people entertained me at dinner. Then, at 7:30 p.m. I spoke for 15 minutes on "The New Elements" during the General Electric "Science Forum" broadcast over radio station WGY. I concluded my remarks by saying, "One should not close without saying just a word about the possibility for the discovery of elements with even higher atomic numbers than 96. This, of course, is in the field of conjecture, but I think it is safe to guess that such elements will also be discovered in the future."

I left Schenectady at 8:18 p.m. on the NYC train, "The DeWitt Clinton," for New York City.

[In Berkeley, Mary Bender sent out two letters of recommendation over my name for William J. Knox, who is applying for graduate work in physics, using the letter I recently sent to the Department of Physics here as a pattern. The letters went to Professor I. I. Rabi at Columbia University and to Professor John C. Slater at MIT.]

Thursday, March 27, 1947

In New York City. I arrived at Pennsylvania Station a bit after midnight and took a taxi to the Hotel Commodore and checked in.

During the morning I called John Pfeiffer of CBS. Pfeiffer told me that CBS is planning a documentary about peaceful uses of atomic energy and will soon send one of CBS's staff to Berkeley to interview scientists about this subject. I agreed to making arrangements for such a visit.

In addition, I called Bert Goldschmidt at the Barbizon Palace Hotel and chatted with him about our days together at Chicago and his new fiancée.

At 1:55 p.m. I caught a train to Princeton, arriving at 3:15 p.m. Charles Smyth met me and drove me to the campus of Princeton University, where I spoke at a 4:30 p.m. seminar of Princeton's Chemistry Department on "The Transuranium Elements."

After an early dinner with Smyth and a few others, I caught a train at 8:08 p.m. at Princeton Junction for Washington. I arrived in Washington at 11:15 p.m. and took a taxi to the Hotel Statler.

[In Berkeley, Iz Perlman gave the Chem 123 class its first midterm.]

Friday, March 28, 1947

In Washington. I went by cab from the Hotel Statler to the new Washington headquarters of the Atomic Energy Commission (the newly vacated Public Health Building) at 1901 Constitution Avenue, N.W. Here the third meeting of the General Advisory Committee started at 9:45 a.m.

in Room 213. All the members were present: Robert Oppenheimer (Chairman), James Conant, Lee DuBridge, Enrico Fermi, Isidor Rabi, Cyril Smith, Hood Worthington, Hartley Rowe, and I. John Manley (Executive Secretary of the GAC), Commissioner Robert Bacher, AEC General Manager Carroll Wilson, AEC Director of Research James Fisk, General James McCormack (Director of Military Application), and Mr. Shivers (a security officer) also attended. Oppenheimer outlined a provisional agenda: (1) consideration of the letter written by the Chairman of the GAC to the Chairman of the Atomic Energy Commission following the second meeting of the GAC (February 2, 3, 1947); (2) action or considerations taken by the Atomic Energy Commission since that meeting; (3) the minutes of the second meeting; (4) the questions that have recently been received from the Atomic Energy Commission in three memoranda; (5) reports from the chairmen of the three subcommittees of the GAC; (6) method of approach to the problems thus placed before the GAC. The temporary agenda was approved, and the letter of February 4th to the Chairman of the AEC from the Chairman of the GAC, covering the second meeting of the GAC, was read and approved.

Wilson then summarized the actions and considerations of the Commission since the last GAC meeting. He described how the Monsanto plant at Oak Ridge has increased the rate of production of initiators for atomic weapons and their plans for further increase of production. The Commission, he said, has decided to keep the weapons laboratory at Los Alamos and has extended the contract with the University of California for its operation to July, 1948, and he reported that a small group has been given responsibility for preparing a paper about the need for tests of nuclear weapons. Provision has been made, he said, for production of Zinn's fast reactor at the site of the Argonne Laboratory and arrangements have been made with Stewart Oxygen Company for the distribution of deuterium. Wilson began a discussion of the three Commission memoranda by describing the problems the Commission faced when it took over operations from the Manhattan District--budget, scope of activities, etc. The first memorandum (Report of Subcommittee on Reactors) reads:

> The Subcommittee on Reactors has carried out a general survey of proposals for pile development. In the course of this survey it became plain that the construction of additional and replacement piles at Hanford is in a category by itself, and with the associated development and installation of the Redox process must have the highest priority. The design should not be substantially changed, - thus no change in the coolant should be considered, but changes that will not sensibly delay construction should be incorporated, such as modification of the graphite structure. No experimental facilities for power generation or for research other than simple irradiation should be included. This does not preclude the possibility of incorporating the results of experience as the program of construction advances. The possibility of using portions of the present water treatment plants for the new units should be considered.
>
> The main objective of the Hanford work should be steady production of plutonium until the operation of breeders is able to meet all demands for plutonium. There is no point in planning large expansion of

production without the installation of new methods of conserving raw material. This involves chemical recovery, as by the Redox process; high plutonium concentration (which forces a reconsideration of the effect of Pu^{240} on the weapon); and the use of isotope separation to make possible the reuse of depleted material by enrichment.

We list below four other reactors in the estimated order of decreasing importance; the high flux reactor, the Argonne fast reactor, the G.E. intermediate reactor, and the Clinton gas-cooled pile. We recommend that the detailed engineering design, including provision for recovery of fissionable materials, of the first two of these be authorized immediately, that construction be authorized as soon as possible, and that the necessary fissionable material be allocated unless unanticipated difficulties appear.

Though we recommend study and development of the resonance reactor by the General Electric Company, this must not be allowed to interfere with the development and installation of the further facilities at Hanford. The Argonne fast reactor and the high flux reactor are regarded as more urgent, and the Schenectady program should be pursued in a mannter to interfere as little as possible with their completion nor to compete for personnel. The high temperature power reactor seems to be unlikely to produce power promptly, and we do not recommend detailed engineering design. This action is without prejudice to further work on components.

All current reactor designs represent the first stage of developments that will require much detailed engineering for further improvement. Work on many of the components of power reactors can and should proceed independently of integrated design. Metallurgical and ceramic engineering groups should be encouraged to work out the different possibilities of incorporating fissionable and moderating materials in forms that are suitable for use at high temperature and that allow high rates of heat transfer. For breeders the design of structures that can be processed and reformed easily should be the object of special attention. It must be remembered that the overall breeding gain can be arrived at only by considering the frequency and efficiency of the reprocessing step as well as nuclear factors.

A serious study of the use of plutonium as a reactor fuel should be started, and facilities for its metallurgical manipulation should be established at one or two more sites.

Clinton High Flux Reactor

This unit is essentially a research tool. The disposition of fissionable material and coolant seem practical and the operation simple. The flux of fast neutrons is as high as in any planned fast reactor, and the thermal flux much higher.

We see as the primary purpose of this machine the testing of various materials and design elements for future reactors without the necessity of constructing a complete unit. The pile will also be used for investigations in physics, the production of isotopes, and general research purposes.

Argonne Fast Reactor

At present this seems to be the prototype of the breeder. Though of very low specific power, (25 kw per kg) it will provide experience with the actual operations of breeding and in particular with the cycling of material. This unit will yield much needed information on the lowering of the ratio of Pu^{240} and Pu^{239} that is expected from the use of fast neutrons. An evaluation of liquid metal (Na-K) cooling will also be obtained.

G.E. Intermediate Reactor

This reactor is planned at present for operation in the resonance region, and is designed for a specific power of 200 kw per kg. It is expected to test the feasibility of a breeder operating at resonance, which at present is completely unpredictable because of the lack of the necessary cross sections. The basic design is adaptable to operation over a wide range of neutron energies with appropriate adjustment in the amounts of fissionable material and moderator. It appears to us that the construction of this reactor is for the General Electric Company second in importance only to its primary vital responsibility at Hanford. Accordingly we redemand that the development of this unit be encouraged to the greatest extent that will not interfere with the Hanford program.

Clinton Gas-cooled Pile

This unit was originally conceived as a means of providing an early demonstration of atomic power. At present it does not appear that the design could be realized as soon or as simply as was believed initially. The attractive features of this scheme are its use of refractories capable of standing very high temperatures and the fact that gas above 600°C is produced, which conceivably could be used directly as a turbine.

[PARAGRAPH DELETED]

This memorandum suggests a policy for the scope of the AEC activities in view of the shortage of qualified personnel and other factors while the second and third memoranda apply this policy to the specific contracts of the General Electric Company and the Monsanto Company. There was a discussion of the time scale for the development of nuclear power for civilian use; there was agreement that this is probably years off in view of the military problem, the raw material situation, and the long-time scale for breeder operation. Fermi estimated that it would be 50 years before any nuclear electric power production at the magnitude of the electric power now consumed will be available. He suggested that breeding must be the ultimate basic solution for the shortage of uranium raw materials problem. We adjourned at 12:35 p.m.

The GAC reconvened at 2 p.m. with Manley and the security officer W. Balentine. Cyril Smith reported on the meeting of the Reactor Subcommittee held yesterday, suggesting that the Commission concentrate on the Clinton high flux reactor and a fast breeder reactor while giving

only limited attention to the General Electric reactor and the Daniels' high temperature pile.

Conant and Fermi then gave the report of the Weapons Subcommittee, with Conant describing the need for tests for nuclear weapons while Fermi urged that realistic theoretical studies of thermonuclear designs be made. I reported on the "Chemical Problems Attendant with Immediate Increased Production of Plutonium," based on the memorandum that I sent to the Commission, dated March 17, 1947, which stated that a substantial increase in plutonium production would depend more on additional reactors at Hanford than on breeders. I urged the early development of a plan for handling the irradiated uranium wastes at Hanford, explaining that the Redox Process, which will use solvent extraction techniques to recover both uranium and plutonium will help in the waste storage problem. Successful development of breeding, I said, might well depend on the development of a process such as the Redox Process. We adjourned at 5:15 p.m.

Saturday, March 29, 1947

In Washington. Attending this morning's 9:30 a.m. meeting were all nine members plus Manley, Fisk, Roger S. Warner (Director of Engineering), and Shivers (security officer). Fisk introduced a fourth Commission memorandum about Los Alamos, which we agreed that the Weapons Subcommittee will investigate during its visit there next week. We then went back to discussing the first Commission memorandum, the report of the Research Subcommittee, and consideration of a central laboratory to carry out primary research and development responsibilities of the Commission. I again brought up the possible delay in the Redox program, pending unification of research in such a central laboratory.

The GAC then went on to consider the second Commission memorandum, in which Wilson proposed a hard line with General Electric on its responsibilities at Hanford, in contrast to its interests in the new nuclear research laboratory at Schenectady, which is so dear to their heart. Wilson wants more effort than the company proposes on the Redox program, the production reactor replacement, the extension of existing reactor life, and much less work on power reactors. We agreed with this approach and then talked about Wilson's third memorandum, which covers Monsanto and their operation of the Clinton Laboratories. The GAC focussed on the concept of a central laboratory for power reactor development and agreed that a high flux reactor is the backbone of a long-range reactor program. The most attractive site for a central research laboratory, we believe, is the new site of the Argonne Laboratory in DuPage County near Chicago. At this point Oppenheimer suggested that Smith draft a memorandum about the Hanford problem and that Smith prepare a draft memorandum on the Commission's first memo. We adjourned at 12:30 p.m.

During this noon's intermission, which lasted to 3:40 p.m., I talked with Jim Fisk about Spofford English, who is being considered for an important position in Fisk's office. I recommended Spof highly.

Also during this intermission I sat for a photographic session at Harris and Ewing.

At the afternoon session, attended by all nine members of the GAC, plus Manley and Shivers, DuBridge read his draft of our comments on the first Commission memo, "Considerations of the Subcommittee on Research," which we adopted in principle. We also approved Smith's draft of our comments on the second memorandum. The GAC then took up the subject of the third memorandum; we emphasized again that the high flux reactor is to be regarded as the backbone of the long-range reactor program and that the group working on it is a key reactor group. The Daniels' high temperature pile, we agreed, is not suited to produce power rapidly and, although the discontinuance of this effort would not be serious, it would be desirable to continue work on fuel rods and gas cooling. We also agreed that the most attractive proposal for the location of the central reactor laboratory would be the new Argonne site, which might also be the site for the Argonne fast reactor and the high flux reactor (moved from Clinton). Fermi particularly was concerned about the possible effect on the personnel of Clinton. We took no action on this, however; we did recommend that Monsanto be urged to work on chemical problems, particularly waste recovery. The meeting was then adjourned--at 5:30 p.m.

Sunday, March 30, 1947

In Washington. I again took a taxi to the AEC Building to attend a 10 a.m. GAC meeting with all the members except Rowe. Shivers was present as a security officer. Oppenheimer brought up the material flow and appointed a new subcommittee--Fermi (chairman), Worthington, and me to gather information so that the GAC can make recommendations on this problem. We are directed to report as early as possible on an investigation of Pu^{240} tolerances in weapons as they affect Hanford operating procedure and on the question of increasing the reactivity of spent uranium fuel recovered from Hanford operations by adding enriched U^{235} from the K-25 plant to it or recycling some of the spent Hanford uranium fuel through a specially designed diffusion plant. We adjourned at 12:05 p.m.

The afternoon session began at 1:10 p.m. with the same group. We continued our discussions on the various matters brought up in this third meeting of the GAC. Bacher, Waymack, Wilson, Fisk, and McCormack joined us at 2:30 p.m. Oppenheimer, in his usual masterful way, summarized our conclusions on the Commission's memoranda 1, 2, and 3, said that the Subcommittee on Weapons will report back to the GAC on memorandum 4 after the group visits Los Alamos, and told of the creation of a new subcommittee to examine the efficient utilization of raw materials. Oppenheimer also said that the GAC believes a theoretical physicist is required on the matter of the "super." Wilson asked if it would be possible to proceed with pile construction on the assumption that the Redox Process would be successful--I replied that I had no doubt as to the success but only as to the possibility of meeting the time schedule for construction and operation; however, I said that I do not feel parallel work by other companies will cut the schedule because of competition for the same personnel. The meeting adjourned at 3:50 p.m.

Although I had reservations to return to Berkeley, via Chicago, by train, I decided to fly home in order to get there several days earlier. I took a taxi to the Washington National Airport, where I caught American Flight 407 at 10:20 p.m. for Los Angeles.

[In Berkeley, Helen, Jeanette, and Pete visited with Stan and Alice Thompson--Alice is expecting a baby momentarily.]

Monday, March 31, 1947

I arrived in Los Angeles about 8 a.m. I telephoned my parents and then caught Western Flight 312 at about 10 a.m. for Oakland. This arrived in Oakland about noon; a Radiation Laboratory driver met me and drove me to the Rad Lab, where I looked over my correspondence. I noted a copy of a letter from Darrell Osborne to Ralph James, asking James to check the beam energy for an experiment described in the paper "Products of Helium-Ion and Deuteron Bombardment of U^{235} and U^{238}" for Volume 14B.

I read a letter, written in French, that arrived from Professor J. Timmermans of the Institut International de Chimie Solvay. Timmermans thanked me for the information I sent him in February, particularly the suggestion of the name of Melvin Calvin as a speaker on photosynthesis at the 7th International Council of Chemistry. He asked me to convince Calvin to accept.

I saw that the note, "Nuclear Properties of U^{233}: A New Fissionable Isotope of Uranium," by Seaborg, Gofman, and Stoughton--although brutally censored for declassification--appeared in the March 15th issue of The Physical Review [Phys. Rev. 71, 378 (1947)]. This is based on our paper that was mailed to the Uranium Committee in Washington, D.C., on April 13, 1942.

Another letter that I read was a March 19 note from Vance Cooper, saying that he and his wife Mary will arrive in Berkeley the evening of March 24 and leave the morning of March 26 on their way to Richland. [Iz had informed Vance that I would be out of town on those dates.]

I looked over a report from H. C. Ott and S. G. English addressed to R. W. Stoughton: "Expected Formation of Heavy Isotopes in the Proposed Clinton Heterogeneous Pile."

At home Helen told me I had missed two significant events while I was away--Peter had cut two teeth! Also, at home I found a nice note from A. N. Stevens, who arranged for my talk to the Indiana Section, thanking me for stopping in Indianapolis to speak.

Tuesday, April 1, 1947

In attendance at this morning's group meeting were Blaedel, Conway, Covey, Cunningham, Eyring, Garden, Hopkins, Howland, James, La Chapelle, Lindner, Magnusson, McCullough, Miller, Morgan, Newton, O'Connor, Orlemann, Perlman, Robinson, Seaborg, Stewart, Templeton, and S. Thompson. First to speak was Cunningham who talked about the separation of Am-Cm by ion exchange adsorption. Under conditions described at a previous meeting, they purified curium with respect to americium by a factor of 10; after an eight-hour run, 30% of the original curium was obtained in this condition. Likewise, about 30% of the original americium was purified with respect to curium by a factor of 10. They are attempting to fix the adsorption characteristics of americium and curium with respect to neodymium with a Nalcite resin column in a steam-jacketed column. Cunningham said that Asprey is working on the separation of 5 mg of americium from lanthanum for metal production, using the column. Morgan described a rather poor attempt to separate americium, curium, and element 61, similarly in a Nalcite column.

Morgan and James discussed the use of fluosilicate as an elutriant in the decontamination of americium and curium from rare earth fission products. At this point I mentioned the enormous single crystals of AgCl, LiCl, NaCl, etc., being produced by Harshaw Chemical, where I visited in March.

Templeton reported on his bombardment of tantalum with 370 Mev helium ions. He found a 70-minute positron activity, probably a heavy rare earth, which suggests the knocking out of many neutrons rather than fission. Templeton said there is evidence for fission but the yields are low. Perlman acknowledged that we seem to have reached the limit of the preliminary studies on fission.

I told the group about my recent conversation with Fermi, who believes that mesotrons may be artificially formed only in positive-negative pairs, the minimum energy for deuterons is 200 Mev, which becomes 400 Mev if individual nucleons react and 800 Mev if momentum is conserved in interactions involving a single nucleus.

Blaedel said that McCullough's x-ray density of 24.7 is an indication that it was a coating of surface oxide that gave the pattern. Westrum, he added, plans to work on the melting point of protactinium when he gets here. Eyring gave the results of his three calibration experiments to check the heat of solution of KCl: 4.15, 4.16, and 4.18 Kcal/mole.

James mentioned his proposed bombardment of beryllium-plated uranium with 400 Mev helium ions in order to look for the effect of projecting high energy secondary beryllium particles into uranium. He is not optimistic about the results. Finally Hopkins reported that he found a 7-hour β^+ and a 9-day activity in the selenium fraction of a bombardment of arsenic with 200 Mev deuterons. There was a 3-hour β^- or e^- activity in the arsenic fraction. The gallium fraction milked from the germanium faction showed the one-hour β^+ of Ga^{68}. He reported in this bombardment the yield vs mass was a steadily decreasing curve contrary to the observations reported on a previous bombardment.

* * * * *

At the 10 a.m. meeting of Chemistry 123 the midterms were returned and explained. The subject of today's lecture was induced nuclear reactions and included a discussion of the Bohr concept of the nucleus and potential barriers. Afterwards I stopped on the third floor of Gilman Hall and saw Geoffrey Wilkinson. I told him that we shall try to expedite his clearance so that he can work on the hill and use the 184-inch cyclotron. I also stopped in the Department of Chemistry office before going up to the hill.

In the afternoon I talked with Cunningham, Blaedel, and McCullough about their research programs. McCullough and I again discussed the problem of his replacement when he returns to UCLA. McCullough is willing to train one of our young Ph.D.'s in the use of his equipment; however, this is an idea that must obtain the approval of Dean Latimer.

At 4:15 p.m. there was a meeting of the joint Seaborg-Connick groups; after dinner in the Faculty Club, I attended the Research Conference.

Wednesday, April 2, 1947

After a few conversations and phone calls, I answered a couple of letters, including a response to Dr. E. M. Billings (Rochester, New York) to say that I shall attend the meeting of the Committee on Professional Training (ACS) in Atlantic City on April 14th at 9 a.m., but I have another meeting at 11 a.m. and will leave at that time.

I submitted to D. E. Chambers (General Electric, Schenectady) the traveling and consulting time spent in connection with my consulting arrangement with General Electric for November, February, and March. I explained that the actual traveling expenses were taken care of in connection with the Radiation Laboratory contract.

For some time our clerk Lorraine Petch has been typing the "Table of Isotopes," a formidable job. However, the end is now in sight, and our local Information Division wants to issue the classified report from Berkeley rather than send it to Chicago for issuance as the "Project Handbook." This should save considerable time in getting it into the hands of the scientists.

Later Ernest Lawrence, Don Cooksey, and I took Kai Siegbahn (a Swedish physicist and the son of Manne Siegbahn, the 1924 Nobel Laureate) and some other visitors to San Francisco for dinner.

Thursday, April 3, 1947

In my Gilman Hall office I worked on the Chemistry 123 lecture notes; at 10:10 a.m. I gave the lecture and assigned a new problem set. Today's topic was cross sections.

After class, as usual, I stopped in the Department of Chemistry office to talk with Miss Kittredge and various colleagues.

A note arrived in today's mail from Ed Westrum, announcing his arrival early tomorrow morning and requesting that a pass, a document pass, a submaster key to Building 4, and a key to the counting room await

him at the personnel office early Friday morning.

Friday, April 4, 1947

After some administrative duties and conversations with the men, I replied to a March 25 note from Norman Bonner (Washington University). Bonner is planning work on copper isotopes by bombarding nickel with deuterons and asked if this work has already been done in Berkeley. I informed him that a group of physicists here are engaged in a program of bombarding separated nickel isotopes with deuterons and other particles.

In today's mail was a thank-you note from Caleb Paine for my talk on the "Science Forum" broadcast on March 26.

An invitation arrived for me from Donald A. McPherson (John Wiley & Sons) to a cocktail party at the Hotel Claridge on April 16 from 4 to 7 p.m. during the ACS meetings in Atlantic City.

Helen and I left Peter with Jeanette and walked down to the Albany Theater to see the movie, "The Time, The Place, and The Girl," starring Dennis Morgan and Jack Carson.

Saturday, April 5, 1947

On March 25 Neil Gordon wrote that he is very anxious to reproduce my Wayne University talk in the next issue of Record of Chemical Progress; he asked for some illustrations. In my reply this morning I sent him some photostats and drawings and suggested that he obtain permission from Life magazine if he uses the picture of the first plutonium isolated. Additionally, I made a number of changes in the copy of the text he mailed me.

I answered a March 31 letter from Welden Reynolds (Encyclopaedia Britannica) and said that I believe it will be possible to have my article entitled "Artificial Radioactivity" in his hands by May 15.

The Director of Science of CBS, John E. Pfeiffer, recently asked me about the feasibility of a member of their documentary staff visiting the Radiation Laboratory in order to gather material for a one-hour documentary on the beneficial applications of atomic energy. Yesterday I received a letter from Ruth A. Ashton of the CBS Documentary Unit, saying that she will arrive in Berkeley on April 8 and will spend Tuesday and Wednesday at the University of California. Miss Ashton said that she wants to see laboratories in which such work is going on. Today I wrote to Pfeiffer and said that we shall take care of Miss Ashton when she arrives. I also suggested that tentatively we have lunch together on Friday, April 11, when I shall be in New York.

The following is J. Robert Oppenheimer's summary of the third meeting (March 28-March 30) of the General Advisory Committee, which he prepared for David Lilienthal:

LOS ALAMOS SCIENTIFIC LABORATORY
P. O. Box 1663
SANTA FE, NEW MEXICO

SECRET

April 3, 1947

Mr. David Lilienthal
United States Atomic Energy Commission
1901 Constitution Avenue
Public Health Building
Washington, D. C.

Dear Mr. Lilienthal:

This letter will give a summary of the suggestions and recommendations which have emerged from the third meeting of the General Advisory Committee and the meetings of its subcommittees. On some aspects of these recommendations the annexes to this letter will give more detailed discussion. The General Advisory Committee is transmitting with this letter two documents: (1) a report of the Subcommittee on Research, which elaborates the views of the Advisory Committee on the functions of a central development laboratory; (2) the report of the Subcommittee on Reactors, summarizing our recommendations with regard to reactor development as implied by the specific recommendations discussed in this letter. I may call your attention to the verbatim transcription of a meeting held on the afternoon of March 30, 1947, at which the Advisory Committee reported orally to members of the Commission and the Commission staff on many matters covered in this letter, and also to the comments of Dr. Seaborg discussing in a general way the chemical problems associated with reactor development and material conservation.

On March 27 the Subcommittees on Reactors, on Weapons, and on Research met in the Atomic Energy Commission offices in Washington. The General Advisory Committee met in two daily sessions on March 28, March 29, and March 30. Four members of the Subcommittee on Weapons travelled to Los Alamos and on April 2nd and 3rd conferred with the Director of Los Alamos and members of his staff. Not all matters considered during this meeting have been evaluated by the General Advisory Committee, but the General Advisory Committee authorized the Subcommittee to report in its name on certain questions which are covered in this letter.

During the meetings of the General Advisory Committee a new subcommittee was created with Dr. Fermi as chairman, Dr. Seaborg, Mr. Worthington, and Dr. Oppenheimer ex officio as members. The purpose of this subcommittee, which we may call the Subcommittee on Materials Balance, was to examine the implications of the increased production program recommended by us for the conservation and effective use of materials. In particular, this subcommittee was directed to report as early as possible on the desirability of increasing the reactivity of uranium recovered from Hanford reactors with K-25 product, or the alternative desirability of enriching by gas diffusion the reject Hanford uranium. It was further directed to investigate the problem of Pu^{240} tolerances, and the effect of these specifications on chemical and other operating problems of the plutonium production program. The [redacted] committee in the long term is to see that raw materials are

effectively used and that existing facilities are put to the greatest advantage.

The General Advisory Committee was given by the Commission staff four memoranda. The first three memoranda, after general discussion, make specific proposals of policy on which the advice of the General Advisory Committee was requested. The fourth memorandum discussed the desirability of a new directive for the Los Alamos Laboratory and raised specific questions as to the content of that directive. I shall now take up our views with regard to these memoranda, and questions related to them, in order.

I. The first memorandum attempts to distinguish between activities of the A. E. C. for which it is charged with complete responsibility and control, activities in which facilities unique to the A. E. C. put on the Commission a direct responsibility for making these facilities available, and general activities of the A. E. C. in support of nuclear science and the science and technology of closely related fields. The statement of policy on which our advice was requested follows:

> "1. That for effective concentration on urgent problems and for security the activities under Item 1. be conducted as completely as possible within Atomic Energy Commission facilities, essentially disentangled from non-programmatic, fundamental research.
>
> 1-a. That within Atomic Energy Commission facilities, work on fundamental problems arising from a special knowledge of its central problems be not discouraged.
>
> 2. That in activities supported by the Atomic Energy Commission, but not included in Item 1., every effort be made to encourage the training of scientists and engineers, i.e., these activities should serve as a source, not a sink, of trained people.
>
> 3. That any direct support of the general science research and training of Item 2. be considered a relatively short term undertaking, to be reviewed periodically and surrendered if an appropriate general agency is created or other means for its support develops."

The General Advisory Committee unanimously concurred with this statement of policy, but wished to add one reservation and a series of further observations. The reservation is the following: Controlled reactors for studies in physics, chemistry, biology, medicine, and engineering, and the provision of radioactive and stable tracer isotopes for studies in these fields, are largely a monopoly of the Atomic Energy Commission. For these reasons paragraph 3 of the enclosed statement of policy does not and should not refer to them.

In studying this problem of the diverse responsibilities of the A. E. C., we were led to the view that rather different organizational patterns would be appropriate for the different categories of research and development. For categories 2 and 3, as outlined above, we would advocate the continuance of a pattern of dispersal. We would recommend that reactors and isotopes be made as widely available, and as available to varied academic and industrial groups, as lies within the Commission's

power. In these fields most work is not held secret. Normal channels of scientific and technical communication are adequate to insure that progress at one institution will be reflected in the work at others. In these fields, which are so largely non-classified, wide dispersal may help in the program of training of scientists and engineers, on which the continued success of the Atomic Energy Commission will depend.

In the field of the programmatic research and development essential for the conduct of the Commission's prime responsibilities the situation is quite different. These developments, which have to do with discovering, mining, and refining raw materials, with developing and constructing reactors for the production of materials, and, later on, for power, with separation of fissionable materials, with the design and construction of weapons, and with the overall problems of the chemistry and metallurgy of materials essential to these programs are for the most part highly secret developments. They are also developments intimately bearing on each other, as the present difficulties with the expansion of plutonium production will illustrate. We believe the progress on these problems would in the long term be accelerated if they were largely disentangled from the less secret and more dispersed research of categories 1 and 2, and if these activities themselves were far more concentrated in a smaller number of sites than is the case at present. Thus we might look to the establishment of a central development laboratory devoted to the physical, chemical, metallurgical, and engineering problems of a reactor development. We believe that a development in the direction of unification would have several advantages. It would make possible the formulation of technical policy on the working level, where alone it can inform the actual execution of the work, and where alone it can be responsive to varying technical developments. We believe that such an increasing unification may go far toward solving the personnel problems of the A. E. C. We think it easier, for instance, to find one qualified director than six. We, therefore wish to couple our approval of the proposal quoted above with the suggestion that the A. E. C. explore the possibility of concentrating its programmatic research and development.

An examination of some steps leading in this direction will be discussed below. A longer discussion of the merits of this direction will be found in the appended report of the Subcommittee on Research. Here it may suffice to make the following comments:

(a) We recognize that some problems, such as Redox development, are of too great an urgency to await the establishment of a central laboratory.

(b) We recognize that not only the feasibility, but the desirability of establishing a central laboratory must depend on the favorable response of personnel now working on these problems for the A. E. C.

(c) We would suggest as a probably suitable site the new tract of land acquired for the Argonne Laboratory. Should this proposal be adopted, we should suggest that the facilities now in operation at the Argonne Laboratory, namely, the graphite and heavy water reactors, be rated as class two facilities and not part of the central laboratory. A second suggestion as to location is that the central laboratory be near Washington.

(d) We would suggest that this central laboratory be operated, not under contract, and not under civil service, but directly by the Commission with a suitable personnel policy.

(e) One member of the General Advisory Committee, Dr. Fermi, did not wish to take action on a recommendation of a central laboratory, pending further exploration of the response which such a suggestion would meet on the part of present A. E. C. contractor's personnel.

(f) The long range possibility of including weapons development in the same laboratory with other programmatic activities of the Atomic Energy Commission should not be considered at this time, but should not be permanently lost sight of.

II. The second memorandum presented by the A. E. C. has to do with the program of the General Electric Company. The proposal reads:

> "That the General Electric Company be made more fully aware of the urgency and overall importance of the Redox, waste recovery, pile replacement, and pile life problems of the Hanford Engineer Works and that they be urged to devote a major share of their energies to these problems; that their work on the technology of a power reactor be maintained, secondary to Hanford problems; that their program in 'long-range fundamental research', for which Atomic Energy Commission support is expected, be considerably reduced.

> "The proposal to discourage long-range research here is based on the following considerations: (1) The G.E. undertaking cannot be considered as a 'source' but only as a 'sink' for trained people; (2) The Atomic Energy Commission should solicit the participation of strong industrial concerns primarily in the engineering and industrial aspects of nuclear energy problems; (3) It is probably not wise to mix intimately work which must now be classified with work which need not be; (4) Much of the long-range research activity suggested is being done or will be done elsewhere."

This proposal is unanimously approved by the General Advisory Committee. Here again we have a few comments:

(a) We attach great importance to making it possible for the senior officers of the General Electric Company to appreciate the importance of Redox in the Commission's program.

(b) We believe that something of the scale of future plutonium production, which would according to our recommendations involve an increase of a factor 5 over present production, should be discussed with the officers of the Company, so that they may plan construction activities and the development of Redox in an informed way.

(c) We believe that if the General Electric Company understands the importance of the chemical developments, if it has the assistance and backing of the Commission and the help of other A. E. C. personnel, such as the group now working in the Argonne Laboratory, on these problems, the time scale for the first Redox installation may be shortened by as much as six months or a year.

(d) We would attach to the four items mentioned in the proposed General Electric work at Hanford rather different priorities. We believe that Redox is the most urgent, that the construction of further reactors is of only slightly lower priority, and that these two activities should be scheduled for approximately simultaneous completion. We believe that improvements in reactor design, although important, are of lower urgency. We believe that the problem of waste recovery is of still less urgency.

(e) We would recommend that, if possible, the problem of waste recovery be turned over to another industry, possibly Monsanto, in order to relieve the General Electric Company of too heavy a burden. This would fail in its purpose if there were competition between the two companies for personnel.

(f) We support the recommended proposal that the General Electric Company pursue at Schenectady their work on the technology of a power reactor, but here again

we would emphasize that this work is of importance very much secondary to the Hanford program. We believe that special steps may have to be taken to avoid diverting personnel potentially useful to Redox development for the related chemical problems at Schenectady.

III. The third memorandum from the Atomic Energy Commission deals with the nature of the activities of the Clinton Laboratories. We quote the proposal:

> "That Clinton Laboratories concentrate on its present major research facility, the graphite pile, as a research tool in biology, chemistry, physics and metallurgy with the active participation of the proposed 'Oak Ridge Institute of Nuclear Studies'; that the strong supporting groups in pile science and technology be encouraged to take part in the programs at other Atomic Energy Commission Laboratories; that studies for the High-Flux research reactor be continued but that decision on its time and place of construction be reserved; that the Power Pile development be preceded by considerable study of properties of pile materials under high neutron flux; that the contractor, Monsanto Chemical Company, be urged to work actively in its own laboratories with Atomic Energy Commission support, on chemical and chemical engineering problems which are vital to the entire pile program.
>
> "We understand that a solution along these lines would be agreeable to a large fraction of the Clinton Laboratory staff."

With one dissent the General Advisory Committee concurred in this proposal. The General Advisory Committee was unanimous in believing that if this proposal were carried through certain other steps and commitments should accompany it. The one dissent to the proposal, that of Dr. Fermi, is related to his dissent from the recommendation of a central development laboratory, and reflects his doubt that the discontinuance of reactor development at Clinton would in fact meet with the support and understanding of the personnel now engaged in that activity. Should his doubt prove unfounded, he, too, would support the proposal.

As to the positive recommendations which we believe should accompany the discontinuance of active pile development and construction at Clinton, we are much impressed with the necessity for developing and building a high-flux reactor along the lines now being considered at Clinton. Our interest in this development does not lie in its possible use as a tool for physics and chemistry; it lies in the fact that such a reactor will make possible the effective differential study and design of the components of future reactors. We believe that this will be necessary to progress in the field, that it will turn out that it will be impractical to build reactors to test all varieties and types of fuel rods, coolants, moderators, etc., etc. This will be particularly true of the breeders of the future, where the requirements of high breeding rate at high neutron energy will involve enormous investments of material. We therefore strongly recommend the development and construction of a high-flux reactor, as an essential facility of the central development laboratory. We must make a few comments to this recommendation. From the information available to us, it appears that much work needs to be done on the chemical recovery of fuel elements before such a reactor can be built. Further we believe that it would be unwise to authorize work on this reactor without explaining to the personnel involved that its principal function would be as a facility for further reactor design. We believe that there may be some

difference of evaluation on this point. We are thus recommending that the very large group of scientists and engineers now at Clinton Laboratories and interested in the development of a high-flux reactor be assured of the interest of the Atomic Energy Commission in this development, be encouraged to continue their work in this field, at Clinton until the new facilities can be provided, and in the central laboratory as soon as the transfer can be made. We see no difficulty in having both the Argonne reactor and the high-flux reactor developed in the same laboratory.

We agree with the Atomic Energy Commission proposal that work on the construction of the power reactor at Clinton be discontinued, but make this recommendation without prejudice to the further study of the materials and components which may be involved in such reactors in the future.

We concur in the A. E. C. proposal not to supplement Clinton facilities with a large scale development of accelerators and allied equipment. One suggestion as to work of prime interest to the Commission which might be undertaken by the Monsanto Company near its own installations is the problem of Hanford waste recovery.

The report of the Subcommittee on Reactors summarizes the implications of our recommendations in this and the preceding sections with regard to reactor program.

IV. We have discussed the Atomic Energy Commission memorandum No. 4 dealing with the proposed directive for Los Alamos. In the course of our studies we have reached certain conclusions both as to the general directive and as to the specific directive as to types and numbers of weapons. These I now wish to discuss briefly.

With regard to the seven suggested subjects in the general directive, they read as follows:

"1. restate the central function of the Los Alamos Laboratory

2. recognize that the work at Los Alamos has a priority second to none

3. suggest that an Associate Director be appointed to conduct Z Division affairs at Sandia and Los Alamos, and remove a heavy load from the Director

4. emphasize initiator developments with respect to new methods and materials and to the problem of understanding initiator performance and need

5. emphasize development efforts toward simplicity of design of present weapons for reliability and flexibility

6. de-emphasize production at Los Alamos which may be done elsewhere in favor of development studies

7. define tentatively a test program."

We concur in recommendations 1, 2, 3, and 4. We believe that item 6 is not suitable for inclusion in a directive, since the essential step is to

establish the necessary facilities elsewhere. We have made some progress with regard to the definition of a test program as suggested in 7.

Our most important conclusions in discussions with General Brereton and Admiral Parsons in the Subcommittee on Weapons, and discussions in the General Advisory Committee itself, and in discussions with the Director and his staff at Los Alamos, have to do with the type of weapons appropriate for stockpiling in the near future, before the development of markedly increased plutonium production. We would suggest that two types of weapons be included in this stockpile: both implosion weapons, one with composite core of U^{235} and plutonium in a DELETED and one of U^{235} alone. We should recommend the rapid development of both these weapons to the point where a test can be made of their performance in the early months of 1948. We believe that it would be wise to plan this test so that in the event of unsatisfactory behavior of either or both of these weapons another bomb could be exploded, which could establish whether the failure came from unsatisfactory core materials or from DELETED From the estimates of energy yields of these weapons, they would provide the most effective use of present production of fissionable materials which is in sight for the near future. Should the tests confirm expectation, we would recommend the discontinuance of stockpiling of all other types of weapons. These recommendations must be accompanied by others if they are to be effective. One concerns the rate of production of explosive charges, which should reach the level of 10 complete castings a month by early 1948 if possible. The other has to do with the directive to the Monsanto Company with regard to the production of initiators, which should reach a figure of at least 30 a month in early 1948. Unless new evidence supervenes, these initiators should have an initial charge of 60 curies of polonium. As an interim measure, we recommend that the directive to the Los Alamos Laboratory should request the production of at least 15 initiators a month starting in May, 1947. We understand that this is approximately the limit of production with present facilities. This production may be drastically reduced when Monsanto is able to take over.

It will be seen from the above recommendations that we have considered and rejected certain alternatives. Thus, we have been willing to DELETED realizing that reduction in this figure would probably entail too great a loss in yield, that an increase would entail too great a hazard. We have further concluded that on the basis of evidence presently available, both as to the need for, and the possibility of development of, a penetrating weapon, effort in this direction appears unjustified. We wish to make it clear that these judgements require study as to military application of atomic weapons; but unless such study reveals factors now unknown and not anticipated, we believe that the program as here recommended will make the best use of the available material.

Finally, in our discussions in the General Advisory Committee and at Los Alamos, we have come to appreciate that rapid progress in the development of thermonuclear explosives is not to be anticipated; nevertheless, we are convinced that at least for the present studies bearing on this problem are contributing to the health of the Los Alamos Project, and laying the foundation for further progress in the future. For these reasons we would like to see included in the Los Alamos directive a statement along the following lines:

"Research directed towards the utilization of thermonuclear reactions in atomic explosions. Research of this character, while necessarily of a long range nature, is considered of very great interest by the Commission. Periodic status reports at least every six months on the two developments now under consideration are requested, specifically the 'Super' and the 'Alarm Clock'. Each report should include not only a description of the present status of the problem but an estimate of the necessary experimental program, both in time and character required to bring such a development to fruition. There should finally be included brief reports of any experimental programs undertaken whose primary objective concerns thermonuclear weapons."

There are other matters involving the weapons program which the Subcommittee on Weapons will need to discuss with the members of the Advisory Committee before it can properly advise the Atomic Energy Commission.

J R Oppenheimer
for the General Advisory Committee

Los Alamos, New Mexico
April 4, 1947

I also dictated a letter of recommendation to Professor William G. Young for John R. Thomas, who is applying for an instructorship at UCLA. I said that Dr. Francis E. Blacet probably knows more about Thomas's abilities than I, but I have known him since he was in one of my sections of freshman chemistry here (before the war). I wrote that I have followed his work on the Plutonium Project in Berkeley, both by my trips from Chicago to Berkeley and by his reports, and presently by first-hand

observation. He and Howard Crandall have carried through an important development admirably. I concluded my letter with "It seems to me that Thomas might be a good man for your Department to put on and look over in the capacity of an instructor."

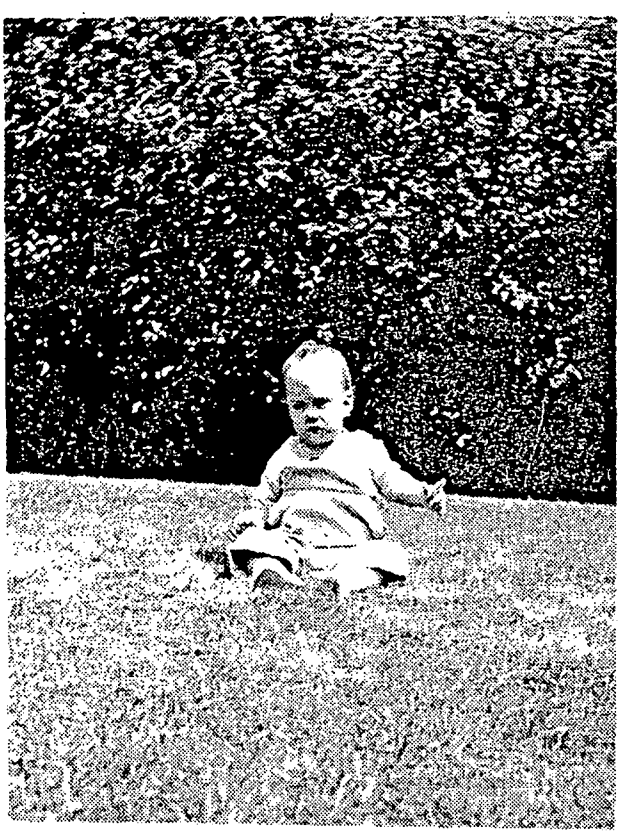
Another letter went to Frank A. Long (Cornell University), explaining that Dr. John G. Kirkwood told me that he believed that Long is still interested in locating a nuclear chemist for Cornell. I suggested the names of David H. Templeton, Leon O. Morgan, and Paul O'Connor as men who are finishing their Ph.D. work and might be possible candidates.

Sunday, April 6, 1947 (Easter)

I worked on my various writing projects and then had a midday Easter dinner with Helen, Jeanette, and Peter. My parents sent us a card and a box of See's chocolates to help celebrate the day.

In the afternoon we tried taking some pictures of Pete on the lawn; however, he did not smile as we tried to get him to do--apparently he doesn't like the feel of the grass. Later we took a ride in Jeanette's car.

Peter and Glenn Seaborg,
836 Washington St., Albany,
April 6, 1947



Peter Seaborg

Monday, April 7, 1947

There was a huge stack of correspondence to answer today, and I kept Mary Bender quite busy with dictation.

I answered a March 12 letter from Beryl Gilman (Technical America) and said that it is reasonably safe to assume that my article will arrive during August although it probably will not arrive before August since it may have to be cleared.

On April 1 Kenneth T. Bainbridge (Harvard) requested unclassified information of relative abundances of isotopes below bismuth (which he needs to fill the commitment he made at the March 22 Chicago NRC committee meeting). I answered by sending him a copy of the revised "Table of Isotopes." In my reply I also explained that the abundances listed are those we considered the best, not averages. I then asked Bainbridge to send us any information on better sets of abundances than we used.

In a note to Francis W. Test (Chicago Patent Group) I suggested that I talk with him when I visit Chicago (April 18-22) about the broad claims on plutonium bromide, about which he questioned W. E. Elliott of our patent office on March 14.

To W. R. Kanne (Research Laboratory, General Electric, Schenectady), I mailed the drawings of our irradiation container, which I promised him when I was recently in Schenectady. I also included a copy of a report by Don Stewart that may be useful to him.

I answered a March 21 letter from M. W. Welch, who asked about an article in C & E News (February 24th, page 550), headed "Committee to Issue New Isotope Chart." Welch is still interested in publishing my chart but wonders about this article. In my reply I said that I am not clear about the article although I do know something of the background; I promised to get in touch with him in April when I am in Chicago to discuss the question.

On March 25, while I was in the East, Art Jaffey wired asking about declassification of alpha energies of a number of nuclei. Perlman replied that Los Alamos had the energy (determined calorimetrically) of the Pu^{239} alpha particles declassified. Iz then sent Jaffey a chart of declassified information in the heavy region. Today I wrote to Jaffey, confirming Iz's information and saying that report A-136, "Production and Properties of the 50-year Element 94," has now been declassified and gives the range of Pu^{238} alpha particles as 4.1 ± 0.1 cm. I said I have no information about U^{235} although something may have escaped me.

My address delivered before the Conference on the Future of Nuclear Science, Princeton University Bicentennial, September 24, 1946, entitled "Artificial Radioactive Tracers: Applications to Chemistry and Medicine" appeared in the current issue (April 4th) of Science [(Science 105, 349 (1947)].

Another note went to Herbert A. Young on the Davis campus, who has an ongoing research project there under our auspices. Young wrote on March

19 that he will be going on sabbatical and asked if Webb (A. Dinsmoor) should continue the work while he is on leave; I wrote that I agree that Webb should do so.

I answered the March 6 letter from John Willard, who requested the names of Ph.D.'s suitable for his department. I suggested the names of David H. Templeton, Leon O. Morgan, and Paul R. O'Connor as men who will probably finish this spring or summer. Paul O'Connor, I said, who is probably known by men in the department there, is developing very nicely, and I am proud of him. I then asked if Edwin M. Larson is still at the University of Wisconsin for we are interested in contacting him again about zirconium-free hafnium.

In today's mail was a draft from Truman Kohman of the report of the March 22 meeting of the Subcommittee on Nuclear Constants. Truman asked for my approval or corrections. He also sent me a copy of "The Specific Activity of Radium" by Kohman, Ames, and Sedlet; again he asked for comments. Truman then asked for my preference on the names and credit assignments for the Chart of Nuclides. I immediately answered, saying that the draft of the minutes of the March 22 meeting is satisfactory and that the only comment I have to suggest on the radium paper is to use a different method to designate samples rather than α and β because of possible confusion with alpha and beta radioactivity. With regard to the key to the chart I suggested that he consider removing the reactions $\gamma\mu^-$ and $\gamma\mu^+$ since it is now considered that these reactions involve two steps (and not mesotrons). I added that Fermi believes the artificial creation of mesotrons occurs only in positive-negative pairs. I then discussed the name of the chart and said that Perlman and I shall continue to use "Table of Isotopes" on our table. I suggested that he allow Sullivan to cast the deciding vote about the name or to use "Chart of Isotopes (Nuclides)." I said, however, that I shall not get mad nor have apoplexy if my dissension is overruled. Finally I offered my suggestion about the authorship and again said that my disagreement with his suggestion is not violent.

An April 1 letter arrived from Edward D. Crane (President of D. Van Nostrand Company, Inc.), who announced that Ralph K. Strong, Editor of Kingzett's Chemical Encyclopedia, is going to ask me to prepare the material on nuclear chemistry for the next edition; Crane hopes that I will have time to do it.

I also received a carbon of a letter from Darrell Osborne to Joe Hamilton. Osborne is interested in the bombarding energy of the particles in a number of old bombardments in order to quote the energies in papers of the Plutonium Project Record (PPR).

Mildred Hoogstraat of Summit, New Jersey, wrote in a letter dated April 2, "Some of my girl friends have Clark Gable or Errol Flynn for their pin-up pictures, but they don't mean anything to me. I am very interested in chemistry, and anybody who has discovered three new elements is much better than just a movie actor. Would you send me a real picture of you and could you sign it because all that I have now is the cover from a magazine, and I would like a real one. Maybe you could find another new element and make it four. All your fans would like that."

When I got home today, Helen mentioned that she had written my parents to thank them for the Easter chocolates, to tell them about Peter's progress, and to suggest they buy a copy of the April 7th issue of Newsweek, explaining that there is an article about me on page 53 (the article on U^{233}).

Tuesday, April 8, 1947

This morning's meeting was attended by Asprey, Blaedel, Cunningham, Eyring, Fries, Garden, Ghiorso, Goeckermann, Hopkins, Howland, Huffman, James, La Chapelle, Lilly, Lindner, Magnusson, Moore, Morgan, Newton, O'Connor, Orlemann, Perlman, Reynolds, Seaborg, Templeton, Thompson, Helene Voyer (a Chemist P-1 working for us in ORL), Werner, and Wheelock. Lindner announced that he will study the distribution of radioactive products from a bombardment of 200 Mev deuterons on antimony in order to see how far down the mass and atomic number scale they extend. He has already found activities in tellurium, antimony, tin, indium, cadmium, and silver and will now investigate palladium, rhodium, and ruthenium. He described the chemistry he will use.

Newton talked about his work with a thorium target, bombarded for about eight months with 44 Mev helium ions. Thin layers from the active area were milled and the total activity and the barium activity were measured. The break in both total and barium activity occurs at about 23 Mev. Other elements will be separated, and these should give a fairly complete picture of the fission product distribution.

Cunningham discussed the use of Nalcite columns to separate americium, curium, and neodymium at 100°C; he mentioned that bubbles are formed in the resin at this temperature unless extreme care is used. The present run had to be stopped for this reason but, since about 40% of the total alpha activity was eluted before 1% of the beta activity appeared, it can be concluded that curium was eluted before neodymium. Asprey, in describing the purification of 5 mg of americium containing 10% lanthanum plus lanthanum tracer, said that essentially all of the alpha activity was recovered in the second 50 ml of citrate solution, pH = 3.04.

Hopkins continued his discussion of the 9-day selenium he described last Tuesday and said, after two half-lives, the arsenic was separated from the selenium and found to decay with a 26-hour half-life; the positrons were due to the arsenic daughter. The selenium appears to decay by K capture, but the radiations were not observed. Hopkins then described a method for the separation of arsenic and germanium. He also described some experiments he has made in determining rough beta ray energies using the magnetic counter.

In other work James reported that a lanthanum fluosilicate column did not work due to mechanical difficulties--the lanthanum fluosilicate is too finely divided and packs in the column. Morgan said that equilibration experiments with fluosilicate make it appear that a column will be useful if one can be made mechanically. Goeckermann will repeat experiments with 200 Mev deuterons on lead to search for fission and will also investigate the fission of thallium with 200 Mev deuterons. Blaedel said that while Westrum is here, they will prepare equipment to go to 2000°C to find the melting point of protactinium and make an attempt to

form the protactinium hydride. O'Connor described the distribution of fission products from uranium + 400 Mev helium ions. The results seems to indicate there is a difference from normal neutron fission (where the dip between peaks is deeper) and also from fission of lead and bismuth (where there is no dip).

Garden asked for the men to look at the model of the cave set up in ORL and give their suggestions and approval.

* * * * *

Today's topic for Chem 123 was a discussion of nuclear fission. I also collected the problem sets. Afterwards I went in and talked with Dean Latimer, who finally agreed to offer Dave Templeton, the man I suggested, an instructorship provided he train in the field of x-ray crystallography. I know that this plan is acceptable to both Templeton and McCullough, who will soon be returning to UCLA.

Back at the lab I wrote, at the request of Dean Latimer, to Henry M. Neumann in Minneapolis, who has been accepted for graduate work in Berkeley. I explained that the "research fellowship" described by Professor Latimer is fundamental research in the field of nuclear chemistry, and some of the program, I said, is under the auspices of the Atomic Energy Commission; therefore, at the present time, the participants must have a security clearance. I explained that I am having George Everson sent him the proper forms.

In a memorandum to George Everson, I summarized the reasons why we are requesting clearance for Dr. Geoffrey Wilkinson. I explained that he is an expert in fission product chemistry, and we intend to have him work on the transmutation products produced by irradiation with the 184-inch cyclotron. This work, I said, can be accomplished by allowing Dr. Wilkinson to have access only to the lower floor of Building 4. Our work on the transuranium elements, which is more restricted, is on the second floor.

I went to the 7:30 p.m. meeting of the Research Conference in Room 102, Gilman Hall.

At home Helen mentioned that Jo Owen had visited during the afternoon.

Wednesday, April 9, 1947

On March 27 Professor I. Naray-Szabo (Budapest) wrote a strong letter of recommendation for Dr. K. [Charles M.] Tobias, a chemical engineer and the brother of Cornelius Tobias of the Radiation Laboratory. K. Tobias would like a fellowship in order to become acquainted with the latest advancements in modern physical chemistry, particularly radiochemistry. I have discussed the matter with Professor Latimer and today wrote to Professor Naray-Szabo that we believe it will be possible to make a satisfactory arrangement for Tobias to join us.

While I was in Washington in March, I spoke with Jim Crowe (C & E News) about the then scheduled U^{233} article, that now has appeared in The Physical Review. I suggested that, if he does a story on the U^{233}, he

should make the article broad and include information on thorium. Today I received a letter from Jim, who questioned how Newsweek got a story on U^{233} into their April 7th issue if there was no news release. He is concerned that possibly Dan Wilkes put out a story while I was out of town and also said that they have not received any news releases at all from Wilkes. Crowe asked for references to articles on thorium. I immediately replied today and said that there was no news release from here and that Dan Wilkes was on Navy duty. I then explained that the Newsweek article had been written by Harry David, and I thought it was a better-than-usual write-up. I suggested a couple of United Nations documents for information on thorium, and I requested that he not quote me in the article about the use of U^{233}. From a personal point of view, I said, I would appreciate a minimum emphasis on atomic bombs.

I wrote to Miriam White of Newsweek, who had written me on March 31 about their story on U^{233}, saying that the article struck me as being quite well done. To her query about the naming of plutonium I said, "The naming of plutonium should, I believe, be attributed to the whole group of discoverers, G. T. Seaborg, E. M. McMillan, A. C. Wahl, and J. W. Kennedy. This element was discovered late in 1940 but no name was suggested at that time. McMillan at the earlier time of his discovery of neptunium with Abelson, had in mind that the system of naming after the planets should be applied to the following element whenever it was discovered; a couple of others in this plutonium group, a year or so after the discovery, when the group was separated, independently suggested the same name."

Perlman sent a request to Don Cooksey for authorization for a trip for Al Ghiorso to Washington and to Chicago. In Washington Al will attend a meeting of the National Research Council Committee on Radioactivity on April 28; he will then continue his fission measurements with neutrons at the Argonne heavy water pile in Chicago on April 29 and 30.

This evening I went to a retirement dinner in honor of Professor C. Walter Porter of the Department of Chemistry.

Thursday, April 10, 1947

Another problem set was assigned to the Chem 123 class this morning. The subject of today's lecture was accelerators.

Back in my office I answered the April 5 letter from Ralph K. Strong (Editor of Kingzett's Chemical Encyclopedia), who wrote to describe the article on nuclear chemistry that he wants me to prepare. In my response I questioned whether he wanted separate sections on radioactivity and nuclear chemistry, whether sections of the order of some five to ten pages would be proper, and whether a date such as July or August would be appropriate. I then suggested, because of my time limitation, that I have a qualified colleague write the article. I emphasized my sole interest is to present new information in nuclear fields accurately. I also commented on the need to revise the Table of Elements and the Periodic Chart to accommodate the new elements and the possible need for articles on the individual transuranium elements.

Recently (April 5), I suggested to John Pfeiffer (CBS) that we have lunch on April 11. Yesterday he wired me, "WILL BE OUT OF TOWN FRIDAY. CAN YOU MAKE IT MONDAY OR LATER IN THE WEEK." My response today was: "IN TOWN ONLY FRIDAY, HOWEVER SEE YOU ANOTHER TRIP."

In the afternoon I went to San Francisco and took a limousine to the airport, where I caught United Flight 4 at 6 p.m. for New York City.

Friday, April 11, 1947

In New York. My flight arrived at 9:35 a.m., and I took the limousine to the Hotel Commodore, where I checked in. After resting during the morning and part of the afternoon, I went by taxi to Penn Station and boarded a train to North Philadelphia. Here I changed to a train bound for Atlantic City. In Atlantic City I took a cab to the Hotel Traymore, where I shall stay during the ACS meetings.

[In Berkeley, Helen went shopping.

The papers today reported a $500,000 fire on Treasure Island--six buildings were gutted, including the mess hall; 2,000 firefighters fought the blaze.]

Saturday, April 12, 1947

In Atlantic City. I went to the American Room in the Hotel Traymore at 10:30 a.m. to attend my first ACS Council Meeting in my new position as Councilor-at-Large.

Sunday, April 13, 1947

In Atlantic City. Registration for the ACS meeting took place from 2 p.m. until 8 p.m. on the main floor of Convention Hall. There I saw and talked with many friends, including Frank Long (Cornell University). Long and I discussed some of the men who are about to receive their Ph.D.'s in nuclear chemistry at Berkeley and who might be good candidates for positions at Cornell.

I saw Edward King, formerly at Berkeley and now at Harvard, who asked about the course in nuclear chemistry Iz and I teach. I offered to send him copies of the problem sets and examinations.

Charles C. Price (University of Notre Dame) approached me and asked if I would be able to give a series of lectures, the Nieuwland Lectures, at Notre Dame this fall. I said that, if the date were flexible in order to fit in with some of my other trips, I would do it.

Later, Saul Winstein, my friend at UCLA, and his Harvard chemist friend, Bob (Robert B.) Woodward, had a long and interesting dinner with me.

[In Berkeley, Helen visited the Ghiorsos and had dinner at Leilani.]

Monday, April 14, 1947

In Atlantic City. At 9 a.m. I went to the meeting of the ACS Committee on Professional Training in the Stratosphere Room of the Hotel Traymore (8th floor). E. M. Billings was chairman.

I then attended the meeting of the ACS Committee on Foreign Compendia, which started at 11 a.m. at the Marlborough-Blenheim Hotel. Present were Henry B. Hass (Chairman), Roger Adams, Marston T. Bogert, L. F. Fieser, Henry Gilman, E. H. Huntress, C. S. "Speed" Marvel, Otto Kay, and A. V. Grosse. Dimitri R. Stein, an official representative of the Gmelin Institute in the United States, reported on the work of the Gmelin Institute in Germany (Clausthel-Zellerfeld, British Zone, under the direction of Dr. E. Pietsch). The biggest need is for current English and American literature for the war years. There is a stock of back issues of Gmelin and unbound sheets of issues printed during the war, and there was a discussion about the sale of these overseas. Dr. Otto Kay presented his proposal about the publication of an English translation of the Gmelin Handbook by the Gmelin Institute. The Committee reaffirmed its previous recommendation that the publication of Gmelin and Beilstein by Gmelin Institute and the Beilstein-Redaktion be continued; welcomed in principle, the publication of an English translation of the Gmelin Handbook by the Gmelin Institute, concurrent and simultaneous with the German edition; favored establishment of an International Advisory Committee for the Institute as soon as possible; suggested the possibility of a non-profit or low-profit distribution in the United States of the future English edition and the now available volumes; and suggested that such attempts be based on a strictly non-profit organization of the Gmelin Institute.

I was appointed to a subcommittee to investigate the obtaining of funds from our chemical industry in order that the Committee on Foreign Compendia carry out its work effectively. Other members of the subcommittee are Fieser, Huntress, and Grosse.

Marvel read a letter from Mr. Matthew Miller of FIAT about the status of the Beilstein Handbook; the Committee will suggest that the Board of Directors of ACS recommend to the military authorities the necessity of allocating the paper for printing the Beilstein volumes. The Committee unanimously moved to make a request to the Board of Directors of ACS for $500 to carry on its business.

At 2 p.m. I went to the meeting of the ACS Committee Advisory to the Chemical Corps in the Pine Room of the Hotel Traymore. Remarks were given by Dr. W. A. Noyes, Jr. (chairman), Major General Alden H. Waitt, Colonel Charles E. Loucks, and Mr. L. Wilson Greene (all of the Chemical Corps). They told us that the ultimate object of their chemical research and development is primarily to provide specific agents, munitions, and protective devices that can be used in combat. Our committee may be asked to advise on (1) Research, (2) Development of offensive and defensive devices, (3) Process development and plant design, (4) Production, and (5) Technical administration. Noyes then went on to describe the formation, organization, and operation of subcommittees with the assignment of a number of tasks.

At 7 p.m. I attended the dinner meeting of the Division of Physical and Inorganic Chemistry in the Hotel Madison and then stopped in at the general mixer on the main floor of Convention Hall. The program featured the Rhythm Doctors, an amateur orchestra of chemists and chemical engineers from Wilmington, Delaware. Among the people I saw during the evening was Alan F. Clifford, a former Met Lab colleague and a graduate student at the University of Delaware. Clifford queried me about working for his Ph.D. in Berkeley after he receives his master's degree. I told him to write to me when his work is nearer completion.

[In Berkeley, a telegram arrived from W. H Sullivan: "WOULDN'T WANT TO PERPETUATE TITLE CHART OF ISOTOPES BECAUSE OF LACK OF BETTER TERM AND WOULD PREFER CHART OF NUCLEAR SPECIES WITH NUCLIDES APPENDED PARENTHETICALLY TO INTRODUCE NEW SYNONYMOUS TERM NOT YET WIDELY KNOWN BUT WHICH MIGHT THROUGH USE. WOULD PREFER WIDESPREAD REACTION TO TERM NUCLIDE FIRST BUT PERHAPS REACTION FROM USE IN PROJECT PUBLICATION WILL BE HELPFUL IN DETERMINING USE ELSEWHERE."]

Tuesday, April 15, 1947

In Atlantic City. In the afternoon session, I heard some of the papers given in the meeting of the Division of Physical and Inorganic Chemistry, presided over by Henry Eyring.

Then, at 7 p.m., I attended the Alpha Chi Sigma dinner at the Hotel Chelsea, where the announcement was made that I am the recipient of the 1947 Award for Pure Chemistry ($1,000). At this meeting Charles A. Thomas, Vice President of Monsanto Chemical Company and President-Elect of the ACS, spoke on the peacetime applications of atomic energy.

[In Berkeley, Perlman conducted our group meeting. The following were present: Asprey, Blaedel, Conway, Covey, Cunningham, Fries, Garden, Goeckermann, Hopkins, Howland, James, La Chapelle, Lilly, Lindner, Magnusson, Miller, Morgan, McCullough, Orlemann, Perlman, Robinson, Stewart, Templeton, Thompson, Werner, and Wheelock. Perlman announced that there will be two meetings in the next three weeks with the next meeting on Thursday, April 24.

Lindner reported on his bombardment of antimony with 200 Mev deuterons in which the palladium fraction showed almost entirely positron activity with a very short period and a 13-hour period. He said the new palladium positron emitter must have a mass number below 102, indicating that at least 22 mass units were knocked out of the compound nucleus.

Blaedel talked about his work with Westrum and their attempts to make a protactinium hydride since they have never proved chemically that they made the metal. The hydride experiment gave negative results. In their attempts to determine the melting point by the "jaw" method, the jaws moved together at about 2250°C, but they feel that the jaws closed because of an alloy formation. Using another method the suspected metal was heated on a platinum filament in an atmosphere of helium. At 1200° the piece spread over the platinum surface, but the etched platinum surface showed that a reaction had taken place.

James reported a half-life for Pu^{238} as 92 ± 2 years by milking

Pu^{238} from Cm^{242} (Jaffey's latest figure is 90 years by direct decay). James' figure for the half-life of Np^{235} (followed for 11 months) is 400 ± 20 days.

Cunningham spoke of the difficulties with the Nalcite column: bubbles are still showing up after about five hours of operation even though the resin was boiled and the column operated at only 70°C. He is making a room temperature run to separate curium, americium, and neodymium; the plot of specific alpha activity of successive samples of the eluate shows one maximum and no β^- (neodymium) yet. The run will continue for about 100 hours. Hopkins is also studying the equilibria for americium, curium, neodymium, and element 61. Stewart talked about the equilibrium of americium, curium, and element 61 in citrate solutions on the Nalcite resin at 25°C.

Goeckermann described his double target of lead and thallium that was bombarded with 200 Mev deuterons and examined for strontium, molybdenum, and ruthenium fission product activities. The relative cross sections are 50 for fission of lead and 10 for fission of thallium; on the same arbitrary scale, the fission of lead by 400 Mev helium ions is 500.

Morgan talked about his attempts to make mass assignments for the new 12-hour and 50-hour americium activities, made by a 20 Mev deuteron bombardment of Pu^{239}, by means of an excitation curve for which the deuteron energy was degraded by platinum absorber. Unfortunately, the platinum absorber melted. He said the electron spectrum, measured with the magnetic counter, shows structure and probably consists of several conversion electrons of about 0.20 to 0.23 Mev; the decay of the x-rays is being followed. He said the alpha branching of the 12-hour americium is only ~0.1%.

* * * * *

Perlman discussed piles at today's Chemistry 123 class and collected the problem set.

The following wire arrived from Truman Kohman: "SULLIVAN SUGGESTED CHART OF NUCLEAR SPECIES (NUCLIDES). MULLIKEN PREFERS CHART OF NUCLIDES (NUCLEAR SPECIES). I ALSO THINK THIS IS MORE LOGICAL. WOULD YOU AGREE?"]

Wednesday, April 16, 1947

In Atlantic City. At this morning's ACS meeting, I heard several papers in the Division of Physical and Inorganic Chemistry. I was particularly interested in one entitled "The X-Ray Structure of Uranium Hydride and the Lower Thorium Hydride" by R(obert) E. Rundle.

Then, at 2 in the afternoon, I went to the General Meeting in the Ballroom of Convention Hall. There was a special program devoted to International Cooperation in Science. A very interesting talk was given by Marston T. Bogert (President of the International Union of Chemistry) on "Rebuilding and Advance of the International Union of Chemistry."

A telegram arrived for me from Judith Grayson, Newsweek magazine, "WOULD GREATLY APPRECIATE PERMISSION TO PUBLISH YOUR LETTER OF APRIL 9.

FEEL READERS WOULD BE INTERESTED IN INFORMATION GIVEN. PLEASE WIRE COLLECT AT YOUR EARLIEST CONVENIENCE." By return wire I said, "PLEASE DO NOT PUBLISH MY LETTER OF APRIL 9."

In the late afternoon I went to the cocktail party given by John Wiley Publishers in the Hotel Claridge.

[In Berkeley, Helen took Pete to Dr. McAlear for a check-up and in the afternoon went to the tea for chemistry wives at Genevieve Calvin's home.]

Thursday, April 17, 1947

In Atlantic City. Jim Crowe and I played a round of golf on a local course.

Later, I took a taxi from my hotel to the Atlantic City station and caught the 4:35 p.m. train to North Philadelphia. In North Philadelphia I transferred to the Pennsylvania Railroad train "Broadway Limited" at 7:19 p.m., bound for Chicago.

[In Berkeley, Iz Perlman gave the Chem 123 lecture--the topic was preparation of radioactive materials.

Joy (Townsley) and Carl Mann visited Helen today. (Helen has known Joy since high school and junior college days.)]

Friday, April 18, 1947

In Chicago. I arrived at the Englewood Station at 8:45 a.m. and went by cab to the Hotel Windermere East. After checking in, I took a taxi to the University of Chicago campus and went directly to the New Chemistry Building.

Among the men I talked with was Nathan Ballou, who worked with Charles Coryell during the Metallurgical Laboratory days. Ballou is completing his doctorate at the University of Chicago and is naturally looking for a job. I explained that we cannot hire permanent men at the Radiation Laboratory but we may be able to work out some sort of temporary position.

Other conversations were with Joe Katz and others about Plutonium Project Record matters, Winston Manning on our collaborative research programs, Darrell Osborne and others on our various cyclotron bombardments, and Art Jaffey about his Pu^{238} decay measurements, etc.

I returned to the Windermere East Hotel by cab and had a quiet dinner there.

Saturday, April 19, 1947

In Chicago. Today is my birthday, which will be celebrated after I return to Berkeley. [My friend Joe Katz has the same birthday and was born in the same year--1912.]

I again took a taxi to the New Chemistry Building and continued the discussions of yesterday. Earl Hyde told me that he has observed the n,γ reaction on Th^{233}, through the identification of Th^{234} in neutron-irradiated Th^{232}.

I returned to the Windermere East Hotel by cab and found a telegram from M. W. Welch: "PLEASE MEET ME UNIVERSITY CLUB MONROE AND MICHIGAN 4:30 p.m. SUNDAY IF INCONVENIENT FOR YOU PLEASE TELEGRAPH ME COLLECT 888 HILL ROAD WINNETKA." I then had a quiet dinner in the hotel.

[In Berkeley, Helen went to an Iota Sigma Pi dinner with Bea Barbachano.]

Sunday, April 20, 1947

In Chicago. During the morning I took a leisurely walk through my old neighborhood and wound up at the home of Winston and Dorothy Manning (on Ingleside Avenue, across from New Chemistry), where I joined them and their seven-year old son Steve for midday dinner. I was introduced to their new two and one-half month-old daughter Joan.

Later in the afternoon I took the Illinois Central train to the Loop and met, at 4:30 p.m., M. W. Welch at the University Club. We discussed their periodic chart, especially the way to fit the actinide elements into it. Welch promised to mail a copy of the chart and a slide of it to me in Berkeley.

I then went to the ABC broadcasting studio on the 20th floor of the Merchandise Mart and met John Lewellen. I witnessed the live broadcast of the "Quiz Kids" radio program and then had a light supper with John and some of the staff. Afterwards I hurried back to my hotel to go to bed in order to get some relief from the migraine headache that had developed during the afternoon.

[In Berkeley, during the morning Herman Robinson brought Zene and Celeste Jasaitis over to visit Helen; he took some pictures of Peter.]

Peter Seaborg and Zene Jasaitis
April 20, 1947 (10:15 a.m.)
836 Washington Street, Albany

Monday, April 21, 1947

In Chicago. I walked from the Windermere East Hotel to the nearby Museum of Science and Industry (57th Street and Outer Drive at Jackson Park). There, at 9 a.m., I attended the Chemistry Session of the AEC Information Meeting, held in the large auditorium of the West Wing. F. H. Spedding was the chairman, and there were some very interesting talks. John A. Ayres spoke on "The Purification of Zr Salts by Ion Exchange" (he has separated iron, titanium, and rare earths but not hafnium); Ray Fisher talked on "Removal of Rare Earth Impurities from Thorium Nitrate by Solvent Extraction" (using tributylphosphate and butyl ether); Bruce Ketelle's talk was on "Rare Earth Separations by Ion Exchange Techniques" (he uses Dowex-50 and high temperatures for separations); Donald Ahmann spoke on "Recent Work in Rare Earth Metallurgy" (he has prepared kilogram quantities of lanthanum and cerium in addition to neodymium); George Reed talked on "Thermal Neutron Fission and Capture Cross Sections for Pu^{238}" (fission cross section is ~16 b); and A. H. Jaffey spoke on "Thermal Neutron Capture Cross Section of Np^{237} and the Radiations of Np^{238}" (Np^{238} decays with a 0.2 Mev beta particle and 1.2 Mev gamma ray in addition to a decay by a 1.4 Mev beta particle; from this he determined the half-life of Pu^{238} to be 77 years, but by direct decay the half-life is 89 years; the capture cross section of 37 is 153 b (77 year) and 177 b (89 year).

I then spoke on "Nuclear Reactions Produced by 200 Mev Deuterons and 400 Mev Alpha Particles, Including Observations on Fission in Several Elements Including Lead, Thallium, and Platinum." Nathan Sugarman talked about "Short-Lived Ge and Se Fission Products" (he found 67-second Se^{83} and 59-second Ge^{71}); Aaron Novick discussed work with H. L. Anderson on "Production and Properties of Tritium" (half-life of H^3 of 11.4 ± 0.2 years from rate of growth of daughter He^3--Rod Spence noted that Los Alamos is getting 10 years by direct decay); W. A. Rodger reported on "Development for Production of Radioisotopes;" Don Hughes spoke on "Properties of Be^{10}" (half-life of 3×10^6 years); and R. L. Macklin talked about "Gamma Rays of U^{235}" (165 kev energy).

A letter from Iz Perlman arrived here in Chicago for me. Iz explained that he was writing in case he could not get through by phone. He reported that fission was observed with 200 Mev deuterons on lead and thallium and said that I probably now know that Sullivan voted for "Chart of Nuclear Species (Nuclides)." Iz also mentioned the telegram from Newsweek (that I eventually received in Atlanta), letters from A. C. English, Edgar Gordon from Wisconsin, and Strong from Terre Haute. He also told me that Ralph James' new half-life value for Pu^{238} is 92 ± 2 years and for Np^{235} is 400 ± 20 days.

[In Berkeley, Helen went to a child guidance meeting with Rebecca Cason during the evening.]

Tuesday, April 22, 1947

In Chicago. I was chairman at the 9 a.m. Chemistry Session of today's Information Meeting, again held at the Museum of Science and Industry. Speakers today were C. E. Larson (with G. Nessle and W. R. Grimes) on "Potentiometric Determination of Uranium"; N. Baenziger on

"The Crystal Structure of U_2O_5" (he has prepared single black crystals); L. B. Rogers on "Polarography with Stationary Electrodes" (he intends to apply this to element 43); L. E. Burkhart on "The X-ray Spectrography of Element 43"; Charles V. Banks on "The Determination of Carbon in Th Metal"; B. F. Scribner on "Improved Technique of Spectroscopic Analysis of Beryllium"; David Peterson on "Quality of Th versus Purity of Ca Used in Th Production"; Wright Langham (with W. W. Foreman and Arthur Murray) on "The Use of the Halogen-Metal Interconversion Reaction for the Preparation of Compounds Marked with C^{14} and its Application to the Synthesis of Labelled Nicotinic Acid"; E. S. Amis on "Physical Properties Versus Molecular Weights of the Chlorotrifluoroethylene Polymers, MFL"; L. I. Katzin on "Organic Solvates of Uranium Nitrate"; Louis Kaplan on "Enhanced Solvent Extraction of Heavy Metals by Solvent-Soluble Bases"; and H. H. Hyman (with F. T. Hagemann and S. Vogler) on "Separation of Gram Quantities of U^{233} from Th."

I took a taxi to the Chicago airport and caught UAL Flight 35 for Oakland at about 8 p.m.

[In Berkeley, Iz gave the Chemistry 123 lecture on charged particle bombardments.

The report of the Berkeley Chemistry Group for the period February 1 to March 1 by Amos Newton (for Eastman Kodak) was issued today. He covered the following items for the work of our group: A new isotope of element 85, At^{210}, is described, decaying by K electron capture with a 7.8 hour half-life to Po^{210}. Fission has been observed in lead, bismuth, thallium, and platinum with 400 Mev helium ions. A comparison of the yields of some fission products has been made showing a lowering of the fission probability in going from lead down to platinum. The production of At^{211} from Bi^{209} by deuteron bombardment has been shown to be possible by reaction of secondary alphas; the cross sections for the reactions are not improbable ones.]

Wednesday, April 23, 1947

I arrived in Oakland at 5 a.m. and took the limousine to downtown Oakland, where Jeanette met me and drove me home. After breakfast I went to the laboratory and immediately looked over my stack of correspondence.

I answered a letter from W. D. Leech, who wrote about the arrangements for the 20th Faraday Lecture that I am to give on April 29. I explained that I shall arrive in Los Angeles on the morning of April 29 on the "Lark" and will spend most of the day with my parents. I suggested that someone come by my parents' home (9237 San Antonio, South Gate) to take me to Pasadena. My parents' phone number is Kimball 1738, I said, and I presume that he can reach me through a dial phone even if the phone strike is still on.

I noted an acknowledgment from Edgar S. Gordon to my acceptance of his invitation to speak at the symposium at the University of Wisconsin on September 10. Gordon is allowing fifty minutes for my talk and will send me instructions about the manuscript within two weeks.

Notification of my appointment to a promotion committee under the

chairmanship of Professor H. A. Barker arrived from the Office of the President (Provost Monroe E. Deutsch). The name of the person to be considered is to be obtained from the chairman or the President's office.

I read a note from John E. Pfeiffer: "I am very sorry I missed you this week, but please let me know when you will be here next time, and let me know well in advance."

A copy of the notes for a course at MIT, "Seminar in Nuclear Science and Engineering," arrived for me from the secretary to Dr. J. R. Zacharias. She said that Dr. James B. Fisk suggested that I would be interested in seeing them; she will send me copies of future lectures.

I read a copy of a letter from Ralph James to Art Jaffey, dated April 17, about his measurements of the half-life of Pu^{238} (92 ± 2 years); James again described this work. He also mentioned his direct decay measurements of the Np^{235} half-life of 400 ± 20 days. James asked that these values be used in his Plutonium Project Record papers.

I deferred answering the rest of my correspondence in order to visit the various laboratories and check on the progress of the research.

Thursday, April 24, 1947

Our group met today (Thursday) instead of Tuesday (when I was absent). Present today were Asprey, Conway, Covey, Cunningham, Eyring, Garden, Ghiorso, H. Hicks, Howland, Huffman, La Chapelle, Lilly, Lindner, Magnusson, McCullough, Miller, Morgan, O'Connor, Perlman, Robinson, Seaborg, Street, Templeton, and Werner. [Hicks and Street just obtained their clearances and can now attend our meetings.] Perlman announced that the next meeting will be Tuesday, May 6.

I then gave a few remarks about the ACS meeting and the Information Meeting. I mentioned that Bogert, at the ACS meeting, commented that there is no universal agreement on the names and discoverers of elements 43, 61, 85, and 87. I said he apparently doesn't feel the decisions on these are as clear cut as we think they are; he seems to misunderstand the situation with respect to element 61. I also told about Jaffey's results on the half-life of Pu^{238}, the half-life of H^3 (10-11 years), and the report by Ketelle on the high temperature operation of ion exchange columns.

Morgan summarized the radiations associated with the 12-hour and 50-hour americium activities: the radiations of the 12-hour period consists of 0.1% alpha and its other emissions, x-rays, 220 kev e^-, and 240 kev gamma ray in the ratio 1:1:0.5. The 50-hour activity consists of x-rays, 0.9 Mev e^-, and 1 Mev gamma ray in the ratio of 1:0.1:0.9. The ratio of the yields of the 12-hour and 50-hour x-ray activities are roughly the same when the activities are produced by either 12 Mev or 20 Mev deuteron bombardment of Pu^{239}. Although these radioactivities could be isomers, the gamma decay curve showed no genetic relation.

Cunningham then talked about a room temperature Nalcite column run to separate americium, curium, and neodymium. The neodymium was completely separated from the americium and curium, 50% of the curium was recovered,

and the americium in it was reduced 1:200. He said that it is clear that an actinide element forms more stable complexes than a rare earth of the same ionic radius.

O'Connor talked about his plans to study the distribution of isobars in the fission products of U^{238} when bombarded with 400 Mev helium ions. Conway announced that we are now set up to do spectrographic analyses of non-radioactive samples; the dry boxes for active samples are not ready yet.

McCullough spoke on the x-ray diffraction spectrum of a sample of protactinium metal heated to over 2000°C (by Blaedel); this fits into the series for the dioxides and probably the sample became PaO_2 on the surface.

Ghiorso told of his plans for more slow neutron fission measurements next week in the Argonne pile--Am^{241} and Cm^{242} and samples of Am^{241} and Cm^{242} that have been irradiated with neutrons at Hanford.

Miller reported on a study of the previously discovered 7.8-hour β^+-emitting Fe^{52} produced in the bombardment of copper with 200 Mev deuterons. He said the yield of the 21-minute manganese milked from it seems to drop off faster than the 7.8-hour decay, possibly suggesting that the Fe^{52} has isomers. I mentioned the Se^{83} and Ge^{77} isomers recently reported by Sugarman in his talk at Chicago on Monday. I also told the group about Earl Hyde's observation of the n,γ reaction on Th^{233}, by observing the formation of Th^{234} in neutron-irradiated Th^{232}. I speculated that a second order n,2n reaction on Pu^{238}, to produce 40-day Pu^{237} could complicate the processing of pile-produced plutonium.

* * * * *

Today's Chem 123 lecture covered methods of assigning mass numbers.

On the hill I wrote to Fred Albaugh to ask if he had made up his mind about the General Electric proposition. I said that, after seeing the key General Electric people in Chicago, I believe he should go to Hanford rather than Schenectady. I also mentioned to Fred that Spof English has decided to accept a position in the Washington, D.C. office of Dr. J. B. Fisk, Director of Research of the AEC.

I received another note dated April 19, from Mildred Hoogstraat who said, "Thank you for sending me the picture. It was nice. And thank you for writing on it. I see it every day. You look like you have a sense of humor. I like people like that. Today is your birthday, and I hope you have a nice time. Today is my mother's birthday, too. I bought her some flowers. Today will be Jean Louise's birthday next year, but she was just born today. I haven't seen her yet, but she has a pretty mother. Thank you again for the picture."

In the evening Helen and I walked down to the Albany Theater and saw Gary Cooper in "Cloak and Dagger."

Friday, April 25, 1947

The first thing I did this morning was to tackle my correspondence: Ralph K. Strong, (Editor, Kingzett's Chemical Encyclopedia), wrote on April 12, agreeing to my ideas about the article he requested and enclosing a skeleton outline. Today I again wrote Strong and said that Professor I. Perlman will write the article. I included a revised outline prepared by Perlman and explained that the payment should be made out to him.

I received a letter from A. C. English, dated April 12, in which he gave the authors for their "Letter to the Editor" on the 4n + 1 radioactive series, as A. C. English, T. E. Cranshaw, P. Demers, J. A. Harvey, E. P. Hincks, J. V. Jelley, and A. N. May. He said the Chalk River people have apparently agreed to parallel publication of our two letters on this subject.

While I was in Chicago, Nathan Ballou talked with me about a possible job in Berkeley. I also had a letter waiting for me here in Berkeley from Ballou about his background [Ballou worked for Coryell on fission product research during the Met Lab days and is now getting a Ph.D. from the University of Chicago]. Perlman and I talked about the situation, and so I wrote to Ballou this morning, offering him a year or two of postdoctoral work here at Berkeley. I told him that I shall ask George Everson to send him the proper forms.

The following note arrived from David E. Lilienthal (Chairman, Atomic Energy Commission): "My hearty congratulations to you on receiving the American Chemical Society's award in chemistry. This must give you great satisfaction. Best wishes to you as always."

I also received an April 21 letter from W. Albert Noyes, Jr., who also congratulated me on the award in Pure Chemistry and warned me that I may be requested to speak before the General Session at the New York City ACS meeting on September 17. Noyes said that other speakers at that session will be W. K. Lewis of MIT, who will receive the Priestley Medal, and Noyes himself, in his role as President of the ACS.

I sent the original and first copy of the article, "The Transuranium Elements," that I wrote with Emilio Segrè for Nature to Emilio for checking. Mary Bender typed this for us while I was back East.

Another note went to J. Kenneth Maddock (Manager, Science and Industry Division of John Wiley & Sons, Inc.) to thank him and Professor Yost for a copy of Rare Earth Elements and Their Compounds, which I said is an excellent book that will be widely used.

Finally, I returned the proofs of the photographs taken recently at Harris and Ewing Photographers in Washington, D.C. I told Miss Edith Mitchell that my first choice is "E" for the head-and-shoulders view and "D" for the half-length portrait.

Saturday, April 26, 1947

This morning I answered an April 18 letter from W. E. Kelley (AEC

Area Manager, New York Area), who said that he learned from the General Manager that the GAC has designated Cyril Smith, Hood Worthington, and me as a group to review the work at Battelle Memorial Institute on low grade ores. Kelley suggests that the group visit Battelle before the May 14 and 15 conference on low grade ores in Washington. I explained in my reply that, since I have just returned from two extended trips East, I shall not be able to come East again until a few days before the next GAC meeting, at which time I hope to attend a May 28th meeting in this connection. I wrote that I hope Dr. Smith and Mr. Worthington will be able to visit Battelle before then.

In today's mail was a form letter from Albert Einstein, requesting support for the "Emergency Committee of Atomic Scientists."

A letter also arrived from Dr. W. D. Leech, giving more details of the arrangements for the Faraday Lecture next Tuesday. Leech said the audience will consist not only of the 300 special youngsters, but also a fair sprinkling of visitors from Cal Tech and Mt. Wilson and a mixed group from the community. Leech suggested that this will present a real challenge to me.

Sunday, April 27, 1947

Most of my day was spent with my family, but I also read and went over my notes and assembled my slides for my talk, "The New Chemical Elements," for the Faraday Lecture in Pasadena on Tuesday.

Monday, April 28, 1947

This morning I checked on the research and then wrote to Winston Manning about Professor Herbert A. Young, Head of the Chemistry Department on the Davis campus. Young is scheduled for a sabbatical next semester and, although we would like to have him work in Berkeley, the Administration believes he should go farther from home. I talked with Young about Argonne; in this letter, I told Manning that I believe he should bend every effort to get Young to spend the leave at Argonne.

Another note went to Cyril S. Smith, who asked on April 10 for an evaluation of Richard A. Reinhardt. I explained that I have just learned that Reinhardt accepted an instructorship at Cornell so I shall not write the letter of recommendation.

Fred Albaugh replied to my query of last Thursday, saying that he intends to join the Hanford group although he has not yet received a formal offer from them. Fred also mentioned that the salary scale seems lower than that of the oil industry. Edrey, Fred said, has been visiting in Chicago and the novelty of "batching" has worn off. They expect to pass through Berkeley on their way to Hanford in about a month.

In the evening I boarded the "Lark" for Los Angeles.

Tuesday, April 29, 1947

In Los Angeles. I went to my parents' home in South Gate in order to spend the day with them.

About 5 p.m. I was picked up by W. D. Leech and driven to Pasadena. I had dinner with a small group and then, at 8 p.m., gave my Faraday Lecture at the Sexson Auditorium on "The New Chemical Elements." The student contestants were given an instruction sheet (a copy follows) --they were to be judged on the quality of their notes on the lecture; these were to be handed in twenty minutes after the conclusion of the lecture.

THE FARADAY LECTURES

Pasadena City Schools

Suggestions and Instructions to Student Contestants

We look forward to an interesting evening on Tuesday, April 29, 1947, at the Sexson Auditorium, when Dr. Glenn T. Seaborg, head of the chemical division, U.C. Radiation Laboratory, will speak on New Elements, and How to Make them.

TO CONTESTANTS: It is not expected that you will be able to write a set of notes which covers every point in the lecture. Perhaps some of the lecture you will not understand. The main purpose of the lecture is to give you a unique opportunity to hear an expert in the field of science and to do your best in writing a set of notes which covers as much of the lecture as possible. Do not feel disappointed if you get only part of all that is presented. Do your best with the parts which you understand and have time to take down.

Parents are invited to attend the lecture.

GENERAL INSTRUCTIONS

1. Secure this sheet from your Junior High School teacher and fill in the bottom card admitting you to special center front section of auditorium.
   Sign the right hand side and return that part to your teacher.
2. Attend the lecture and take notes. Paper for notes and identification cards will be given to you at the auditorium. Put number of paper on ticket blank. All contestants are to be in place by 7:45 p.m.
3. Remain in your seats at the close of the lecture. **Twenty minutes** will be allowed for rewriting your notes or correcting them.
4. Hand in your notes to the instructor in charge.
5. You might bring a thin magazine on which to write.

OUTLINE OF NOTES (Suggestions for what might go into your notes.)

1. Title: What was the object of the lecture?
2. Method: How was the subject presented and developed?
   A. Apparatus: What apparatus or machines were used?
      (draw simple diagrams where you can.)
   B. Procedure: How was the apparatus used?
4. Observations: What did you see?
5. Conclusions: What do you judge or conclude from what you saw?
6. Principles: State principles that were involved in the discussion and development in the lecture
7. Applications: Applications to present day life.

EVALUATION OF NOTES (These are the points upon which your notes will be judged)

1. Grasp of subject matter and demonstrations.
2. Neatness, legibility, spelling, and arrangement.
3. Illustrations and principles.

Notes will be arranged according to excellence, and each school will be awarded at least one prize. NOTE: We are not only interested in how much the student actually learns from the lecture, but what inspiration he may derive from hearing an expert in his field.

PRIZES

First—2 Grand Prizes for the school system. Brown leather zip-zip, 3 ring brief cases with winner's name in gold. One for best boy and one for best girl. Junior High winners will receive a popular science book autographed by the speaker.

REFERENCES: Ask your librarian for book on atoms, radiation, atomic bombs, etc.

Later I was driven back to my parents' home to spend the night.

[In Berkeley, Iz gave the second midterm to the Chemistry 123 class.]

Wednesday, April 30, 1947

I returned to Oakland by plane and went to my office. After looking over my mail, I checked the various laboratories to learn about the

progress of the research.

A copy of "The Vapor Pressure of Americium" by N. D. Erway and O. C. Simpson (CC-3721) has arrived in Berkeley, along with a copy of "The Dry Chemistry of Americium" by Sherman Fried (CC-3786).

I wrote to John Willard and told him that I have seen Professor Mathews' (J. Howard) offer to Blaedel and that I think it is a good proposition. I recommended Blaedel very highly and said that I shall write a letter of recommendation to Mathews as soon as I have time.

Iz stopped by the house during the evening to pick up some slides for a talk he is going to give.

Thursday, May 1, 1947

After going over my notes for Chemistry 123, I gave the lecture at 10:10 a.m. in Room 219, Gilman Hall. I defined the broad term, "nuclear chemistry," and then talked about the various properties that can be determined by tracer techniques. I also assigned a new problem set. Afterwards I stopped at the third floor and talked with Geoffrey Wilkinson about his research. Iz and I recently talked with another graduate student, George Barton, who wants to do his graduate research in our field. Until has clearance comes through, Barton, too, will work on the third floor of Gilman Hall.

In the afternoon, Iz, Stan, Professor Gibson and I played a round of golf at Mira Vista Country Club (IP-108, SGT-91, GTS-99, GEG-111). Gibson and I played against Iz and Stan for two points per hole (low total and low ball), and we lost, 9 and 6.

Friday, May 2, 1947

After making the rounds of the labs, I answered an April 10 letter from Frank A. Long (Cornell University), who asked for more information about possible candidates for positions at Cornell. I said that I am not certain I need write because of our conversation in Atlantic City recently, but I will write a few lines about Morgan and O'Connor (Templeton has eliminated himself as a candidate). I said both men have an extraordinary ability to work with other people--Morgan, for example, has been assigned the responsibility for arranging all the irradiations our chemical group has undertaken with the 60-inch cyclotron group . Morgan has had more experience, I said, but I sometimes feel that O'Connor is further along for a comparable stage of experience. I noted that both men have had a number of academic offers; I also said that we shall have more men completing their work next year.

A letter of recommendation for Walter J. Blaedel was mailed to Professor J. Howard Mathews at the University of Wisconsin. I said that I chose Blaedel as one of only a couple of postdoctorate men to move to California when I returned here. Since coming to California, Blaedel has been in a field new to him, a sort of combination of microchemistry or ultramicrochemistry, thermodynamics, and nuclear chemistry; he has built apparatus and learned techniques. I wrote that I believe Blaedel is unusually well suited for his position, as John Willard can tell him, and we shall find it difficult to carry on Blaedel's work if he leaves. I also asked Mathews for applications for graduate school from outstanding University of Wisconsin graduates; occasionally, I said, we can use an outstanding postgraduate man for a year or two postgraduate experience.

I mailed "RADIOACTIVITY, ARTIFICIAL" (54 typed double-spaced pages) to Welden Reynolds of the Encyclopaedia Britannica. In my covering letter I suggested that it would be desirable to have more cross references to Teller's article on the "Nucleus" and Chadwick's article on "Radioactivity, Natural."

To M. W. Welch I acknowledged receiving the large periodic chart and a colored slide of it; these were mailed to me on April 25.

I also wrote to B. S. Kinsella to acknowledge the check for $400 for the fee for the revision to the section on "Chemistry" for the Lincoln Library of Essential Information, adding that I shall be glad to work on the galley whenever it might be necessary. Since Iz prepared the revision, I endorsed the check to him.

A nice note arrived today from Hugh D. Beach from Newsweek, thanking me for my interest in their U^{233} story and expressing regret that I did not allow my letter to be published.

During the evening Helen and I left Peter in the care of his grandmother and went to the Oaks Theater and saw Sydney Greenstreet and Peter Lorre in "The Verdict."

Saturday, May 3, 1947

At the lab this morning I again dictated responses to some of my mail and then visited the laboratories.

I answered an April 28 letter from W. Albert Noyes, saying that I shall be glad to give an address at the General Session of the New York City meeting on September 17. I added that I presume this is the same talk about which I have had correspondence with Mr. Emery.

A thank-you note arrived from Ruth A. Ashton of the CBS Documentary Unit. Miss Ashton said her trip, for the purpose of obtaining background material for an article on atomic energy, was successful and she is now beginning the task of writing the program.

Professor W. W. Lake (College of Mines and Metallurgy, El Paso, Texas) wrote on May 1, asking for the pronounciation of americium. Today I replied, again explaining that we pronounce it with a soft "c."

I thanked Professor J. R. Zacharias (MIT) for the notes on his seminar in Nuclear Science and Engineering that Barbara M. Bowles sent me and said that I look forward to receiving copies of the notes of other lectures as they are issued.

To Harlan L. Baumbach (fellow UCLA student and Met Lab colleague) at Paramount Pictures, I wrote, "I want to heartily congratulate you on your winning of an Academy Award. I know that you are the best chemist in the motion picture industry but I didn't realize that the Award Committee would have sufficient perspicacity to recognize this. Best regards to you, Nathalie, and little Robert."

Sunday, May 4, 1947

I worked for a while on an abstract entitled "Transmutations with High Energy Deuterons and Helium Ions in the 184-Inch Cyclotron"; this paper will be presented at the Stanford meeting of the American Physical Society in July.

In today's golf game at Mira Vista Tom Morgan got 112, I scored 109, and Professor Gibson got 111.

Monday, May 5, 1947

The first thing I did when I got to my office this morning was to take care of my correspondence. I replied affirmatively by collect wire to Warren Weaver's invitation of April 21. Weaver explained that he, Dr. George W. Corner, Dr. Douglas S. Freeman, Dr. Frank B. Jewett, Dr. Harlow Shapley, and Dr. Wendell M. Stanley, constitute an advisory committee that plans and selects speakers for the radio programs on science given during the intermission of the Philharmonic Symphony programs on Sunday afternoon. He said that, if U. S. Rubber continues to sponsor the concerts, the committee unanimously wants me to be one of the speakers for the 1947-48 season. If I accept, I am to prepare a script of 1350 words ($300) with the understanding that, if the program continues, I shall receive an additional $200 for delivering the script on the air.

I also agreed to attend the luncheon of the Alumni Association in the Faculty Glade on June 21 (Robert Sibley, Executive Director of the Alumni Association extended this invitation on April 24). However, Sibley requested that I set up a Geiger counter for a demonstration for the 1,200 guests. In a letter today I suggested that it would be better if I didn't participate in the demonstration but that one of the fellows from the Radiation Laboratory could do this. I told Sibley that I will say a few words about the role of the UC scientists in the development of atomic energy.

I read an April 29 response from Neil Gordon, who will try to comply with my requests about my manuscript (Wayne University talk). He has heard from O'Connor and Morgan and is interested in both men for positions at Wayne University. Gordon mentioned that he has completed negotiations with a man from MIT for a job in the fall.

In a May 1 letter, Alden Emery sent me an invitation that President Noyes had received, which asked for a speaker to represent the ACS at a meeting of the Chemical Institute of Canada in Banff (June 8 to 11). President Noyes, Emery said, feels I am the logical man to do this. In today's reply I explained that I have already refused an invitation to speak from Dr. Leon Lortie because of a conflict with final examinations here in Berkeley and that, for the same reason, I must not accept Noyes' suggestion. I mentioned that Professor Clifford S. Garner of UCLA might be able to attend but noted that it may be difficult to find anyone to attend unless there are arrangements for travel reimbursement. A blind copy of this reply was sent to Noyes.

I wrote to Professor Franz Schrader (Zoology Department, Columbia University), explaining that I am trying to find a proper place in the academic field for an outstanding scientist, Leonard I. Katzin, who worked with me at the Met Lab. I described Katzin's qualifications and said that, although he has a good position at Argonne National Laboratory, I know he wants to get into academic work.

To James B. Fisk (Director of Research, Atomic Energy Commission, Washington, D. C.), I sent a copy of a letter about the meeting of the XIV Congress of Pure and Applied Chemistry in London in July. I explained that I know that organizers of the Congress would appreciate American representatives and suggested that perhaps the Atomic Energy

Commission might send a few representative American chemists to the symposia on radiochemistry. I then commented on Spof English's acceptance of a job with the AEC and said that it is a serious loss to the University of California.

I then made the rounds of the labs to check on the progress of the research.

Helen's mother, who has been here to help Helen for about six weeks, left for her home in Los Angeles today.

Tuesday, May 6, 1947

Present at this morning's meeting of our group were Asprey, Blaedel, Conway, Covey, Cunningham, Eyring, Ghiorso, Goeckermann, H. Hicks, Hopkins, Howland, Huffman, James, Lilly, Lindner, Magnusson, McCullough, Miller, Morgan, Newton, O'Connor, Perlman, Robinson, Seaborg, Street, Templeton, S. Thompson, Voyer, Werner, and Wheelock. There were a few announcements, such as one about a meeting this afternoon to work out a schedule for the 60-inch cyclotron for the coming year, and people who want such bombardments should go prepared to state times and dates of the irradiations. It was suggested that Howland and Templeton look into the possibility of a mercury target. Cunningham and Thompson were asked to have the americium target ready for bombardment in the high flux Canadian pile before June 1. Since there will be more curium samples because of americium pile irradiations, the men were reminded that great care be taken to maintain the identity of the samples.

Templeton reported that it has been learned that the so-called helium ion bombardments on the 184-inch cyclotron may have contained up to 50% deuterons because of inadequate flushing. Eugene Gardner will flush for about a week with helium if there are sufficient cases to warrant it. It was then decided that this is desirable about July 1 for copper bombardments for Miller, arsenic for Hopkins, antimony for Lindner, uranium for O'Connor, uranium behind beryllium for James, and bismuth for Goeckermann.

Ghiorso discussed the question of what added isotopes causes the fission cross section of neutron-irradiated Am^{241} to be raised from 2.8 barns (americium milked from plutonium) to apparently 40 barns (neutron-irradiated Am^{241}). He believes Am^{240}, Am^{242}, and an isomer of Am^{241} should be ruled out, but he stated that Am^{243} is a remote possibility since it may be β-stable and might be formed by $Am^{241}(n,\gamma)Am^{242} \rightarrow Cm^{242}(n,\gamma)Cm^{243} \rightarrow$ (K cap) Am^{243}. Ghiorso said that if the effect is due to Am^{243}, its thermal neutron fission cross section would have to be about 10^4 barns.

Conway reported that the optical spectrograph is in focus and can handle a limited number of samples.

Lindner looked at ruthenium, rhodium, palladium, and silver activities in a bombardment of antimony with 200 Mev deuterons: the rhodium chemistry was inadequate, 3-hour and 5-day activities were found in both the ruthenium and palladium fractions, and the palladium activities are positron emitters and may be due to tellurium

contamination. The silver fraction showed 3-hour and 8-day activities; the former is probably Ag^{112} while the 8-day is probably Ag^{106}.

Werner talked about his work on the chemistry of curium. A curium absorption curve showed no well-defined peaks. He discussed his separation attempts using TTA in benzene; he has, at present, 3 μg curium with less than 0.2 μg americium and about 100 μg of plutonium as the sulfates.

Hicks talked about his work with Wilkinson and Street on the radioactive isotopes of gold and platinum: bombardments by deuterons of platinum at the 60-inch cyclotron give gold activities of 13 hours (Au^{194}?), 5 days (Au^{196}?), 100 days (Au^{195}?). There was evidence of a 4.4-day Pt (Pt^{193}?) growing from the gold fraction. Iridium plus deuterons showed 60-day Ir^{192}, 19-hour Ir^{194} and a 2.9-day platinum and the 4.4-day platinum. The 5-day and 13-hour gold activities appear to decay by K-capture.

* * * * *

I gave the Chem 123 lecture, which was a continuation of a discussion of natural radioactivities of light elements. I also collected the problem set.

Back in my office on the hill I answered an April 30 request from H. B. Hass (Purdue University) for the name of a young physical chemist experienced in tracer work and an interest in biochemical research. I suggested the names of Peter E. Yankwich and Bert M. Tolbert as men who would be interested in regular departmental staff positions only. In addition, I mentioned Leonard I. Katzin as an unusually capable man, who has a fine position at Argonne Laboratory but wants to find a place in academic work.

Jim Crowe asked me, at Atlantic City, to locate other pictures of G. N. Lewis, suitable for a C & E News cover sketch; he already has a side view photograph. He reminded me of this in a letter dated April 29 and also asked about two slides he had mailed me. Today I replied to his letter, said that I have been unable to locate any other picture of Lewis, and that I received the slides in plenty of time. I then mentioned that I shall be in Washington from Wednesday morning (May 28) through Monday (June 2), and I hope I may have time for an afternoon of golf or at least a dinner with him.

Later (4:15 p.m.) I went to the joint meeting of the Connick-Seaborg groups on campus; in the evening I attended the weekly Research Conference.

At home Helen reported that Peter had taken his first step today.

Wednesday, May 7, 1947

I took care of some administrative matters and made the usual rounds of the laboratories to discuss the research.

I mailed Edward King (Harvard University) copies of the examinations

and problem sets that we have used in Chemistry 123.

One time-consuming task I have is reading and assimilating the data from various journals and classified reports; this is something I try to do regularly.

Later I went to Gilman Hall to check in at the Department office, to speak with Wilkinson about his work on platinum and gold isotopes, and to to look over the lecture notes for tomorrow's Chemistry 123 class. I also made out a new problem set for the class.

Herman Robinson came over to the house this evening to determine why our phonograph is not operating. He ascertained the problem and will return tomorrow evening to repair it.

Thursday, May 8, 1947

I checked in my office before going to campus.

A thank-you note arrived in the mail from Ralph K. Strong for my letter of April 25 and Iz Perlman's outline of the section on "Nuclear Chemistry" for Kingzett's Chemical Encyclopedia. [Because of my limited time, I suggested that Perlman write the article for Strong.]

Then, on campus I gave the Chem 123 lecture--today I talked about recently discovered elements: 43, 61, 85, and 87. I assigned a new problem set. Afterwards I stopped in the Department office to check with Miss Kittredge and to arrange for an afternoon outing.

After lunch Bill Dauben, Ernest Gibson, and I went out to Mira Vista for a round of golf--WGD-106, GTS-100, Professor Gibson did not complete the round.

Helen reported that she had an appointment with Dr. Borson this afternoon--her pregnancy is progressing ok. In the evening Herman Robinson came by and completed the repairs on our phonograph. [My electronics ability has not improved since my graduate school days.]

Friday, May 9, 1947

After dealing with some administrative matters, I looked over the mail. I received an April 29 letter from Joseph F. Hynes (Assistant Managing Director, Mid American Exposition, Cleveland), who sent me and other scientists a list of questions about the feasibility of atomic power. I immediately replied that I have not yet formed opinions on these questions and I do not want to make guesses at this time.

In a May 5 letter, J. H. Mathews (University of Wisconsin) thanked me for my recommendation of Walter Blaedel and said that he wired Blaedel on May 5 and made him a definite offer.

Warren Weaver wrote (May 6) to thank me for accepting his invitation to write a radio script (Philharmonic Symphony broadcasts) and to ask for a preliminary draft around the first of June.

Robert Sibley, in another note, this one dated May 8, said he can understand why I do not want to use a Geiger counter in a demonstration (for my remarks at the Alumni Association luncheon on June 21) but he would like a piece of apparatus or material to give a dramatic appeal to my talk.

Saturday, May 10, 1947

I made the usual rounds of the laboratories to check on the research and talk with the fellows.

Today's mail brought me a letter from Herbert H. Anderson (colleague in Section C-I, Met Lab), in which he said that he gave my name as a reference for an associate professorship at the State College of Washington.

Sunday, May 11, 1947

Part of my day was spent drafting a radio script for the Philharmonic Symphony intermission, requested by Warren Weaver.

Later Tom Morgan, G. Ernest Gibson, and I went to Mira Vista for a game of golf--GEG-105, GTS-107. Morgan didn't complete the round; a couple of other fellows played along with us.

Monday, May 12, 1947

I declined an April 23rd invitation from A. J. E. Welch to attend and/or speak at the XIth International Congress of Pure and Applied Chemistry in London in July. I explained that a family matter must keep me at home at this time and then suggested a number of names, both American and non-American, of people qualified to speak on the topic of radiochemistry.

I also wrote to Professor J. F. Joliot:

> I had been hoping to attend the meeting of the Commission of Radioactive Constants of the International Union of Chemistry in London this coming July, but it is becoming apparent, due to a family matter which has arisen, that I shall not be able to do so. I am particularly disappointed because I have learned about the interesting symposia on radiochemistry which have been planned in connection with the International Congress of Pure and Applied Chemistry for the same period.
>
> It turns out that I wouldn't be able to have ready by that time a new up-to-date table of artificial radioactivities. It appears that such a compilation, upon which we are now working, will not be ready before the end of the year.
>
> I suppose that one of the questions which might arise at a meeting of the Commission of Radioactivity might be that of the new unit of intensity which has been proposed in some quarters, namely the "rutherford." I am personally not in favor of such a new unit, but favor the retention, or perhaps we should say formal adoption, of the

"curie" as a general unit of intensity, perhaps with a definition of its value as exactly 3.7×10^{10} disintegrations per second in order to meet one of the objections.

I do not know of Professor Lind's intentions, but I am hoping that he will be able to attend the London meetings, in which case I shall be able to learn of them first hand.

Iz Perlman showed me the list of 11 long-range requests for bombardments at the 60-inch cyclotron that he sent to Russell Ball in the AEC Area Manager's office.

James B. Fisk (Division of Research) wrote, in a letter dated May 7th, that he will visit Berkeley on May 15, 16, and 17. He would like to visit me and has written Ernest Lawrence. Fisk noted that he also wants to meet Professor Latimer. I then prepared a brief summary of our research problem in nuclear chemistry to give him when he visits:

Chemistry of the heavy elements. The program in progress concerns a study of both transuranium elements and those immediately preceding uranium, including those properties that are indicative of the structure of the heavy element transition series. In this latter connection some work is being done on the chemistry of the rare earth (lanthanide) elements for comparative purposes.

Some of the recently accomplished work includes: isolation of Am (element 95), experiments toward the isolation of Cm (element 96), absorption spectra of Am and Cm, preparation of halides and metals of Am and Pa and determination of some of their properties, study of the oxidation states of Am, separation of Am, Cm and rare earths by precipitation and adsorption methods, methods for decontaminating Am and Cm from fission products. The future program is a continuation of these investigations on Cm, Am, Pu and Pa and, in addition, more emphasis on Np chemistry.

Nuclear reactions with high energy particles. A large fraction of the effort is concerned with the identification of the products of nuclear reactions induced by the 200 Mev deuterons, 400 Mev helium ions, and 100 Mev neutrons produced in the 184-inch cyclotron. This work has resulted in the discovery and characterization of the fission reaction in the "light elements" between tantalum (element 73) and bismuth (element 83). Throughout the periodic table, many new nuclear reactions have been found in which the product nuclei are up to 8 atomic number units and 21 mass units lighter than the target nuclei. A considerable insight is being obtained into the properties of highly excited nuclei. The high energy particles also allow very light isotopes of a given element to be reached. As an example it has been found that very light bismuth isotopes are alpha-emitters.

The field of investigation opened up by the 184-inch cyclotron and future accelerators is so broad that it may be said that the program has just started. The creation of secondary high energy particles and mesotrons are among the problems that may be attacked by methods of nuclear chemistry.

Nuclear properties of heavy isotopes. A considerable amount of work has been done in the region of the heavy isotopes with the aim toward determining, for all isotopes possible, the decay characteristics, methods of preparation, formation by neutrons in pile operation, fissionability properties, etc. In the past year this program has resulted in a considerable extension of the list of known isotopes especially those of Bi, Po and Am. The investigation of fission properties is still in progress and will yield information of fission cross sections as a function of nuclear type. Part of these studies in progress is the attempt to prepare and identify isotopes of transcurium elements.

Training program. One of the most important functions of the laboratory is the training of graduate students in Nuclear Chemistry. About three-fourths of the work in progress now is being carried out by such graduate students.

After completing this summary, I answered an April 17 letter from Professor R. Delaby, saying that I heartily agree with the nomination of Professor F. Joliot as president of the Commission of Radioactive Constants. Then I explained that I will not be able to attend the International Congress of Pure and Applied Chemistry in July because of a family situation (Helen's pregnancy).

In today's mail was a May 4 letter from John R. Kuebler, Grand Recorder of Alpha Chi Sigma. Kuebler congratulated me on winning the ACS Award in Pure Chemistry and said The Hexagon (publication of Alpha Chi Sigma) would like a statement from me about the award and a photograph to publish in the September issue.

I also received a note from Paula Lane of Harris & Ewing (photographers of Washington, D. C.). She acknowledged receipt of the proofs I returned to them on April 25 and said my selections are being placed in their permanent files and will be available for prints when I need them.

Iz and I had a visit in Building 4 this afternoon from Commander J. J. Fee, who is in charge of the new Navy Radiation Laboratory at Hunters Point. We discussed procurement of laboratory personnel for his laboratory and designs for laboratories for working with radioactivity; we showed him the general features of a laboratory for handling alpha activity and the construction on Building 5.

Tuesday, May 13, 1947

At today's meeting of our group the following people were present: Asprey, Connick, Conway, Eyring, Ghiorso, Goeckermann, Hicks, Howland, Huffman, James, La Chapelle, Lilly, Lindner, Magnusson, McCullough, Morgan, O'Connor, Perlman, Robinson, Seaborg, Stewart, Street, Templeton, Thompson, and Voyer. I made a few announcements about note taking (Goeckermann, Lindner, and Miller have volunteered for the summer), vacations (should be taken during the last half of June, particularly for those who will work on the Hanford samples), the fact that Huffman's group can handle some analytical work, and that the men should see

Thompson about specific fractions from Hanford neutron-irradiated americium we are to receive.

Eugene H. Huffman

Goeckermann talked about the bromine, barium, and yttrium fractions from the high energy neutron bombardment of bismuth (at the 184-inch cyclotron). He said the total activity in the uranium, also bombarded with neutrons at the 184-inch cyclotron, was only 30 times that of the bismuth, indicating that fission was due to neutrons rather than deuterons in the particular experimental setup used. Goeckermann mentioned that a bismuth target that he had placed directly in the deuteron beam had melted. There was a discussion of other targets to use.

Asprey told about his plans to try to make Am_2O_3. Then Street talked about his work with Hicks--they have tentatively assigned two platinum activities of 2.9 and 4.4 days to Pt^{191} and Pt^{193}. I said that it is important to work out this region since such activities are formed in bombardments of heavier elements on the 184-inch cyclotron.

Robinson described an x-ray crystal spectrometer of the Cauchois type, which he believes is the best design. There was a discussion of the strength needed for the source. James commented that there is a need for a good beta-ray spectrometer, and I suggested that it might be possible to develop one here.

Morgan discussed a milking experiment to determine the decay scheme of various americium isotopes. Asprey reported that Tompkins in Oak Ridge has made column separations of rare earth elements at rapid rates using a new Dow resin in a small column.

Magnusson described his plans for the study of Np(V). Finally, Howland talked about the odd mass neutron-deficient isotopes of bismuth, which are unknown; these are those with mass numbers 203, 205, and 207, probably long-lived, that were searched for in the bismuth fraction of an old helium-ion bombarded thallium target at the 60-inch cyclotron. I suggested that some of these samples, bismuth and lead fractions essentially inactive with a Geiger counter, might be checked with an alpha-particle counter.

* * * * *

At 10:10 a.m. I gave the Chem 123 lecture in Room 219, Gilman Hall. Today's topic was the radioactive decay series. I also collected the problem sets.

Up on the hill I had the abstract of a paper that we went to present at the Stanford meeting of the American Physical Society in July mailed to Professor Joseph Kaplan:

Transmutations with High Energy Deuterons and Helium Ions in the 184-inch Cyclotron

B. B. Cunningham, H. H. Hopkins, M. Lindner, D. R. Miller, P. R. O'Connor, I. Perlman, G. T. Seaborg, and R. C. Thompson

The successful operation of the Berkeley 184-inch cyclotron to produce useful beams of deuterons and helium ions of approximately 200 and 400 Mev, respectively, has made possible the radiochemical identification of many of the transmutation products of nuclei excited to very high energies. Products with considerably lower atomic number than the target were observed in all cases. For example, with 200 Mev deuterons on $_{33}As^{75}$, activities were found representative of all elements from $_{34}Se$ to $_{25}Mn$ with lower elements not excluded. Both negative beta particle and positron (or orbital electron capture) activities are noted, indicating reactions both of multiple neutron ejection and multiple charged particle ejection. For example with deuterons on $_{51}Sb^{121}$ and ^{123} both 6.7-hour $_{48}Cd^{107}$ and 2.5-day $_{48}Cd^{115}$ were produced and on $_{33}As^{75}$ cobalt activities tentatively assigned to $_{27}Co^{55}$ or $_{27}Co^{54}$ (~12-hour) and $_{27}Co^{61}$ (~1.8-hour) were found.

For the formation of light isotopes, several different reactions are possible since in most cases the products will not be shielded from formation through a chain of short-lived positron (or orbital electron capture) ancestors following the ejection of different combinations of neutrons and charged particles in the primary reaction. As an example, from deuterons on $_{29}Cu^{63}$ and ^{65}, the formation of a new 8-hour iron positron emitter assigned to $_{26}Fe^{52}$ may be represented by the expression, $Cu^{63}(d,4z13a)Fe^{52}$, which indicates that four units of charge and thirteen mass units are lost in arriving at the product. In the case of the heavier isotopes it may be possible to write somewhat more definite reactions of the type $Sb^{123}(d,2\alpha2n)Cd^{115}$ and $As^{75}(d,p3\alpha3n)Co^{61}$, although negative beta-particle emitting parents are certainly possible.

Uranium was bombarded with 400 Mev helium ions and a wide range of activities were identified both in the heavy element region and in the region of the fission products. Representative of the heavy isotopes observed were $_{88}Ra^{223}$(AcX), $_{88}Ra^{224}$(ThX), and $_{85}At^{211}$. The fission product distribution is characterized by a much shallower dip between the maxima than is the case for slow neutron fission, and the yields of the lightest observed fission products are much higher.

The various aspects of this work will be published more fully by the individual groups of investigators.

This paper is based on work performed under Contract No. W-7405-eng-48 with the Atomic Energy Commission in connection with the Radiation Laboratory, University of California.

This is a longer than usual abstract for a meeting, but it is such important work that I am certain it will be accepted.

I have been receiving a fair number of letters recently that I have had Mary Bender file in the "screwball" file. Today's letter arrived from an Albert A. Larson, stating, "...On the other hand perhaps another element will have been found which has the peculiarity of its motion, i.e.: vary in brightness from hour to hour, which occurs at regular intervals (over a period of five and a quarter hours) not unlike a pair of dumbbells revolving around, one being white the other dark, the dark one obscuring the white one at intervals of rotation..."

Later I went to the afternoon meeting of the Connick-Seaborg groups and then, in the evening, to the Research Conference.

Wednesday, May 14, 1947

After making arrangements with Iz for a round of golf later, I read and talked with the various men about their research this morning.

Later I wrote to Robert Sibley to tell him that I plan to take a one-pound cube of uranium to the Alumni Luncheon and will explain to the group how long this small piece of uranium can supply electric power to the city of Berkeley.

Iz's score was 101 and my score was 104 in this afternoon's round of golf at Mira Vista.

Thursday, May 15, 1947

My lecture today in Chemistry 123 was about chemical properties of the elements in the actinide series and the electronic structure of these elements.

James Fisk (Director of Research of the AEC) is visiting the laboratory this week, and I spent some time with him talking about our research. I gave him the summary of our program that I prepared on Monday.

Iz and I received an analysis from Dr. Earl R. Miller of an accident on April 25 in Room 110, in which a platinum capsule originally containing Hanford-irradiated americium (and removed in November 1946) was being re-examined and a splash occurred. Miller's calculations showed that the air tolerance was exceeded by 43.5 times. Miller emphasized the dangers inherent in our work.

At home Helen reported that she took Pete to Dr. McAlear for a check-up this morning; he seems to be developing normally. In the late afternoon we went to the home of Ernest and Molly Lawrence for a cocktail party in honor of James and Cynthia Fisk.

Friday, May 16, 1947

Much of my morning was spent showing Jim Fisk our various research projects; I introduced him to some of our graduate students.

In correspondence today, I agreed to Alden Emery's request in his letter of May 8 that I serve on the ACS temporary Advisory Committee on Publications.

I wrote to Joe Katz, suggesting that he visit the first week of July (as opposed to the week of June 23) for more men will be here. I asked that he send me a copy of Chapter 1 (the introductory chapter) of Volume 14A of the Plutonium Project Record (PPR) before he comes so that I may work on it a bit more. I also asked him to check with Hoylande Young about the clearance of our paper on the U^{233} decay products and our other pre-Project papers.

At home Helen told me that Jo Owen had visited in the afternoon.

Saturday, May 17, 1947

As usual, I spent the morning at the Radiation Laboratory.

A May 7 letter arrived in the mail from Walter Yust, acknowledging the receipt of my article, "RADIOACTIVITY, ARTIFICIAL," for the Encyclopaedia Britannica, and enclosing a check for $225 for my work on this assignment. He also included a copyright release, which I signed and returned.

I received and read a thank-you letter from H. B. Hass (Purdue University) for my assessments of Tolbert, Yankwich, and Katzin.

W. H Sullivan wrote, in a letter dated May 12 from the National Research Council, that the Manhattan Project Editorial Advisory Board recommended, at its meeting on April 18, that the Atomic Energy Commission publish his "Chart of Isotopes" as part of the Manhattan Project Technical Series. Sullivan said that the work on it is continuing and that Welch has said that his company is willing to consider publishing the chart in a form that he has proposed (trilinear format).

I read a letter from W. Albert Noyes, Jr. (President of the ACS), who explained that the general talk he asked me to give replaces the talk that I should give in connection with the medal I am to receive. He also said that the title of "The Eight New Elements" that I suggested is appropriate for the general meeting.

A note arrived from Leonard Katzin, thanking me for the carbons of my letters [my attempts to find an academic position for him]. Katzin said he will let me know if anything significant transpires.

Sunday, May 18, 1947

This was mostly a family day, but I also worked on my radio script for Warren Weaver.

Helen and I have had several conversations about our housing situation, which is barely adequate now and will certainly be inadequate when our new baby arrives later in the summer. This afternoon we spent several hours looking at some houses that are within our price range and

are presently on the market.

Later we, along with Pete and Jeanette, stopped to visit Len and Dagmar Dreher and five-year old Jerry. The Drehers are expecting another baby in a couple of months.

Monday, May 19, 1947

I made the rounds of the labs to check on the research and then sent George Everson a list of the full-time summer starting dates for our graduate students. [Our students are paid on a half-time basis during the school year, but they can be paid full time during the summer; however, the date varies because of vacation plans, etc.]

Name	Start full time
Asprey, L. B.	June 15
Eyring, LeRoy	June 15
Goeckermann, R.	July 1
Hopkins, H. H.	June 15
James, Ralph	July 1
La Chapelle, T. J.	June 23
Lindner, Manfred	June 15
Magnusson, L. B.	June 15
Miller, D. R.	June 15
Morgan, L. O.	June 15
O'Connor, P. R.	approx. July 1
Stewart, D. C.	June 23
Templeton, D. H.	June 26
Thompson, S. G.	June 20
Werner, L. B.	July 1

Tuesday, May 20, 1947

In attendance at today's group meeting were Asprey, Blaedel, Bockhop, Cunningham, Ghiorso, Hicks, Hopkins, Howland, Huffman, La Chapelle, Lilly, Lindner, Magnusson, McCullough, Morgan, Newton, Perlman, Reynolds, Robinson, Seaborg, Stewart, Street, Templeton, Thompson, Werner, and Wheelock. I made a few announcements--Joe Katz will be here early in July to confer with authors of PPR papers; the dates for our next meetings will be May 27, June 5, June 24; there will be no storeroom service between June 21 and June 30 because of inventory; and we want to know vacation schedules as soon as possible.

Morgan made a few remarks about some preliminary experiments in the equilibration of tracer solutions of americium and neptunium with Nalcite resin using citrate solutions of pH 2.62 and 3.37.

Ghiorso commented that, since the deuteron beam has been brought outside the 184-inch cyclotron, it may be possible to count fissions inside the beam in order to determine the total deuteron-induced fission cross section of some of the lower elements, such as bismuth. There was a discussion of the possibility and value of this. Perlman pointed out the need for this information to correlate with the yields obtained by chemical analysis; I emphasized the value of comparing the energy of

these fission pulses with those from neutron-induced fission.

Thompson then gave a list of calculated amounts of a couple dozen of isotopes expected from the bombardment of 4.48 mg of Am^{241} in the Hanford pile from April 12, 1946 to June 12, 1947. Thompson also described the chemistry he plans for the target. Major emphasis will be placed on the search for element 97, which will be carried by $BiPO_4$ as 97(IV) from oxidized solution. I mentioned that there is another sample in the same channel of the pile--60 mg of $Pu^{239,240}$.

There was then a discussion of the preparation and analysis (by Conway) of pure hafnium; we plan to try to separate zirconium and hafnium in a Nalcite column.

Asprey reported that he made a crude measurement of the solubility of AmF_3 as 0.15 mg Am/l in 0.2 M citrate, 0.6 M HNO_3, and 3 M HF, which is lower than the solubility of PuF_3 (although it may be Pu(IV) that is dissolved through a fluoride complex ion).

Stewart said that he has made an equilibrium experiment with 10^6 c/m of Cm^{242} in 10 ml of 10^{-4} M $HClO_4$ solution equilibrated with 100 mg of Nalcite. Of the 60% of the counts that went onto the resin, 95% could be taken off with 0.5 ml of citrate solution of pH 4.5. Thus a concentration of 20:1 was effected. One should expect that column runs for good separation should be possible at high flow rates although this has not been borne out in actual column runs.

Blaedel reported on his unsuccessful attempt to determine the melting point of thorium by the tungsten jaw method--thorium wet the tungsten. He plans additional runs, using clean tungsten.

* * * * *

The topic for today's Chem 123 lecture was "Radiation Chemistry." I also announced that the final midterm will be on May 29. After class I stopped to talk with Geoffrey Wilkinson before going to the hill.

An announcement of a meeting of the GAC Subcommittee on Research at 7:30 p.m. on May 29 in Washington, D. C., arrived from L. A. DuBridge (chairman).

Franz Schrader (Columbia University) wrote, in reply to my recent inquiry, to say they have no opening for Leonard Katzin.

Late in the afternoon I went to the meeting of the Seaborg-Connick groups, and in the evening I attended the College of Chemistry's weekly Research Conference.

Wednesday, May 21, 1947

After discussing some administrative matters with Iz, I took care of some of my mail.

I answered a May 14 letter from Donald A. McPherson (John Wiley). McPherson asked my opinion about the publication of a book containing a

table of isotopes, tables of masses, nuclear spins, magnetic moments, cross sections, and a summary for each element. In my reply I informed him that the Committee on Radioactivity of the National Research Council is planning to issue such tables although probably not in a single book. I suggested that he contact W. H Sullivan for more information. In addition, I mentioned that we shall probably use the "Table of Isotopes" in our book and publish it in Reviews of Modern Physics; I doubt, I said, that it will be finished in much less than a year. Finally, in response to his suggestion that we have lunch together some time when I am in New York, I mentioned that I shall be in New York at the time of the ACS meeting in September.

Cliff Garner wrote from UCLA on May 16 to suggest that, since the paper by M. L. Perlman and Seaborg on element 94 in nature was declassified, that we should try to declassify his paper with Norman Bonner and me on the presence of 94^{239} in carnotite and prepare a paper for publication in Journal of the American Chemical Society. In my reply I agreed with him but explained there may be some complications because of the ACS policy on publishing these early papers. I added that I have been appointed to an advisory committee to consider the publication policy of the ACS.

I accepted a May 12 invitation from John J. Cavanaugh (President of the University of Notre Dame, to be the 1947-48 Nieuwland Lecturer). I suggested the first week of December as a convenient time for me to speak since I am scheduled to be in Washington, D. C. the end of the preceding week.

Then, today, I also received a letter from Charles C. (Charlie) Price of Notre Dame, commenting on the invitation and saying that the audience will be primarily graduate and advanced undergraduate students, but they also look forward to informal discussions while I am on campus.

A complimentary photograph arrived from E. S. Morgan of Harris & Ewing, Photographers of National Notables in Washington. I sat for them during the time I was in Washington for the last GAC meeting at the end of March.

A note, approving of my idea to show the Alumni Luncheon attendees a cube of uranium, arrived today from Robert Sibley. He asked me to send him the exact notation of my Chemist of the Year Award so that he can use it in my introduction on June 21. I immediately wrote back and explained that I was voted, by an informal poll of chemists, as 1947 Chemist of the Year, but this year I am being given the 1947 Award in Pure Chemistry of the ACS, which is awarded to the man under 35 years of age, who has done the most significant research in pure chemistry.

I talked by phone with Oswald H. Greager about the possibility of employment of John Thomas and Edwin Zebroski at Hanford--they both want to be assured that they may conduct fundamental research. In response to the conversation, a telegram then arrived from General Electric stating that Thomas and Zebroski may do research as fundamental as their talents permit at Hanford.

A note then went to Fred Albaugh, saying that I understand that he is

accepting the Hanford job, and that Stan Thompson and I plan to spend about a week--June 14 through June 19--in Southern California, golfing. If he has not yet left for Hanford, perhaps we can get together.

Helen and I have been negotiating with the seller of a house we saw on Sunday. Our offer of $12,500, including furniture, was accepted by the owners, John G. and Linza M. Stevens, today; and we are now owners of an old, roomy, two-story house at 2808 Ellsworth Street (between Stuart and Oregon Streets) in Berkeley.

2808 Ellsworth St., Berkeley

Thursday, May 22, 1947

Today's lecture for Chem 123, given by Iz Perlman, was a discussion of the practical applications of atomic energy, and included the topic of power piles. Another problem set was assigned.

I received and read Amos Newton's "Report of the Berkeley Chemistry Group for the Period of March 1 to May 1, 1947," which he prepared for his employer, C. E. K. Mees of Eastman Kodak. In addition to work of other groups of the Radiation Laboratory, Newton described our studies of the fission of heavy nuclei with helium ions and deuterons from the 184-inch cyclotron. He said all elements above tantalum are fissioned by 400 Mev helium ions and all elements above thallium are fissioned by 200 Mev helium ions. Bismuth and heavier elements are fissioned by neutrons from the 184-inch cyclotron. Newton noted that we are presently studying the distribution of fission products of bismuth, lead, and thallium in fission with high energy particles; present indications are that for elements near lead and bismuth, fission is symmetric (single peak in fission product distribution curve) in contrast with the asymmetric curve

of slow-neutron induced fission of uranium. The dip between the peaks for this high energy fission of uranium is not so low as in normal slow-neutron induced uranium fission.

I received from Roland A. Anderson (Deputy Advisor on Patent Matters, AEC) an Assignment of Invention form for Case S-52B (a section of the chemical part of the plutonium discovery patents), which Anderson asked me to execute and return to him.

Professor Gibson and I played the last nine holes at Mira Vista this afternoon--GEG-49, GTS-59.

During the evening Helen and I went to see "Blue Skies" with Bing Crosby and Fred Astaire; this was playing at the Oaks Theater.

Friday, May 23, 1947

I received the material on thorium and uranium from M. C. Sneed (Comprehensive Inorganic Chemistry) that I requested on March 8 for use in writing a chapter on the actinide elements for his volume. Sneed apologized for the delay in sending me the material and agreed with my approach to writing the chapter.

I mailed my typed radio script (1,350 words) to Warren Weaver. This is to be possibly used for intermission broadcast at one of the Philharmonic-Symphony programs, which are sponsored by U. S. Rubber.

In response to his request of May 4, I mailed John R. Kuebler (Grand Recorder, Alpha Chi Sigma) a photograph and a brief write-up about the ACS Award in Pure Chemistry for use in the Hexagon. I said, in part:

> Alpha Chi Sigma is performing a genuine service to chemistry in its backing of this award and I believe that it can be proud that it is furthering in this way one of its principal aims--to strive for the advancement of chemistry both as a science and as a profession.

Alice Thompson, along with her young daughters Ruth and Joyce, stopped by our home in the afternoon to visit with Helen.

Saturday, May 24, 1947

As usual I spent much of the morning at the laboratory, conferring with some of the men about their research and then trying to catch up on my report reading. Stan Thompson and I lunched together at Larry Blake's.

At home I admired Peter's first haircut, which Helen gave him today.

Sunday, May 25, 1947

I was unable to find a golf partner this morning, so I went out to Mira Vista alone and played nine holes (53) with some fellows named Frank (39), Chuck (44), Larry (60). Frank and I then continued for the additional nine holes: Frank 82, GTS 104.

Later I spent some time looking over the material I received from

Sneed on Friday.

Monday, May 26, 1947

My correspondence took up considerable time this morning. I answered a May 5 letter from W. W. Hawes (Acting Technical Director, Radiation Laboratory, San Francisco), who asked for an evaluation of Alan A. Jarrett (colleague in my Section C-I at the Met Lab). In my reply today I reported that, although there were better men than Jarrett working on the Manhattan Project, he is a good man and has the kind of experience in which Hawes is interested.

In response to a May 16 request from C. C. Todd (State College of Washington), I wrote an evaluation of Herbert H. Anderson, saying that Anderson has a great deal of experience in the synthesis and identification of new inorganic compounds, but he is a lone wolf type of worker, which I believe is caused by an innate shyness. I suggested that a personal interview would be appropriate.

A reply to my query about the status of the 4n + 1 paper arrived from Leonard Katzin, who wrote that it has been held up because of misinformation on the clearance of the Canadian papers but it is now being submitted to the Responsible Reviewer; this probably means a month or so before it is cleared.

I recently submitted a rather long abstract of the work of our group, using the reactions of the 184-inch cyclotron, to Joe Kaplan for the July meeting of the American Physical Society that will be held at Stanford ["Transmutations with High Energy Deuterons and Helium Ions in the 184-inch Cyclotron" by B. B. Cunningham, H. H. Hopkins, M. Lindner, D. R. Miller, P. R. O'Connor, I. Perlman, G. T. Seaborg, and R. C. Thompson]. Today I received a carbon of a letter to Kaplan from K. K. Darrow (Secretary, American Physical Society), which reads in part:

> Well done! Only, you have not yet cultivated the ruthlessness necessary in a Secretary, and so well exemplified by R. T. Birge. I wish that the 400-word abstracts of Serber and of the Seaborg coterie might have been sent to your illustrious predecessor. Le Conte Hall would have rocked on its foundations, the windows would have splattered onto the grass, and the ears of the gentlemen in question would have been badly blistered. Your idea of printing a 400-word abstract under two consecutive numbers is ingenious, but would involve you and me in endless pleas for a repetition of the favor, and would cause repercussions in the Council. Since you did not have the nerve to wield the axe, I did. I sliced the abstract from the Seaborg group into two, one containing the results obtained with deuterons and the other, those pertaining to helium ions. Since they were of unequal length and the former overran 200 words considerably, I had to cut some words completely out (as I also did to several other abstracts, including <u>both</u> of Serber's). By carbon copy of this letter to Glenn Seaborg I am acquainting him with these facts, asking him tartly how he could think us so soft as to permit a 400-word abstract, and telling him that he owes me two drinks for retyping the abstract in question in the form of two abstracts.

Late this afternoon Ralph James informed me that he passed his prelims today. His committee consisted of William F. Giauque, Gerald E. K. Branch, Robert E. Connick, Axel R. Olson, Edwin M. McMillan, and Lauriston C. Marshall.

Tuesday, May 27, 1947

In attendance at this morning's meeting were Asprey, Blaedel, Bockhop, Conway, Covey, Cunningham, Fries, Garden, Goeckermann, Ghiorso, Hicks, Hopkins, Howland, Huffman, James, La Chapelle, Lilly, Lindner, Magnusson, Miller, Morgan, McCullough, Newton, Perlman, Robinson, Seaborg, Stewart, Street, Templeton, Thompson, and Werner. Goeckermann reported on the fission yields in the high energy neutron bombardment of uranium, bismuth, and lead (this was a repeat experiment). He intends to have a long bismuth plus deuterons bombardment in order to get accurate fission yield data.

Ghiorso talked extensively on the feasibility of counting fission fragments in or near a deuteron beam, needed in order to calculate absolute fission yields.

Thompson then reported on the plans for the Hanford-irradiated americium sample soon expected. He mentioned that Garden is testing methods of opening the sample and then talked about the chemistry planned for the sample. He included a discussion of the fraction that might contain element 97, to be isolated by coprecipitation of 97(IV) in $BiPO_4$ from oxidizing solution; plans were also made to isolate any element 98 that might be present--it should either elute ahead of curium or be in the Np-Pu fraction. Werner will be given the curium fraction that should contain approximately 200 micrograms of curium. Werner than talked about the curium fraction from this irradiated americium and the experiments planned for the curium fraction: (1) better absorption spectrum, (2) sample for Conway to use in determining the emission spectrum and the impurities, (3) specific activity determination made gravimetrically as Cm_2O_3, (4) x-ray diffraction pattern of the oxide, (5) solubilities of some curium compounds.

La Chapelle described some equipment that has been constructed for the preparation of solid Np(V) compounds. Finally, Blaedel talked about his determinations of the melting point of thorium metal; he obtained values of 1675-1695°C and 1685-1705°C.

* * * * *

Today's lecture for Chem 123 was devoted to the applications of radioactive tracers to chemistry. Another problem set was assigned.

I took the limousine from downtown Oakland to the Oakland airport, where I caught a flight to Los Angeles. In Los Angeles I transferred to American Flight 410 at 5:15 p.m. bound for Washington, D. C.

[In Berkeley, this afternoon, Manfred Lindner took and passed his prelims. His committee consisted of William F. Giauque, Kenneth S. Pitzer, Lauriston C. Marshall, Edwin M. McMillan (absent), William G. Dauben, and Bruno H. Zimm.]

Wednesday, May 28, 1947

In Washington. When my flight arrived in Washington this morning, I took the airport limousine to the Hotel Statler, checked in, and made a few phone calls.

In the afternoon Jim Crowe and Bob Gould played golf with me at the Rock Creek Park golf course. Later Jim and I had dinner together.

[In Berkeley. Helen's mother arrived to stay with her while I am out of town.

Mary Bender sent L. Fitzgerald a list of people who will be able to help with the inventory in Building 4.

Room No.	Name
102	Mary Bender and Lorraine Petch
103	Jeanne Wheelock and Ellen Jane Grahlman
103C	Roger Dorr
106A	Carol Leon
106B	Bill Hail
107	J. J. Howland
109	L. O. Morgan
110	A. S. Newton
201	T. J. La Chapelle
202	Winifred Heppler
203A and E	R. C. Lilly
203B	Horace Hopkins
203C	Don Stewart
203D	Jane Boggs
203F	Bob Goeckermann (after June 26)]

Thursday, May 29, 1947

In Washington. I took a taxi from my hotel to the Atomic Energy Commission Building (corner Constitution Avenue and 19th Street) in order to attend the 9:30 a.m. GAC meeting of the Material Balance Subcommittee in Room 213 (chaired by Cyril Smith).

Much of my afternoon was spent talking with the people in the Research Division of the AEC.

After dinner at the Statler Hotel, I again took a cab to the AEC Building for a 7:30 p.m. GAC Research Subcommittee meeting, chaired by Lee DuBridge. Among the topics covered were: 1. What is the priority among various areas of research in which the Commission should be interested? 2. What should be the attitude and degree of support of the Commission toward general research in nuclear physics not specifically connected with fission processes? 3. What types of research should be encouraged and supported in the various Commission laboratories, such as Clinton, Brookhaven, Radiation Laboratory, etc.? 4. What general types of work should the Commission encourage and support in university and

industrial laboratories? 5. How should the Commission eventually divide areas of interest with the Office of Naval Research or with the Science Foundation? 6. How can the Commission handle problems of classification and publication of the results of research?

[In Berkeley, Iz gave the following midterm to the Chem 123 class:]

Chemistry 123
Midterm Examination

May 27, 1947

1. (20)

Accepting the actinide theory for the structure of the heavy elements, predict the chemical properties in terms of principal valence states for the following:

(10) (a) Element 104
(5) (b) Element 97
(5) (c) Element 108 (comparison with element of similar properties acceptable)

2. (30)

(15) (a) The city of Berkeley consumes power at the average rate of 35,000 KW. How much plutonium would be used in a power plant in one year if the electrical energy could be generated and distributed with 35% efficiency?

(10) (b) Explain the differences in mechanism of heat production in a nuclear power plant and coal power plant.

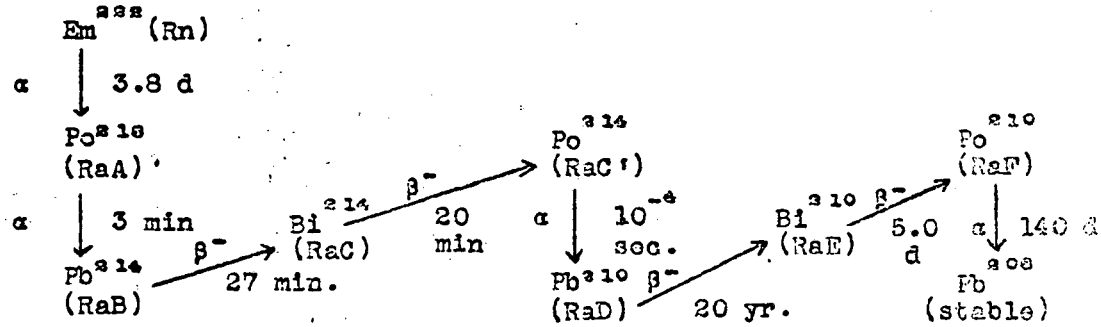
(5) (c) Give reasons why a nuclear engine may be of particular interest for submarine operation.

3. (25)

One millicurie of radon, Em^{222}, was pumped into a tube, sealed and allowed to decay for one month. The tube was then opened and the α-activity measured.

(10) (a) Draw a rough qualitative curve of the logarithm of α-activity vs. time in years.

(15) (b) How much α-activity would be present after 40 years? (Indicate activity in millicuries or disintegrations per minute.)



4. (25)

(15) Calculate the range in air of the Po^{210} α-particle (5.3 Mev) if the average number of ion pairs formed per millimeter over the entire range is 4000.

(10) Using the exact energy of the Po^{210} α-particle as 5.298 Mev and assuming no gamma-rays, calculate the total energy for this disintegration, i.e., the energy that would be measured in a calorimetric experiment (disintegration energy).

Friday, May 30, 1947

In Washington. Again I took a cab to the AEC Building in order to attend the 10 a.m. GAC meeting. All of the members were present: Conant, DuBridge, Rowe, Fermi, Worthington, Smith, Rabi, Oppenheimer, and I. After a short discussion of the agenda, Oppenheimer asked that the report from the Atomic Energy Commission be considered first. Then Commissioners Lilienthal, Bacher, Waymack, and Pike, along with Lyle Belsley (Assistant General Manager), Jim Fisk (Director, Division of Research), Kenneth Fields (Military Application Division), and Carroll Wilson (General Manager) joined the meeting. There was some discussion of a central laboratory for the development of nuclear power reactors, and the GAC was asked to consider this further. Wilson reported that the Commission is actively looking for a successor to Monsanto for the operation of the laboratories at Clinton. Fisk then talked about the problems of General Electric-Hanford, the stack gas problem, the basic research at Schenectady.

Fisk said that the General Electric is in receipt of a letter authorizing two new piles in associated areas at Hanford. Discussions with General Electric have resulted in the revision of the time scale on the Redox Process, which lengthens it to three and one-third years for the completion of construction of a plant to handle the output from one pile. There has been no estimate of the dates for completion of the piles; this led Oppenheimer to remark that we should not expect any change in the plutonium-U^{235} ratio for the Los Alamos program for the next four years.

Colonel Fields reviewed the situation with regard to Los Alamos, saying that parity between the production of weapons material parts and high explosives should be reached by October 1 and that Monsanto's Dayton plant will start producing Po^{210} initiators on or about July 1, which should put this item in balance with the other ingredients of weapons. Carroll Tyler has been appointed the new Area Manager for Los Alamos, he said. Oppenheimer asked about the testing of nuclear weapons, and Fields said that this hasn't been resolved. Lilienthal commented that the matter has been discussed with President Truman and the Secretaries of War and Navy, in general terms. Wilson then announced that Argonne National Laboratory now has acquired all but 80 acres for its site and a contract has been signed with the University of Chicago for the operation of Argonne.

A question by me resulted in my appointment, along with Worthington and Conant, to a committee to look into the time scale for the introduction of the Redox Process at Hanford. Wilson also told about the appointment of an advisory board, consisting of Leonard B. Loeb, Alan Gregg, Herbert S. Gasser, A. Baird-Hastings, Detlev W. Bronk, Ernest W. Goodpasture, and Wallace O. Fenn, to review the research in health and biology. We were also told by Waymack and Lilienthal about plans for the creation of an appeal board to review clearances of personnel.

The remainder of the items on the agenda were about more technical matters, so the Commission staff, except Bacher and Fisk, left and AEC General Consul Herbert S. Marks joined us to describe the aspects of foreign distribution of tracer isotopes and the employment of foreign

scientists. We concluded that the distribution of isotopes should be carried out but that employment of aliens might in some cases, such as in programmatic work, be inadvisable. We adjourned at 12:45 p.m.

I found a few minutes to speak with Jim Fisk about some motor generators that Professor Giauque asked me to try to get allocated to him for his work. Giauque has learned that a 3500 kw set is in standby status at Y-12, and he would like to have it.

The GAC, along with Manley, reconvened at 2 p.m. We adopted the report DuBridge prepared for the Subcommittee on Research. One point we agreed upon was to recommend that the Commission try to work out an arrangement with the Office of Naval Research for the Commission to take over the role of support of basic science, but only after it is assured that the high standards be maintained. We discussed and recommended that the Commission exert every effort to separate classified and unclassified work. Further, we recommended that non-programmatic research be distributed widely in location, avoiding large institutions but noting that the University of California will be an exception because of its special history. Finally, DuBridge moved that the GAC recommend the establishment of a special panel to look into the problem of the extraction of uranium from raw materials. Cyril Smith will assist Wilson with such a panel. We adjourned at 5:30 p.m.

After dinner the GAC came back for an evening session, beginning at 8:30 p.m. Fermi made a preliminary report on behalf of the Subcommittee on Material Balance, emphasizing that definite recommendations cannot be made until decisions are made on the number of weapons desired since this use places emphasis on plutonium whereas the nuclear power industry would place emphasis on U^{235} as fuel. Conant emphasized that another factor is the question of the Pu^{240} tolerance and its effect on weapons efficiency. Oppenheimer proposed that, because of the tightness of the raw materials, the concentration of Pu^{240} should be increased as far as technically feasible; there was no objection to this statement. Despite its incompleteness, the GAC decided to transmit the draft report of the Subcommittee on Material Balance to the Commission as a working paper. We adjourned at 9:45 p.m.

Saturday, May 31, 1947

In Washington. The GAC convened at 9:45 a.m. with all members and John Manley present. Oppenheimer presented a detailed agenda for consideration: (1) foreign isotope distribution and employment of foreign scientists, (2) declassification, (3) problems of public information, (4) an appeal board for security clearances, (5) reactor laboratories and the high-flux reactor location, (6) reactor hazards, (7) Redox and the Chelate Process, (8) the relation between the Commission, scientists, and science. On item (1) we decided to recommend the policy of foreign distribution of isotopes and that foreign scientists could not generally be properly employed on classified work; but in classified locations (working on unclassified material), a case by case examination should be made. There is no restriction on unclassified work in unclassified locations. We concluded on item (2) that, for all elements with atomic number 90 and above, classification be retained on the

behavior when bombarded with neutrons, spontaneous fission, and the number and energy of neutrons emitted per fission. With respect to item (3) (public information), we decided to recommend that (a) the Commission, by appropriate announcements, usher into public life those discoveries which ought, from their nature, be made public, (b) that it have prepared reports on the usefulness of the work it is undertaking, and (c) that it have written, for public release, a realistic evaluation of the possibilities of atomic power. With respect to item (4) (an appeal board), we decided to recommend to the Commission the establishment of such a board to aid the Commission in determining whether the background of a prospective employee would prejudice the National defense and security. We adjourned (12:30 p.m.) after a short discussion of the location of the high-flux reactor (item 5).

At 2 p.m. the GAC and Manley reconvened, but we had no agreement that building the high-flux reactor at Clinton Laboratories is wise. Rabi suggested that this reactor be built at Brookhaven while Conant suggested the Argonne Laboratory location. We did not reach a conclusion. We decided to recommend the formation of a committee of experts to consider item 6 (reactor hazards): Dr. Richard Feynman, Colonel Kenneth D. Nichols, Dr. Lombard Squires, Dr. Edward Teller, and Dr. John Wheeler.

After a short intermission, we met with Pike, Strauss, Waymack, Lilienthal, Bacher, Belsley, and Fisk. At this time Oppenheimer made his usual masterful summary of the progress and recommendations of the General Advisory Committee at this meeting. [I later saw the verbatim transcript of this extemporaneous summary; this ran 33 pages.] We then had another intermission, while the Commissioners and staff left, then continued talking at 5 p.m. on item (7) (the Redox and Chelate processes). I described the Chelate Process, its advantages and disadvantages; I pointed out that, because it is two or three years behind the Redox Process in development, we have no choice but to go ahead with the Redox Process at Hanford. Worthington and I were asked to prepare a note for the Commission about methods for shortening the time scale for developing the Redox Process.

[In Berkeley, Helen, Grandma Griggs, and Aunt Jeanette helped Peter celebrate his first birthday. In addition to receiving some presents--cloth books from his parents; rattle, sun suit, and tee shirt from Grandma Griggs; two tee shirts, knit overalls, and short suit from Grandma and Grandpa Seaborg; sand bucket and shovel from Aunt Jeanette; a record from the John Lewellens; a bath rattle from Aunt Josephine Owen; Brier Fox doll from Lois and Tommy Russell; and cards from Pete and Greg de Gabriele, the Sundlies, and the Osborne girls--Pete had a birthday cake (with one candle), into which he promptly stuck his hand.]

Sunday, June 1, 1947

In Washington. I again attended the GAC meeting, which began at 9:50 a.m. Fermi, Conant, and Rowe were absent. We spent the time in informal discussion and in drafting the various documents that we had agreed to do. Our formal meeting adjourned at 10:30 a.m. The following is Oppenheimer's report of this meeting, addressed to David Lilienthal:

[redacted] FILE COPY June 1, 1947

AEC Doc 51

Mr. David Lilienthal
United States Atomic Energy Commission
1901 Constitution Avenue
Public Health Building
Washington, D. C.

Dear Mr. Lilienthal:
No.

The Fourth Meeting of the General Advisory Committee took place on May 30, May 31, and June 1, 1947. This letter will transmit to you such views as we were able to formulate on matters referred to us by the Commission and its staff, and on one or two matters which seemed to us sufficiently important to warrant consideration. This letter will be supplemented by the reports of the Sub-Committees on Weapons, on Material Balance, and on Research, by a draft statement of the reasons for establishing a distribution of radio-isotopes in foreign countries and by a note on the Redox time scale. It can further be supplemented by the verbatim transcript of the afternoon meeting of May 31, at which the Committee met with the Commission:

1. Material Balance. The report of this Sub-Committee should be regarded as a preliminary study. The purpose of the study is to see by what schemes of interrelated isotope separation and reactor operation one may best use a limited supply of raw materials to achieve a stockpile of nuclear fuel. In all such schemes economy demands that material which has been used in a Hanford reactor be recovered and enriched by the use of a diffusion process. According to present knowledge the cheapest method of attaining a stockpile for conversion and breeding purposes would be to pass the material through the Hanford plant only once, and not to increase the capacity of that plant. By a further expenditure of funds but without jeopardizing the effectiveness of the material for power and breeding purposes the stockpile of atomic weapons can effectively be multiplied by three, by increasing the scale of the Hanford plant to 10 reactors and by returning re-enriched material to the reactor program. In order to make a detailed flow-sheet or to evaluate the merits of alternative schemes it will be necessary to make a study of the cost, effort, and time for the construction of an additional plant. We recommend to the Commission that such information be developed; we should be glad to continue our work when it is made available to us.

   Since in any event some time must elapse before Hanford material can be recovered in useable form, we recommend that the project for increasing the concentration of plutonium be expedited and that the tolerance limit of plutonium 240 be set as high as 500 grams of plutonium per ton [redacted] uranium. This is to be understood as an interim [redacted] pending the development and installation of Redox. [redacted]

   Raw Materials. Part of the responsibility of the Advisory Committee pertains to raw materials. Three members of the Committee have participated in discussions of the problem of the recovery from low-grade ores. We have asked Dr. Cyril Smith to consult with the General Manager of the

Commission to set up a panel including several members of the Advisory Committee and others, not members of the Advisory Committee, who are expert in the geological, mining, ore treatment, and chemical engineering problems involved. This panel should be available directly to advise the Commission and its staff, and should help to keep the Advisory Committee informed.

3. Explosive Reactors. The Atomic Energy Commission has referred to us the question of the hazards attendant upon constructing and operating reactors near centers of population. No general answer to this question can have value. Each reactor must be examined specifically and in detail with regard to such widely varying points as thermal stability, the disposal of radioactive gases, and mechanical stability. In order that the Commission have available the continuing advice of qualified experts, we recommend the creation of a panel to include at least one man versed in mechanical engineering, chemical engineering, pile design, and atomic explosives. For the convenience of the Commission we list what would thus appear to be an appropriate panel:

Dr. Richard Feynman
Col. K. D. Nichols
Dr. Lombard Squires
Dr. Edward Teller
Dr. John Wheeler

4. The Commission has referred to us the question of the extent to which, and the forms in which, it should support basic research. It is recognized that much basic research will rightly and inevitably be conducted by contractors working on the main problems before the Commission. It is also recognized that much basic research, though perhaps of non-secret nature, would involve secret facilities such as reactors. For reasons which have been outlined in our earlier reports, some measure of centralization of such activities is inevitable, though we should even here like to recommend the establishment of small reactor facilities wherever the regional need for them warrants. In this paragraph we shall, however, be concerned with basic research carried out with unclassified facilities and in an unclassified area.

a. We strongly recommend to the Atomic Energy Commission that it give support to non-secret basic research in nuclear science.

b. We recommend that such support be made available primarily, but not exclusively, to qualified groups in universities as an aid to scientists and to science.

c. There are a number of criteria by which the Commission should, in our opinion, evaluate the desirability of supporting a project. Beyond the obvious criteria of the competence of the scientists and the reasonableness of the project we should lay particular emphasis on the following points:

1. The scientific staff for the project should pre-exist at the location of the work. It should not be necessary to recruit scientists from other branches of the atomic energy effort or other universities.

2. The project should be a proper training ground for graduate students; the work of the students should be that of scientists or fellows, and not of workmen and technicians.

3. The administration of the project should not disrupt the equilibrium of effort in the academic institution in question.

4. Projects should be given especially favorable consideration in which there is some local or non-governmental support, in which there is a past record of achievement, and in which the Commission is not asked to initiate wholly new enterprises.

d. We would further recommend a wide dispersal for such non-secret activities, since history has shown that they profit relatively little from concentration.

e. We would strongly recommend that the support of the Atomic Energy Commission for such basic work be given in the form of a grant-in-aid rather than by a specific contract, and that, if a contract proves to be necessary, its terms leave flexible the precise contours of the work that is to be undertaken.

f. For the motivation of these recommendations, and for further details on our views regarding them, reference is made to the attached Sub-Committee report on basic research.

5. As an example of basic research of a non-secret nature which should be of interest to the Commission, since it is in the field of nuclear science, we think of the contracts of the Office of Naval Research in the construction of electronuclear accelerators. Generally speaking, the Advisory Committee look with favor on a policy of the Atomic Energy Commission of taking over from the Office of Naval Research, projects in basic nuclear science which the Office of Naval Research is willing or eager to relinquish, and which after proper review appear sound. Specifically, we make this recommendation of the accelerator projects referred to us. We believe it appropriate that the Atomic Energy Commission, which is by law directed to further understanding in the field of nuclear physics, assume the responsibility for the support of this work. Nevertheless, we must make one reservation. The Office of Naval Research has by its contract policy, by its administration, by its liberal terms, and by the absence of excessive paper work, done a highly commendable job. We believe that the Atomic Energy Commission should not undertake to administer these contracts until it is in a position to do an equally good one. We see no objection to an interim arrangement for financing such projects under the caretaker supervision of the Office of Naval Research. In the long term we believe this arrangement would be unacceptable.

6. As to the scope of the support for decentralized unclassified basic research in nuclear science which may be appropriate for the Commission, we have no fixed and clear answer, nor do we believe that such an answer can be given without a study of the concrete proposals. We would, however, point out that with an expenditure in the last fiscal year of $10,000,000 and a proposed expenditure for the coming fiscal year of $6,000,000, the Office of Naval Research has gone far toward answering the need for this sort of support. We are, therefore, inclined to the view that a sum not far in excess of $10,000,000 should suffice.

7. In many of the Commission's large laboratories much of the work is by nature non-secret. This applies to almost all of the work carried out with the big accelerators: to all of it, that is, except when the targets are themselves classified materials. It applies to much of the work carried with neutrons from reactors. We would like to propose to the Commission, and strongly advocate, two steps to aid in the disentangling of the non-secret and the secret. The first of these is to define areas of work (such, for example, as the use of electronuclear machines with non-classified targets) in which information obtained is <u>not only declassifiable but does not require declassification</u>: in which it is completely open: in which the question of secrecy does not occur. Such work could be discussed freely even during the period when it was in progress and before a document had been written up and submitted for declassification.

In many laboratories a great part, if not all, of the work is of non-secret nature and involves non-secret facilities. In these cases we believe it most desirable to set up <u>non-secret projects</u> either as a whole or as a part of the work undertaken by the contractor. For these non-secret projects there would appear to be no need for security clearance, and no proscription against the employment of foreigners. Access would be limited, if at all, only for administrative convenience.

We are aware of the dangers that may come from the proximity of secret and non-secret work. We cannot overemphasize the advantages which would accrue from establishing non-secret laboratories, even at the cost of transporting, removing, fencing off, or otherwise dealing with secret facilities which may in the past have been associated with them.

8. We heartily concur in the proposal of the Commission to distribute certain radio-isotopes abroad. To the many reasons well known to the Commission for taking this step, we would add that it appears to have a great effect in restoring the confidence of scientists, and educated men generally, in foreign countries, in their colleagues in the United States. We attach great importance to this objective.

We have, however, a few differences in detail from the proposal made by the Commission. We believe that the restriction of the use of these isotopes to therapy and medical research is needlessly and unfortunately narrow. We believe that their use could well be broadened to include all basic research, and should surely be broadened to include basic research

in biological science. With the safeguards of publication and openness, we see no added danger in permitting this wider field of application, and we anticipate not only good will but good science to result from the broader policy. We concur in the suggestion that laboratories using isotopes obtained from the Atomic Energy Commission should permit access to American scientists. We believe, however, that the phrase "duly accredited by the United States" should be deleted, and that in its place there should stand "irrespective of nationality." We further believe that this practice of admitting scientists, irrespective of nationality, should be recognized as traditional and as prevailing, by and large, in comparable laboratories in the United States. The reasons for these changes are that we desire to see these laboratories open, and desire to see visitors from the United States treated as scientists and colleagues, and not as agents of their government.

We should like at a later date to return to the question of the extension of the list of available isotopes to include stable isotopes and fission products. At the moment we are concerned that there be a prompt start. We should again, however, wish to emphasize that the omission of hydrogen 3, Tritium, both from domestic out-of-project distribution and from foreign distribution, coupled with the publication of data indicating the existence of large amounts of this isotope, is very bad security indeed. Finally, we are attaching a draft statement which we hope may prove useful in explaining publicly the reasons for the foreign distribution of isotopes.

9. We have examined the document submitted on questions of declassification. We believe that this subject will concern us very often in the future. We do not believe that the Commission should make any permanent commitment to abide by the philosophy of the Committee on Declassification, but would heartily concur in accepting these, with appropriate revisions mentioned below, as an <u>interim</u> policy of the Commission. The reservations are precisely those outlined in H. A. Fidler's paper. We concur in his redefinition of the aspects of the nuclear physics of the elements of high atomic number which should be kept secret, but for precision would formulate them as follows:

> "The behavior of materials of atomic number 90 or above under neutron bombardment, the spontaneous fission of such materials, and the number and energy of neutrons emitted in fission by such materials, shall be maintained secret."

We also concur with the finding that the declassification of small reactors would not be consistent with present security policy on the fission properties of heavy elements, and that pending a closer examination of this latter policy, small reactors cannot be declassified.

With regard to the question of the concerting of our policy with that of the United Kingdom and Canada, we would strongly recommend that an attempt at such concert be made on a continuing basis; but that, among other reasons because we cannot always give the full grounds for our decisions, no binding requirement of concerted action be entered into.

10. The Commission has referred to us the question of establishing boards of appeal which may, upon the request of the individual concerned, hear his evidence and his witnesses, and advise the Commission in the event of his employment having been found inimical to the common defense and security. We most strongly concur in this proposal since it represents a step heartily desired by the great majority of our colleagues, and especially urged by the directors of the great laboratories under the Commission's auspices.

11. The Commission has again asked us to examine the question of the location of the high flux reactor and the related question of a main or central laboratory for the development of reactors. We are reluctant to accept a permanent division of the work between Clinton and Argonne; but we feel ill-qualified to advocate further the desirability of establishing a central laboratory at the latter site in the light of the difficulties encountered by the Commission in planning for this. It would be our general view that if such a unification were to cost no more than a delay in time of a year or so in the construction of the high flux reactor, and were in the long term to have no adverse effect upon the highly qualified personnel who have in the past worked on these problems at Clinton, we should recommend accepting the postponement in the interests of greater future effectiveness. In anticipation of the possibility that the damaging effects of transferring the high flux reactor to the Argonne may be even more serious than a year's delay and the consequent likelihood that the Commission may wish to establish it at Clinton, we strongly recommend the closest possible technical liaison between Argonne and Clinton, to the end that they may be regarded as the two divisions of a single project. We have reason to believe that even now in the fields of chemistry and metallurgy such close technical consultation and planning would eliminate harmful duplication of facilities and understaffing of effort. It is true that there should be good technical coordination between these laboratories and others concerned with reactor program such as Knolls, Hanford, and Los Alamos. Nevertheless a much closer degree of coordination appears to be necessary between Argonne and Clinton, and this should not be sacrificed in the interests of a more uniform treatment of the various laboratories.

We have considered the possibility of locating the high flux reactor at Brookhaven, under separate contract with the M.I.T. Arguments in favor of this were presented to by Dr. Rabi, with some concurrence from Dr. Conant. This step, in the view of the majority of the Committee, does not contribute toward the establishment of a central reactor laboratory, and should be considered only as an alternative in the event that neither the Argonne nor Clinton proves to be a possible site.

12. We would like to bring to the attention of the Commission certain matters of public information. The first of these is the desirability of making available grants-in-aid for the preparation of distinguished non-secret monographs on basic nuclear sciences. These are sadly wanting, and could contribute profoundly to the welfare of the science basic to the work of the Commission.

Of more direct concern to the Commission are three matters of public information toward which the members of the General Advisory Committee would be glad to contribute, to the extent that their qualifications permit.

a. We recommend to the Commission the issuance of public reports of the progress of the non-secret researches carried out with the aid of its support or its facilities. Important examples lie in the field of photosynthesis, and in the study of the magnetic moments of neutron and triton. The purpose of these reports would be to give a broader general appreciation of the constructive work of the Commission in support of science, and a better appreciation of the nature of the progress that was being made.

b. There may be specific cases, and some have arisen in the past, of findings made by scientists working under contract with the Commission, findings which are not secret and which in due course will be declassified, yet findings which, unless correctly understood, would appear to involve matters of very high classification. An example, and we believe not an unique example, is the discovery of the fissionability of common elements, such as lead, under bombardment by deuterons, neutrons, and alpha particles of high energy. These findings are already widely known and will in due course be declassified and should properly be declassified. They will appear of inflammatory importance to the public, who will tend to suppose that hereby new avenues of making atomic explosives have been opened up. The Commission should put out, as an official document, an explanation of what these findings mean, and why they are not treated as secret.

c. Even more important and even more difficult, in our opinion, is the official publication by the Commission of a candid, non-secret, honest account of the prospects of industrial power from atomic energy. We believe that the widespread misunderstanding of the prospects in this field can have disastrous consequences unless they are corrected promptly, and by a source unquestionably in possession of the facts.

It will be noted that many of the matters discussed above are not technical. They are matters affecting the relations between the Commission and the scientists of the country. We believe that much of the future health of work in atomic energy rests on the successful development of these relations, and that the Commission might be well advised to give prompt attention to their cultivation.

J. R. Oppenheimer

Monday, June 2, 1947

In Washington. I spent much of the day at the AEC headquarters. I placed a phone call to Charlie Price at Notre Dame but couldn't reach him--so I left him a message saying that, because of the dates of the November GAC meeting, it is necessary that I change the date of my Nieuwland Lectures to the week of November 17.

Later I took a taxi from the Hotel Statler to the airport to catch a flight home.

Tuesday, June 3, 1947

When I arrived in Oakland, I took a limousine from the airport to downtown Oakland; a lab driver met me and drove me to the lab. There I looked over my mail while Iz gave the Chem 123 lecture--he returned and explained the midterm and then talked about the use of tracers in biological research.

On May 27 Alden H. Emery mailed me the roster of the Committee on Publications: Edward R. Weidlein, Beverly L. Clarke, Calvin S. Fuller, Loren C. Hurd, H. F. Johnstone, A. L. Marshall, W. O. Milligan, Charles C. Price, and Glenn T. Seaborg.

Another letter, dated May 27, had arrived from Donald A. McPherson (John Wiley), who informed me that he has kept in touch with Dr. Gibbs and Dr. Goldsmith of the Committee on Radioactivity. McPherson believes that a book of tables is needed right now, but he will contact W. H Sullivan, as I suggested. He also said that he is sorry that our table of isotopes is not available to them. If he is not on vacation, McPherson may see me during the September ACS meeting in New York.

There was a nice note from Reverend John J. Cavanaugh (Notre Dame), who is pleased that I accepted his invitation to be the Nieuwland Lecturer this fall.

Leonard Katzin asked, in a letter dated May 28, that I write letters of recommendation to C. W. Metz, head of zoology at the University of Pennsylvania, and to B. H. Willier, head of biology at Johns Hopkins, for him.

I read the following May 21 letter from Frederic Joliot:

Mon cher Seaborg,

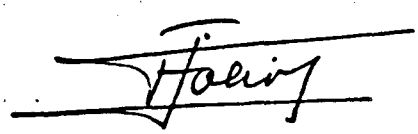
Je vous remercie vivement de votre lettre du I2 mai. Nous regretterons beaucoup votre absence à Londres en juillet, votre présence aurait été des plus utiles mais je comprends les raisons qui ont motivé votre décision.

Dans un autre ordre d'idées, vous savez sans doute que le septième Conseil de Chimie Solvay me demande de rédiger un rapport sur le mode de formation, constitution et filiation des isotopes notamment des isotopes artificiels. Je m'arrangerai pour modifier ce titre qui n'est pas très clair.

Toutefois,il serait utile de donner les derniers résultats concernant l'objet de ce rapport; je vous serais reconnaissant, si cela vous est possible, de me faire parvenir un exemplaire des tables que vous auriez, le plus récemment, mis à jour, vous êtes parmi les spécialistes de ce domaine le plus qualifié pour me donner ces informations.

J'espère avoir le plaisir de vous rencontrer un jour prochain.

Bien sincèrement.



F. JOLIOT.

TRANSLATION

I indeed thank you for your letter of May 12. We will very much regret your absence in London in July. Your presence would have been most opportune, but I understand the reasons which have influenced your decision.

Changing the subject, you no doubt are aware that the seventh Solvay Chemistry Council has asked me to prepare a report on the means of formation, constitution and affiliation of isotopes, particularly artificial isotopes. I shall arrange to change this title, which is not very clear.

Anyway, it would be useful to give the latest results concerning the object of this report; I should be grateful to you if it would be possible for you to send me a copy of the tables which you might have, the most recently issued. Among the specialists in this field, you are the one most qualified to give me this information.

I hope to have the pleasure of seeing you again in the near future.

Sincerely,

/s/ Frederic Joliot

I also noted a May 21 letter from Cyril Smith, who has been looking into the MIT and the Battelle Institute work on low grade ores for our GAC assignment. Smith suggested that I stop in Columbus on my way East for the Battelle people badly need to talk with a good chemist. [Obviously, the letter arrived after my departure for the East.]

I made my rounds of the labs to talk with the men about their work.

Helen's mother, who has been staying with Helen, left for Seattle to visit a friend.

Wednesday, June 4, 1947

I again worked on my correspondence. I answered a May 25 letter from Truman Kohman, who wants to visit for a month or so this summer and look for artificial alpha activities among the elements lighter than lead. I said, in my reply, that we believe we can make the arrangements he suggested--travel expenses, no compensation, but a per diem living expense allotment. I pointed out, however, that he must be prepared to find us in very cramped conditions, especially since we are remodeling--he must be prepared to share occupancy in one of the old rooms.

Ed Westrum wrote on May 27 to explain that he has completed his writing projects (PPR papers) by working 140 hours on the task (or 3.5 weeks according to Westrum) and expending $3.65 for postage. My action was to send a memo to Kenneth Priestley, our business manager, stating that Westrum should receive compensation for this time.

Charlie Price (University of Notre Dame) wrote, in a letter dated May 28, that the first week of December is satisfactory for the Nieuwland Lectures. He asked for a title.

A check for $750 arrived from C. E. Richard (Paymaster of the Nucleonics Project, Schenectady). This is payment for my consulting since November 9, 1946.

C. C. Todd (The State College of Washington) thanked me in a letter dated May 29, for my frank appraisal of Herbert Anderson. He asked for the name of a young man, who might be available with required qualifications.

A note of appreciation arrived from R. F. West (Baillière, Tindall, and Cox, publishers of Kingzett's Chemical Encyclopedia) for my making arrangements with Dr. Strong to have Iz Perlman write the article on "Nuclear Chemistry" for their encyclopedia.

I noted a letter from Dr. R. R. Newell, who included a glossary of an exhibit to be put on at an American Medical Association meeting in Atlantic City this month. He asked for my comments.

Stan Thompson and I have decided to again go to Southern California this year for a golfing vacation (and to visit our families). Today I wrote a note to the Rio Hondo Golf Club to ask them to put us down for tee-off times on the mornings of June 14 and June 15 since we shall arrive too late Friday, June 13, to arrange for times.

Finally, I mailed a personal check for $85 to Dr. Harold Winkler (South Hall, campus) to cover the first month's rent on an apartment he is renting to Edgar Westrum for the summer.

Thursday, June 5, 1947

Our group meeting was held today instead of Tuesday because of my absence then. Present were Asprey, Bockhop, Conway, Covey, Cunningham, Eyring, Garden, Ghiorso, Goeckermann, H. Hicks, Hopkins, Howland, Huffman, James, La Chapelle, Lilly, Lindner, Magnusson, McCullough, Miller, Morgan, Newton, O'Connor, Orlemann, Perlman, Seaborg, Stewart, Street, Templeton, Thompson, Voyer, and Werner. Hopkins described the bombardment of arsenic with 200 Mev deuterons. He found a 9.2-day selenium decaying by orbital electron capture to a 26-hour arsenic; this he assigned to Se^{72} for no obvious reason. He also found a 6.8-hour positron-emitting selenium--no assignment. Hopkins also discussed a 20-day activity in an old arsenic fraction (possibly As^{74}) and a 1-hour positron-emitting arsenic activity. He found a 65-minute germanium with a 9-hour gallium daughter. There was discussion about the assignments; Hopkins will next have a 400 Mev helium ion bombardment of arsenic.

Morgan talked extensively about experiments with last fall's Hanford neutron-bombarded americium sample. His conclusion is that some 5% of the Am^{241} was transmuted to Cm^{242} through the 17-hour decay of Am^{242}. If another 5% goes by the alternate mechanism to a long-lived Am^{242}, the alpha half-life is of the order of 350,000 years based on the equilibrium amount of Np^{238} found in the experiment (assuming 100% chemical yield). In the discussion that followed I stressed the importance of the result since it changes completely the ideas about the number of nuclear isomers in the heavy region. I suggested that the possibility of analogous odd-odd isomeric nuclei, such as Np^{236} (through the observation of daughter beta-emitting Pa^{232}) should be investigated, in addition to looking for the beta decay of this long-lived isomeric Am^{242} (through the observation of daughter alpha-emitting Cm^{242}).

Thompson continued his discussion of plans for working up the Hanford neutron-irradiated americium. An element 98 fraction will be isolated, on the assumption that this element exists in the 3, 4, or 5 oxidation state. If it is in the 4 state, it will be found in the $BiPO_4$ fraction along with element 97. In the 3 state, it will be eluted in the tail of

the curium peak along with element 97. In the 5 state, it will initially follow the curium fraction since it is thought the 5 state of neptunium is carried on a fluoride precipitate. Stan announced that the sample will probably be worked up on July 7, which I said is a good date because the men can spend July 4, 5, and 6 getting ready for it. I also announced that on June 24 an experiment will be performed in which Am^{241} will be bombarded with 44 Mev helium ions at the 60-inch cyclotron in order to look for 97^{243} and 97^{244}, using $BiPO_4$ to carry element 97 in the 4 oxidation state.

Miller talked about his bombardment of copper with 400 Mev helium ions. He found a 39-minute chlorine, probably Cl^{38}; this corresponds to 14 charge units out and 29-31 mass units out. Morgan suggested this might be a fission reaction.

O'Connor discussed his bombardment of uranium with 400 Mev helium ions, in which he found activities of gold, iron, nickel, and copper, the latter three all below the normal fission product region. I suggested that he should use uranium of known purity in order to preclude the possibility of these activities coming from impurities and that he should look at tantalum and tungsten activities. O'Connor said he wants to look for beryllium and other light nuclei, which he believes might be activities formed in almost every element that is bombarded.

Lindner described his latest bombardment of antimony with 400 Mev helium ions. He separated elements from silver to molybdenum with a fall-off of 1000 in activity between silver and molybdenum. Howland described a sample of bismuth irradiated in the Hanford pile for several months to look for Bi^{208}. A sample of the bismuth fraction seems to have a half-life of about one year.

* * * * *

Today I gave the last lecture this semester in Chemistry 123--this was on cosmic rays. I then stopped in to see Geoffrey Wilkinson on the top floor of Gilman Hall before returning to the hill.

Friday, June 6, 1947

Much of my day was spent talking with the men about their work. Don Stewart, who has not been doing too well in his course work and has been contemplating transferring to biochemistry, told me that he has made arrangements for such a transfer. He will continue to work part time on the hill.

Helen had a 3 p.m. appointment with Dr. Lois S. Wilson, Dr. Borson's associate in obstetrics. Then, in the evening she and I went to the Oaks Theater and saw Larry Parks and Evelyn Keyes in "The Jolson Story."

Saturday, June 7, 1947

Both Tom Morgan and Paul O'Connor will be getting their Ph.D.'s in September, and I have been spending some of my "free" time reading the drafts of their theses and offering suggestions. Morgan's thesis is called "Isotopes of the New Element Americium (Atomic No. 95)"; his

thesis committee consists of Robert B. Brode, Robert E. Connick, and Glenn T. Seaborg. O'Connor's thesis is entitled "The Chemical Identification of Isotopes Formed by the Bombardment of Uranium with High Energy Particles," and his committee consists of the same men. Since some of the work here at the Radiation Laboratory is not yet declassifiable, we have arranged to have theses containing reference to such work filed temporarily here on the hill rather than on campus; the men will still be credited with having filed their theses.

As we often do on Saturday, Stan Thompson and I had lunch at Larry Blake's--in addition to a long talk about our plans to produce and chemically identify element 97, we also talked about the University's baseball team under Coach Clint Evans. The team, with pitcher Jack Jensen, has been doing very well this season even though yesterday's game against Washington State was called because of rain and today's weather does not look promising.

Sunday, June 8, 1947

The leading article on the sports page in today's San Francisco Chronicle reported that, by agreement, California's baseball team was declared the winner of the Pacific Coast Intercollegiate playoffs and will represent the Coast in the NC2A Western Regional playoffs at Denver, June 20 and 21. The article described how yesterday's game between Washington State and the Bears was called in the fifth inning because of the field had become a quagmire. After the game, Director of Athletics at Washington State, Lloyd Bury, and Coach Buck Bailey decided that the Bears should represent the Coast on the basis of their 6-1 triumph Friday.

Then, after a lazy morning at home during which I caught up on some reading, Helen, Jeanette, Pete and I took a drive in the afternoon around the neighborhood of our recently acquired home on Ellsworth Street.

Ernest Lawrence and I went with some of the General Electric people to Planters' Dock for dinner.

Monday, June 9, 1947

A telegram arrived from W. H Sullivan, asking if July 28 is a satisfactory date for the nuclear constants meeting in Washington. I immediately wired a response to Sullivan at Clinton, saying that the evening of the 28 is ok and that the remainder of the committee should meet further during the day.

Iz and I spent some time today preparing the final examination for Chem 123 (to be given on Wednesday).

Tuesday, June 10, 1947

There were administrative matters and considerable mail to handle this morning.

On May 24, Miss Frances V. Benner (Special Assistant, American Chemical Society) wrote about the multitude of local section groups who

want me to address them during the next year. Miss Benner made several suggestions how these invitations might be handled. In my reply today I explained that, because of my heavy schedule, I feel that I must decline all of the speaking tours she suggested.

I also declined a May 26 invitation from Edward L. Haenisch (Vice Chairman, Division of Chemical Education, ACS) to address a symposium at the New York ACS meeting in September on "Transuranic Elements, Their Detection, Properties and Significance." I suggested to Haenisch that Professor Isadore Perlman is well suited to give such a talk.

Gerard Piel (Editor & Publisher of The Sciences, a new magazine for the intelligent layman that he and Dennis Flanagan have conceived) wrote on May 27 to send me a draft of the magazine, to ask my comments, and to ask me to join an informal "committee of correspondence"--sort of a review committee. I replied that I can find nothing to criticize in the "scale model" although I think there is a great danger of a gradual falling off in quality. I said that I am willing to join an informal "committee of correspondence," but I have very limited time to devote to it.

I wrote to W. O. Milligan (Rice University) to decline his June 4 invitation to speak at the Southwest Regional Meeting of the ACS in December. Again I explained that my schedule is overfilled.

I replied to a couple of letters (May 24 and 30) from A. C. English at Northwestern University. English wrote that the Canadians have cleared their paper on the 4n + 1 series. I wrote that I am becoming annoyed that our paper has not yet been cleared. [I suggested, by carbon, that Leonard Katzin check into this.] I then offered English early photographs of such things as micro precipitates of Np^{237} and Am^{241} for use in the monograph that he and Paneth are preparing and suggested that he work with Leonard Katzin about obtaining a security clearance for himself so that he can discuss the 4n + 1 work with Americans; this may be a long process, however, judging from our experience with Geoffrey Wilkinson. I then wrote to Katzin, enclosing the carbon of my letter to English and copies of their manuscript, and asked him again to look into the clearance problem of our article. I suggested to Leonard that he work over the previous letter of recommendation that I sent Schrader at Columbia; then I will write to Professor C. W. Metz at the University of Pennsylvania and Professor B. H. Willier at Johns Hopkins, where Katzin plans to apply for academic positions.

To Charlie Price (Notre Dame) I sent the title, "Transuranium Elements and Nuclear Energy," for my Nieuwland Lectures. I added that I hope the change of date to the week of November 17 is satisfactory to the University; I asked the best way to go to Washington from South Bend on Thursday night, November 20.

In reply to a letter of May 26 from Dr. Stacy R. Mettier (Head of Postgraduate Instruction of UC Medical School), I wrote that I shall be glad to speak on radiochemistry and the new elements on the evening of July 8, in connection with the Special Lecture Course to be given at the Medical Center.

Finally, I mailed Miss Hoylande Young a copy of the completed "Table of Isotopes" for inclusion in the handbook volume of the PPR. I explained that, when the Information Office here learned of the existence of the "Table of Isotopes," they wanted to issue it, and we agreed to let them. I added that we may submit it for declassification as soon as the rules about the declassification of the radioactive properties of the heavy elements are clarified.

Wednesday, June 11, 1947

After a couple of phone calls, I looked at the mail. A thank-you note arrived from John R. Kuebler (Grand Recorder of Alpha Chi Sigma) for the photograph and remarks I mailed him a couple of weeks ago for use in the Hexagon in September.

I answered a June 6 letter from Edgar Westrum, who told about his arrival plans in Berkeley on June 17 or 18 and enclosed a check to reimburse me for his apartment deposit; he then asked about the feasibility of visiting Los Alamos on his way to Berkeley. In my reply I explained that it appears impossible to visit Los Alamos on such short notice and it would be better to make a side trip from Berkeley. I also told him that I shall see him on June 20 since I shall be in Los Angeles through the 19th.

Since many of the fellows are busy with final examinations, I spent much of the day reading.

Thursday, June 12, 1947

Iz and I gave the following final examination, which we believe is rather tough, to the Chemistry 123 class. We spent the evening grading the papers.

Chemistry 123
Final Examination
June 12, 1947

1.

(10) (a) Calculate the energetic threshold, neglecting conservation of momentum, for the following nuclear reaction:

$$_{26}Fe^{57}(\alpha,4n)_{28}Ni^{57}$$

The upper energy limits of the $_{28}Ni^{57}$ and $_{27}Co^{57}$ positron spectra are 0.67 Mev and 0.26 Mev respectively. Assume no gamma rays.

(8) (b) Calculate the energetic threshold taking into account conservation of momentum.

(3) (c) Explain whether or not this reaction would be observed at about the threshold calculated in part (b).

2.

(8) The sun radiates energy at the rate of 3.8×10^{33} ergs per second. Calculate its consumption of hydrogen in grams per second.

3.

(10) A mixture of 8.0-day I^{131} and 1.0-day I^{133} was allowed to decay and the activity after 12 hrs was 2420 counts/minute and after 10 days it was 375 counts/minute. How many counts/minute of each isotope was present at zero time? Solve graphically and explain your method of solution.

4.

(8) (a) Explain how you would use radioactive Cu^{64} to determine the solubility of CuS in pure water.

(7) (b) Outline a possible method for separating radio-iodine from radio-bromine without carriers.

5.

(10) Boron has a neutron capture cross-section of 540 barns for thermal neutrons. A beam of thermal neutrons is directed at a layer of boric acid (H_3BO_3), density 1.2. How thick in cm. must this layer be in order to absorb half of the neutrons?

6.

(8) (a) List and give the origin of the various types of particles and radiation produced during the operation of a chain reacting pile.

(8) (b) Discuss the important chemical and physical effects that any of these might produce in the components of a future high energy power pile and hence how they will affect the method of operation. That is, what limitations does

the existence of these effects impose upon pile operation?

7.

(3) (a) If $_{92}U^{235}$ is beta-stable would $_{92}U^{236}$ be predicted to be beta-stable?

(3) (b) Which of the natural decay series would U^{232} tie into?

(3) (c) In the slow neutron fission of U^{235} what is the probable mass number of the complementary chain to the mass number 80 chain?

(3) (d) Under what conditions might one observe a (p,γ) reaction?

(3) (e) What reaction with the cyclotron would produce 2.7-day Au^{198} from Au^{197} in highest yield in a thin gold foil? (Au^{197} captures neutrons with a cross-section of 100×10^{-24} cm^2 to give 2.7-day Au^{198}.)

8.

(10) The isotope $_5B^9$ would be expected to be a positron emitter like the analogous "mirror image" isotopes $_7N^{13}$, $_9F^{17}$, $_{11}Na^{21}$ of the type p = n + 1 which decays to n = p + 1. Following the same analogy, the half-life for β^+ emission would be expected to be of reasonable length. As B^9 has never been seen, show by means of calculations why it probably cannot exist.

Friday, June 13, 1947

I attempted to clear away some of my accumulated correspondence, preparatory to my vacation.

I answered a June 6 letter from Milton Burton, who is pleased that I will give the Nieuwland lectures. Burton offered to have me stay in his home as a guest and then said that he and Sarah, his wife, want to have a party for me while I am at Notre Dame. In my reply I said that I shall be happy to attend a party, but I believe I should stay in a public hostelry. I then mentioned that the lectures are tentatively set for the week of November 17 and asked if possibly there is a Notre Dame football game in South Bend on Saturday, November 15, that I might attend if I arrive early.

M. Gutmann, a medical man, wrote on June 8 with questions about a number of compounds that he would like to be made radioactive. He also asked about the effect of radioactivity on cancerous tissue. I told Mary Bender to send the letter to Melvin Calvin and ask him to reply to it.

A June 11 letter arrived from Truman Kohman, who gave a number of reasons why he has decided not to come to Berkeley this summer. Kohman asked about the status of our "Table of Isotopes" and mentioned that H Sullivan (who is leaving for Chalk River to become the scientific attaché for the AEC there) is also interested. He suggested that I inquire of Hoylande Young about the status of our isotope chart in order to speed up the declassification process. I telephoned Truman to discuss these items and then wrote to him. In my letter I suggested that he fill out the personnel security questionnaire, in spite of not coming this summer, so that he can be on our list of consultants. In discussing Chapters 2 and 14 of the Plutonium Project Record (PPR) Vol. 14A, I said that they should be issued in their unaltered form, but they will have to be changed before being declassified for the PPR.

I then wrote Hoylande Young to tell her that Truman, H Sullivan, and I are very concerned that our "Chart of Isotopes" be issued as soon as possible. I wrote that, if there are mechanical problems, such as lack of paper, etc., I would be glad to try to help if I can. I then pointed out that our Information Office here probably did save her some pressure by issuing the "Table of Isotopes" as quickly as they did.

To W. H Sullivan in Chalk River, I wrote explaining that my reply to his telegram about the nuclear constants meeting was sent to Oak Ridge, in spite of his specific instructions because of a secretarial error. I reiterated that I could meet on the evening of Monday, July 28, although the Committee should also meet during the day. I also told Sullivan that our "Table of Isotopes" has been finished and issued as a project report and that he is on the distribution list to receive one at Oak Ridge.

A letter of recommendation went to the Director of Admissions at Cornell University for E. J. Leshan, who received a "B" in my course in nuclear chemistry this semester (Leshan wants to work toward a Ph.D. in chemistry). I wrote that he impressed me as having a nice personality.

Another letter was sent to E. R. Weidlein, who asked on June 4 for

topics for discussion at the New York meeting of our temporary ACS Committee on Publications. My response read:

> There is a widespread feeling among the people who are working in the new field of nuclear chemistry that there is the need for either a new medium for publication or rather serious modification of the methods and rules in operation with the present journals such as the Journal of the American Chemical Society. It seems to me that this might be the sort of problem which properly concerns our Committee.

I also noted a letter from Frank W. Test (Chicago Patent Group), announcing the filing of patent application S-2081 on May 23, 1947.

The final examinations for Chemistry 123 have been graded with the help of our reader, Harry Hicks, and Iz and I have made out the grades for the class: Abshire, C; Bond, B; Awtrey, A^+; Bassham, B; Boydston, B^-; Bromley, A; Coles, C; Conard, B^-; Cunningham, A; Daus, A; Edwards, B^-; Eyring, B^+; Faerber, F; Goeckermann, A^+; Granquist, C; Hicks (Tom), B; Hirsch, B^+; Hurd, C; La Chapelle, A; Leshan, B; Lindner, A; Magnusson, A; Maxwell, C; Mitchell, D; Orr, B; Oswalt, B; Postrel, C; Reinhard, A; Rubin, B; Scadden, C; Schwabacher, B; Street, A^+; Thornton, B; Weiss, B; Werner, A; Wilkins, C; Wingard, B; Witt, C. On the final examination three of our graduate students, not surprisingly, received the top grades: Goeckermann, Lindner, and Street.

Stan Thompson and I took a late afternoon flight to Los Angeles for our golfing vacation--I shall stay, as usual, with my parents in South Gate while Stan will stay with his mother on Dorothy Avenue in South Gate.

Saturday, June 14, 1947

In South Gate. Stan and I went to the Rio Hondo Golf Club for an early game of golf (SGT-89, GTS-104.)

Later my parents questioned me about Peter and Helen. I also told them about some of my activities.

Sunday, June 15, 1947

In South Gate. My cousin Elmer Johnson played the first nine holes with Stan and me at Rio Hondo today. For 18 holes I scored 95 and Stan scored 97. I then returned home to join my parents for one of my mother's typical Sunday dinners.

Monday, June 16, 1947

In South Gate. Instead of playing golf today, Stan and I went out to our alma mater, David Starr Jordan High School, and visited with some of the teachers who were still around. Our chemistry teacher, Dwight Logan Reed, was particularly pleased to see us.

I called my office in Berkeley to check on the mail, etc. and learned the following telegram had arrived from Katzin: "SERIES PAPER DECLASSIFIED AS OF TODAY WILL START RETYPING HERE FOR PHYSICAL REVIEW SUBMITTAL TO AVOID DELAY DUE TO YOUR VACATION WIRE IF PREFER TO SUBMIT

PERSONALLY FROM YOUR OFFICE WILL TELEPHONE ENGLISH WHEN READY TO MAIL MANUSCRIPT."

Tuesday, June 17, 1947

In South Gate. I talked by phone with Leonard Katzin at Argonne about our 4n + 1 paper. I suggested that he contact A. C. English at Northwestern and have him [English] look at our paper and then send both papers to John Tate for simultaneous publication in The Physical Review. On another matter Katzin asked me to send a letter of recommendation for him to Professor Eastlick at Washington State, which I agreed to do after I return to Berkeley.

Stan and I played at the Western Avenue Golf Course today (SGT-100, GTS-94). Fred Metcalf and Bill Jones played along with us.

Later I had dinner with my old friends Rita and Clayton Sheldon and their young sons, Ronnie and Gayle, at their home on Hildreth Avenue in South Gate.

[In Berkeley, a letter arrived from Jim Fisk concerning the motor generator sets in which Professor Giauque is interested. Iz sent the reply on to Professor Giauque.

Pete had a checkup with Dr. McAlear at 10:30 this morning.]

Wednesday, June 18, 1947

In South Gate. Stan and I went to the Baldwin Hills Golf Course out on West Slauson Avenue today. Our golf scores were (SGT-101, GTS-104).

Thursday, June 19, 1947

In South Gate. Stan and I played at the Fox Hills Golf Club at Baldwin Hills. Our scores were SGT-99 and GTS-108.

Later I returned home to Berkeley. It has been a relaxing vacation and possibly for Helen, too--she hasn't had to cook, etc. for me.

Friday, June 20, 1947

Coach Clint Evans and Cal's baseball team are in Denver (and will play Denver) today for the initial game in the Western NCAA playoffs.

Back at the laboratory I made my usual tour of the labs to see how the work is progressing and then checked the mail.

I noted a letter from Truman Kohman, who told me that one of the reasons he decided not to come to Berkeley this summer was that he has heard through Bill Libby of the opening that Professor F. A. Long has at Cornell. Although he had intended to remain in Chicago for another year, he has decided to apply for the Cornell position.

I also heard from Frank Long (Cornell), who said that O'Connor will visit within the next week. Long added that they are also talking with Truman Kohman and asked for my opinion of him.

I read a carbon of a letter from Winston Manning to Herbert A. Young

at Davis. Manning told Young that he is sorry that he (Young) has made other plans for his sabbatical and would not spend it at Argonne.

Later I went over my notes for my remarks for the luncheon tomorrow.

Saturday, June 21, 1947

Sports news this morning--The Bears defeated Denver, 3 to 1, yesterday in the NCAA baseball tournament. They will play the Texas Longhorns tonight.

This noon I took part in the annual Commencement Day luncheon of the California Alumni Association at Faculty Glade. After the luncheon and a few introductions and remarks, I was introduced as co-discoverer of plutonium and "Chemist of the Year." I talked briefly about recent nuclear research at the Radiation Laboratory and the prospects for commercial nuclear power. For illustration, I showed the crowd a one-pound cube of uranium and described how long such a cube could supply Berkeley with electricity.

Bartley C. Crum, San Francisco attorney and a member of the Anglo-American Palestine Commission, gave the main address and charged that a continuation of the Truman Doctrine as applied to Greece and Turkey will lead to the economic ruin of the United States. He asserted that both the United States and Russia must surrender their sovereign power to make war if we are to have peace. The program concluded at 2 p.m. with the singing of "All Hail" and was followed by the Academic Procession to the stadium for Commencement exercises.

Sunday, June 22, 1947

Jerry Howland and I went to Mira Vista Country Club for a round of golf (JJH-119, GTS-102). Two other fellows, Dick and Tony, played along with us.

I played with Peter for a bit and worked for a while on a lecture that I am scheduled to give on July 8 at the Medical Center in San Francisco. The subject is to be radiochemistry and the new elements.

Helen has been most busy for the last couple of weeks preparing and packing for our move to our new home, which we plan to do on Monday of next week. I'm afraid I haven't been particularly helpful.

Monday, June 23, 1947

The first thing I did at the lab this morning was to write a few letters, including a response to Leonard Katzin's letter of June 9 and our phone call while I was in South Gate. I wrote letters of recommendation for him to Professor H. L. Eastlick, Department of Zoology, Washington State University; Professor C. W. Metz, Department of Zoology, University of Pennsylvania; and Professor B. H. Willier, Department of Biology, Johns Hopkins University. These were similar to letters I have written previously for Leonard.

A letter arrived from Milton Burton, saying he is pleased that I will

be able to come to a party at his home during my visit to Notre Dame. He then informed me, unfortunately, that Notre Dame plays in Evanston on November 15 (which means it is not too feasible for me to attend the game); he asked if it would be possible for me to spend the afternoon of November 22 in South Bend for Notre Dame will play Tulane at home on that day.

George W. Barton, Jr., a graduate student who received his B.S. from Cal Tech, has received his clearance and began working with our group today. Iz Perlman will supervise his research.

The rest of the day was spent reading, talking with the fellows, etc. In addition to science, some of the fellows are particularly pleased that the Cal baseball team is going to Kalamazoo, Michigan, next weekend to meet Yale in the NCAA finals.

Tuesday, June 24, 1947

Present at this morning's group meeting were Beaufait, Bockhop, Conway, Covey, David D. Cudaback (an undergraduate whom I met when he was a participant in the Science Talent Search in Washington, D. C., on March 2 of last year and who is working as a technician for us this summer), Eyring, Garden, Howland, Huffman, La Chapelle, McCullough, Miller, Morgan, Newton, Orlemann, Perlman, Seaborg, Stewart, and Westrum.

Howland reported on the Po^{206} (~9-day K electron capture) → Bi^{206} (6.4 day, K electron capture) system. He has produced a stronger sample by bombardment of ordinary lead with 40 Mev helium ions and followed the decay of the activity for 60 days. The half-life of Po^{206}, as observed in this sample, is about 9 days. Howland talked about the evidence for an active isomer of stable Pb^{204} (68-minute gamma-ray decay); the mass assignment to mass number 204 seems to be the most probable, but mass number 203 is not ruled out. This is the only known case of an even-even stable nucleus with an isomeric state. He also elaborated on the assignment of the 9-day activity to Po^{206}. Howland said he plans to bombard lead (Pb^{206} from uranium ores) and bismuth with 30-100 Mev deuterons to look for, among other things, several short-lived, previously observed, alpha activities in the bismuth fraction.

Morgan talked about the previously observed neptunium activity (ether-extracted from the Hanford Am^{241}) that has an absorption curve practically identical with Jaffey's for Np^{238}. He now has more chemical identification with the same conclusion. Morgan also talked about the 12-hour and 50-hour americium activities from helium ion bombardment of Np^{237} and deuteron bombardment of Pu^{239}. He said Thompson and Cunningham are bombarding Np^{237} with 32 Mev helium ions at the 60-inch cyclotron today and that an americium fraction will be isolated to study further the 12-hour and 50-hour activities. The ratio of 12-hour to 50-hour activities seems to be four to ten times greater with Np^{237} plus helium ions than with Pu^{239} plus deuterons.

Newton discussed a separation of U^{232} from the millings of thorium bombarded with 40 Mev helium ions in order to compare the yield from particle emission reactions to the fission yield. His yield curve showed

an inflection that may be due to deuteron contamination of the beam.

McCullough talked about his analysis of a 25-50 mg sample of HfO_2 heated to 1500°C for 15 minutes by Leo Brewer--it definitely has the stable ZrO_2 (monoclinic) structure, not the fluorite structure previously reported by Zachariasen and others.

Beaufait talked about preliminary scouting work on the purification of hafnium from zirconium with Nalcite resin. Perlman and I suggested that he look up Boyd's early reports on resin absorption of fission products.

* * * * *

Members of our group received their copies of the "Table of Isotopes" today from the Information Division.

A letter arrived today from W. Albert Noyes, Jr. (ACS), informing me and W. K. Lewis that the General Meeting, at which both of us are to speak, has been scheduled tentatively for Monday, September 15, at 8 p.m. in Manhattan Center (34th Street, west of 8th Avenue) in New York. The mixer, Noyes said, will probably be held after the General Meeting.

I answered a June 20 letter from Kenneth Bainbridge, who said that the "Table of Isotopes" he received was missing sheets beyond page 47. Bainbridge offered to send us his table of stable isotopes when it is completed. In my reply I told him that I am sending him the missing sheets and that we are looking forward to seeing his table.

Wednesday, June 25, 1947

On June 10, B. S. Kinsella mailed me the galley proof of the material Iz Perlman prepared for The Lincoln Library of Essential Information. Kinsella asked for an additional 290 lines of material for the article in order to meet their requirement for exactly 30 pages (pages 949-978, inclusive). I referred the request to Perlman, who agreed to working up the additional text; today I returned the proof appropriately marked for the additions.

I received a note from W. H Sullivan, thanking me for my letter of June 13 (re the date of meeting of the Committee on Nuclear Constants) and saying that he actually received the telegram that was missent to Oak Ridge. He then asked that I send an additional copy of the "Table of Isotopes" to him at Oak Ridge in order that he may have one for his use at Chalk River and Ruth Hassler will have one to use at Oak Ridge on her work on the Encyclopedia of Nuclear Data. I immediately replied that I am asking the Information Division to send him the additional copy.

Much of my day was spent talking with the men about their work.

Thursday, June 26, 1947

I received and noted a letter from Alfreda Carlson (Lecture Organizer for the University of California Extension). Carlson asked that I fill out a form and list the topics on which I am prepared to speak; this is

to be included in an announcement of Lectures and Readings offered by the Berkeley Faculty (the notice reported that if the list is not returned by July 1, it is assumed that I do not want to be included).

I am trying to keep abreast of report and journal reading, in addition to conferring with the men about their research.

Pete had a shot at Dr. McAlear's today, and then Helen had an appointment with Dr. Borson at 1:15 p.m. Both are doing fine.

Friday, June 27, 1947

This morning I answered a June 11 letter from William E. Roake, who inquired about a position as a physical chemist (Ph.D.) on our staff. I explained that our program is carried on by faculty members, their graduate students, and a limited number (already filled) postgraduate fellows. I suggested that he might be interested in a position at Argonne National Laboratory since the work there is of a similar nature and is carried out largely by Ph.D. men. A carbon of my letter went to Winston Manning.

I wrote to my friend L. Reed Brantley (Occidental College) who, as Chairman of the Southern California Section of the ACS, asked me on June 21 to address their opening Fall meeting on October 3. I explained that I cannot make it on that date because of an AEC General Advisory Committee meeting in Washington; however, I suggested that he invite Dr. I. Perlman, who is an excellent speaker. Another possibility I offered was that I speak at a later date.

Another letter went to W. Albert Noyes, Jr., in which I informed him that I have decided to change the title of my talk before the General Meeting of the ACS at the September meeting in New York to "Nuclear Transformation in the New High Energy Ranges." A carbon copy of my letter went to Alden Emery.

Iz and I prepared and sent to George Everson the following list of monthly salary adjustments to be effective July 1:

Name		Present	Adjusted
Asprey, Larned B.	(P3)	350.00	375.00
Conway, John G.	(P2 to P3)	320.00	365.00
Covey, Elwin H.	(P2)	302.50	320.00
Cunningham, B. B.	(P6)	550.00	600.00
Eyring, Le Roy	(P3)	325.00	350.00
Goeckermann, Robert	(P1 - P2)	270.00	300.00
Ghiorso, Albert	(P4 - P5)	450.00	510.00
Hopkins, H. H.	(P3)	315.00	345.00
Howland, Jerome	(P3 - P4)	390.00	425.00
James, Ralph A.	(P3)	337.50	375.00
La Chapelle, T.	(P3)	345.00	360.00
Lindner, Manfred	(P3)	350.00	375.00
Magnusson, L. B.	(P3)	370.00	390.00
Miller, Daniel R.	(P3)	365.00	390.00

Morgan, Leon O.	(P2 - P3)	330.00	360.00
O'Connor, Paul	(P2 - P3)	315.00	360.00
Reynolds, Frederick	(P3 - P4)	390.00	425.00
Robinson, Herman	(P5)	490.00	550.00
Stewart, D. C.	(P5)	450.00	475.00
Templeton, David	(P3)	345.00	375.00
Thompson, S. G.	(P6)	500.00	550.00
Werner, Louis B.	(P3)	375.00	390.00
Huffman, Eugene H.	(P5)	475.00	500.00

Saturday, June 28, 1947

The headline on today's sports page of the San Franciscco Chronicle read: "After Storm Clears, Bears Massacre Yale Nine, 17-4." This is really exciting news.

"The Transuranium Elements" by Glenn T. Seaborg and Emilio Segrè is scheduled to appear in the June 28th issue of Nature [Nature 159, 863 (1947)]. We describe the early, prefission work by Fermi and colleagues and by Hahn, Meitner, and Strassmann on the so-called "transuranium" elements, the discovery of fission by Hahn and Strassmann, then the subsequent discovery of the real transuranium elements (neptunium and plutonium), the demonstration of the fissionability of Pu^{239}, the isolation of Np^{237}, and the more recent discovery of americium and curium, the naming of these elements, and the demonstration of the presence of Pu^{239} in nature.

I read a letter from Herb Young at Davis, who explained that they are having A. D. Webb appointed as an Associate in Chemistry, effective July 1, since he is needed to assist with teaching. He will be able to continue the research on tungsten compounds, which he will use for his thesis. Young said that he would like to spend some time in Manning's laboratory but, because of the security program, he is taking his sabbatical in another field at Cornell University. I wrote Young a note and said the program is satisfactory to me. I also mentioned that one or two of our young men are negotiating with Frank Long for a position at Cornell so there may be some of our kind of chemistry going on there.

Sunday, June 29, 1947

"Cal Beats Off Yale's Rally, Wins NCAA Baseball Crown" read this morning's sports headline (yesterday's score was 8 to 7).

Jerry Howland and I went to Mira Vista for a few holes of golf today (for nine holes JJH-53, GTS-53). I then returned home and helped Helen move some of our personal things to our new house at 2808 Ellsworth Street in Berkeley.

Monday, June 30, 1947

Helen arranged for a small moving company to move our belongings to our new home this morning. I stayed home during the bustle and activity to help keep an eye on Pete, who is now walking and beginning to climb. After lunch I went to the lab, checked on the mail, and talked with the men.

I received from W. Albert Noyes a list of delegates from the ACS who will attend the meeting of the International Union in London: E. R. Smith, A. H. Emery, L. Pauling, F. D. Snell, R. C. Swain, E. Wichers, F. J. Curtis, I. M. Kolthoff, H. F. Mark, W. J. Murphy, J. W. Perry, A. Silverman, General A. H. Waitt, and W. R. Kirner. Noyes asked me to designate one of them as an alternate to me to attend the meetings of the Commission of Radioactive Constants.

A letter arrived from J. H. Manley, verifying a Statler Hotel reservation for me beginning July 27. Manley asked for the exact period of my stay in Washington. Other items that Manley mentioned include the fact that the duties of Miss Louise Johnson are now being carried out by Anthony A. Tomei (who will be in Room 122 of the AEC Building), that the Commission can secure payment for "collect" telegrams only if they relate to official business (not reservations, etc.), and that the Commission has informed the Military Liaison Committee that it might be desirable for the two committees to have a short time together during the period of the next meeting.

A note arrived from Leonard Katzin in which he informed me that he has learned the "4n + 1 Series" letter will appear in the August 1st issue of The Physical Review.

I also read a note, dated June 26, from Robert Sibley who said, "You certainly were going on all sixteen in that fine presentation of research work in the chemistry and nuclear physics departments of the University. You helped to make the Commencement Luncheon the great success that it was and I offer you my hearty thanks and congratulations."

This evening Helen and I listened on the radio to Edward R. Murrow's broadcast of "The Sunny Side of the Atom." This CBS program on the peaceful uses of atomic energy is partially the result of the work of Ruth Ashton who visited our laboratories recently.

Tuesday, July 1, 1947

At this morning's meeting of our group the following persons were in attendance: Asprey, Barton, Conway, Covey, Cudaback, Cunningham, Garden, Huffman, La Chapelle, Lilly, Lindner, Magnusson, McCullough, Miller, Morgan, Newton, Perlman, Robinson, Seaborg, Stewart, Templeton, Thompson, and Westrum.

Morgan discussed the status of the isomers of Am^{242}: the isomer decaying by alpha particle emission with a 3 x 10^5 year (est) half-life has been definitely established; it may be beta unstable, but no Cm^{242} was detected. Since there is a possibility of orbital electron capture, he plans to look for plutonium K (115 kev) x-rays in a pile-irradiated sample of very pure Am^{241}. Morgan suggested that it is desirable to return the irradiated americium to the pile in order to obtain workable quantities of Am^{243} and higher isotopes--if Am^{243} is beta stable, which it should be if Am^{242} decays by orbital electron capture. I then discussed the decay of known "odd-odd" types of isotopes--Pa^{232} and Np^{238}.

Perlman reported on a 50 Mev deuteron bombardment of Pb^{206} (from uranium ores) in order to investigate some short-lived alpha emitters, thought to be low mass isotopes of bismuth, found in a 200 Mev deuteron bombardment of lead. No alpha emitters were found. The situation now is: Bi^{201}--?, Bi^{202}--?, Bi^{203}--long-lived, Bi^{204}--12 hour, Bi^{205}--long-lived, Bi^{206}--6.4 day. They also plan some more work on At^{211} formation by secondary alpha particles from deuterons on bismuth or some other mechanism.

Magnusson talked about his investigation of the behavior of Np(V) during the precipitation of lanthanum fluoride, which corroborated the observations reported by Hindman at Chicago that LaF_3 will carry up to about 1% of its own weight of Np(V). He concluded that, under the conditions he used, that disproportionation (provided there is a tendency to disproportionate) is sufficiently slow to permit the inclusion of Np(V)in the LaF_3 precipitate. The extractability of Np(V) into ether from a solution of 8 M NH_4NO_3 and 1 M HNO_3 appeared to be quite low; Cunningham said these results are not surprising. I suggested that a comparison of the extractability of Np(V) and Pa(V) by ketones from aqueous solutions should be made. Magnusson also noted that, with a nitrate solution of Np(V), the strong absorption band at 981 mμ was greatly suppressed when the solution was made 8 M in NH_4NO_3.

We talked about the chlorine activities Hopkins found in a 400 Mev helium ion bombardment of $_{33}As^{75}$, in which three activities appeared in the chlorine fraction (none in the bromine fraction): 37-minute (likely Cl^{38}), 100-minute (Cl^{39}), and 19.5-hour (A^{39}). Perlman suggested the 37-minute activity could also be Cl^{34}. I said the presence of chlorine activity means that products have been obtained that are roughly one-half the atomic number and atomic weight of the target. Hopkins also saw a 4-hour $Sc^{43,44}$ and probably 63-hour Sc^{47}. Perlman remarked that Hopkins found that a 26-hour arsenic positron emitter, thought to be As^{72}, could be milked from an 8.2-day selenium activity, and is thought to be a K-capture isotope. Perlman noted that Miller has calculated relative yields for all products down to 16 mass units out, for both

helium ions and deuterons on copper and has found that the relative yields are about the same for all products.

Conway said his hafnium spectroscopic analysis showed 98% hafnium, 2% zirconium, and a trace of silicon (separation by Huffman's group).

Huffman reported that his tests on the adsorption of zirconium on paper pulp, carried out in HCl solution, showed the zirconium to be adsorbed while adsorption did not take place when HF was used instead of HCl. These results agree with Boyd's work.

* * * * *

Part of the rest of my day was spent preparing the talk on radiochemistry and the new elements, to be given at the Medical Center on July 8.

Wednesday, July 2, 1947

I answered the June 22 letter from W. Albert Noyes, in which he asked me to name a substitute to attend the meetings of the Commission of Radioactive Constants at the International Union meeting in London in July. I said,

> My first choice would be Kolthoff, but I would want to make it someone else if Lind has chosen Kolthoff as his alternate. In that case make it Silverman and, if Silverman can't accept this task for some reason, make it R. C. Swain.
>
> I would like to know the name of the man chosen so that I might correspond with him to give him a few of my ideas concerning the coming meetings of this Commission.

I received a letter from Nathan E. Ballou, who said that he has accepted the offer of employment at the Radiation Laboratory and will report for work the last of July.

I made my usual tour of the labs to see how the work is progressing and then worked on a manuscript on "The Actinide Series" that I agreed to prepare for Professor Sneed's Comprehensive Inorganic Chemistry.

Thursday, July 3, 1947

Joe Katz from Argonne is visiting Berkeley this week in order to work with us on the preparation of Volume 14A, Plutonium Project Record, "The Actinide Elements." Much of the day was spent talking and working with him.

In today's mail I noted a letter from Herbert L. Eastlick (The State College of Washington) who wrote on June 28 to thank me for my evaluation of Leonard Katzin, and to say that he has concluded that they cannot offer Katzin sufficient opportunity there since their position requires a great deal of time spent in elementary teaching and only a limited time for research.

Friday, July 4, 1947

This was a quiet holiday. I worked at home for a while on my writing projects and then went to Mira Vista with Stan and Iz for a round of golf (IP-103, SGT-107, GTS-104).

Saturday, July 5, 1947

I again worked with Joe Katz on the PPR papers. I have agreed to revise Chapter 2 of Volume 14A, which is called "Isotopes of Plutonium and Their Radioactive Properties," in addition to revising Chapter 14 on the radioactive properties of the neptunium isotopes.

Sunday, July 6, 1947

Part of the day was spent going over my notes for my talk at the Medical Center on Tuesday. I also looked at the information that I shall present on Friday at the Stanford meeting of the American Physical Society.

Later Helen, Jeanette, Peter, and I took a ride in Jeanette's car. I hope that I shall soon be able to purchase my own car. Unfortunately, most of the car dealers are not too cooperative--they obviously want a little special payment just for being willing to sell a car.

Monday, July 7, 1947

This morning I talked with Joe Katz for a while and then worked on "The Actinide Series" for Professor Sneed's book. Later Iz and I went over the material we plan to present Friday at the APS meeting at Stanford.

Tuesday, July 8, 1947

Present at this morning's group meeting were Asprey, Barton, Bockhop, Conway, Covey, Cudaback, Cunningham, Eyring, Garden, Ghiorso, Goeckermann, Howland, Huffman, James, Katz, La Chapelle, Lilly, Magnusson, Miller, Morgan, Morris, Newton, O'Connor, Perlman, Robinson, Seaborg, Stewart, Templeton, Thompson, Werner, and Westrum. First, Westrum summarized the heat of formation determination that he and Eyring completed: the heat of formation of PuF_3 is -380 kcal/mole. After a discussion of this work, Katz brought the men up to date on the experiments going on at Argonne.

Howland reported on the bombardment of natural Pb^{206} with ~40 Mev deuterons. He obtained a good yield of Bi^{204} but found no alpha emitters. He found an approximately 40-hour lead that may be 52-hour Pb^{203}. Perlman suggested that we should look for Pb^{203} from the alpha branching of Po^{207}.

Miller then summarized the results of bombardments of copper with 200 Mev deuterons and 400 Mev helium ions--he said that a maximum or leveling off of the yield occurs at 12-15 masses out.

Summary of Cu + 200 Mev D^+ and 400 Mev He++

		Relative Yield (D^+)	Relative Yield (He^{++})	
$_{30}Zn^{62}$	9.5 h K	0.03		
Zn^{63}	38 m β^+	0.04		
$_{29}Cu^{60}$	24.5 m β^+	0.23	0.11	
Cu^{61}	3.4 h β^+, K	(1.0)	(1.0)	
Cu^{62}	10.5 m β^+	1.64	0.9	
Cu^{64}	12.8 h β^+, β^-, K	0.58	0.53	
$_{28}Ni^{57}$	36 h β^+	0.046	0.05	
Ni^{65}	2.6 h β^-	0.047	0.052	
Ni	30 d (β^+)	0.024	0.058	
$_{27}Co^{55}$	18 h β^+	0.03		tail > 70 d.
Co^{61}	1.8 h β^-	0.13		
$_{26}Fe^{52}$	7.8 h β^+	0.0014		
Fe^{53}	8.9 m β^+	0.033		
Fe^{59}	47 d β^-	0.066	0.051	
$_{25}Mn^{51}$	46 m β^+		0.025	
Mn^{52}	6.5 d β^+, K	0.12	0.12	
Mn^{56}	2.6 h β^-	0.10	0.07	
$_{24}Cr^{49}$	42 m β^+	0.007	0.007	
Cr^{51}	26 d K		0.024	
$_{17}Cl^{38}$	37 m β^-		0.003	

* * * * *

I checked today's mail, which contained a note from Charlie Price (Notre Dame), saying that the week of November 17 will be satisfactory for my lectures and that it is easy to go from Notre Dame to Washington, either by the Capitol Ltd. from LaPaz or by United Airlines.

Jerry Howland, at my request, prepared a list showing the prevalence of known cases of nuclear isomerism as a function of atomic number and of the type of nucleus.

From 8 p.m. until 10 p.m. I spoke on "Radiochemistry and the New Elements" at the University of California Medical Center in San Francisco. This intensive course, "Applications of Nuclear Physics to the Biological and Medical Science," began June 30 and will continue through July 18. It is designed to introduce doctors to the advances made in the medical applications of atomic physics over the last few years; lecturers include such men as Luis W. Alvarez, Joseph G. Hamilton, Isadore Perlman, Robert S. Stone, et al.

Wednesday, July 9, 1947

Iz and I attended a meeting called by Ernest Lawrence to review the status of the research and to determine what results could be sent in for publication. Others in attendance were Bob Serber, Ed McMillan, Eugene Gardner, Luis Alvarez, Burt Moyer, Bob Thornton, Duane Sewell, and Bill Brobeck. I described the status of eight different research programs in chemistry: 1. Radioisotopes of polonium and bismuth using the 60-inch and the 184-inch cyclotrons, 2. Fission of bismuth, 3. Neptunium (4n + 1) series, 4. Transuranium elements--including work done at Chicago, 5. Uranium fission with alpha particles, 6. Radioactive yields from 200 Mev deuterons and 400 Mev helium ions in several areas below bismuth (gold, arsenic, and copper) not including fission--this research, I added, will be discussed in my September talk before the ACS. 7. Fission cross sections, 8. Secondary reactions.

Part of my day was spent going over the material I shall present at the meeting of the American Physical Society at Stanford on Friday. In addition, I worked on the draft of "The Actinide Series," which I am preparing for Professor Sneed's Comprehensive Inorganic Chemistry.

Thursday, July 10, 1947

I worked quite a bit on my correspondence today. I answered a June 20 letter (missent to UCLA) from Paul F. Ellis (Science Writer, United Press), who asked for a few of my comments on the general topic of atomic energy, e.g., "Did the world lose or gain through the development of the atomic bomb?" Ellis asked for my remarks by July 7 for a story on the second anniversary of the release of atomic energy. In my reply I explained that I had just received his letter and said, "Since this is well beyond your deadline, I am not making an attempt to express my opinions on any of these matters. It seems to me that you have succeeded in formulating most of the '64-dollar questions' and I believe that I would find it difficult to say anything very sensible on most of your topics."

George Watt wrote on June 30 that he was invited to participate in a symposium at Stanford during the period August 4 through 7. He plans to come to Berkeley on the evening of the seventh and leave for Austin on the ninth and would like to see me, Perlman, Thompson, and others and visit the hill if possible. I replied that we are glad to learn that he will visit and will do our best to make arrangements for him to be able

to enter the "sanctum sanctorum" on the hill. Additionally, I said we were thrilled to learn of his twins and hope they will do all right.

I wrote to Welden Reynolds (Encyclopaedia Britannica) in response to his letter of July 1 and said that I shall be able to read the proof of my article, "Radioactivity, Artificial," shortly after I receive it. I also noted that May 1 of next year is probably a realistic date for the completion of my other writing assignments for the Britannica.

W. H Sullivan wrote from Chalk River on July 2, questioning the whereabouts of his copy of the "Table of Isotopes." Sullivan then asked for more details about the mass assignment and radiation characteristics of Co^{62}. In my reply today I explained that report, "Index No. BC-59," was sent to him at Oak Ridge on June 24 and a second copy was sent to Oak Ridge on June 30. I also gave him the information available on Co^{62}; I then asked him to check into the matter of the irradiation of our little sample of Am^{241} in the Chalk River pile, saying that we feel this nonfissionable material can be legally transported to Canada provided someone will make a decision to allow it.

I wrote the requested letter of recommendation for Truman Kohman to Frank Long at Cornell, saying that Kohman is a very good man with a strong bent toward the physical and theoretical side of nuclear science. If I were to deliberately pick out a weakness, I said, it would be an over tendency toward perfection both in his writing and in his attitude toward laboratory work. In comparing Kohman with O'Connor, I explained that they are completely different types--O'Connor is more of a practical chemist and less of a physicist and would probably be faster as a research man, but he is probably three or four years behind Kohman in overall development at this time.

I sent Leonard Katzin a copy of the letter I recently received from Herbert Eastlick (The State College of Washington), in which Eastlick explained that he believes Katzin is overqualified for the position at Pullman. I then asked Katzin when he plans to visit Berkeley this summer and suggested that, if he wants to visit the hill, he come equipped with a V-2 marked for "all areas including buildings 4 and 5."

A June 24 form letter recently arrived from Edward R. Murrow, announcing the June 30 radio broadcast of "The Sunny Side of the Atom." Helen and I listened to the program, and today I wrote Murrow that I heard the broadcast and that I thought on the whole it was well done. I also said that I thought that Ruth Ashton did a good job in gathering material for the report.

John Manley (General Secretary of the GAC) telephoned me and asked if I had any items that I thought should be put on the agenda of the GAC meeting at the end of the month. I said I would inquire around and write to him soon.

I received and returned a card to B. F. Starkoff (Handbook of Chemistry and Physics), which entitles me to a complimentary copy of the handbook with my name in gold on the cover because of my assistance in its preparation.

Friday, July 11, 1947

Iz and I went to Palo Alto to present two papers about the work of our group with the 184-inch cyclotron:

Transmutations with High-Energy Deuterons in the 184-Inch Cyclotron. B. B. CUNNINGHAM, H. H. HOPKINS, M. LINDNER, D. R. MILLER, P. R. O'CONNOR, I. PERLMAN, G. T. SEABORG, AND R. C. THOMPSON, *University of California at Berkeley.*—The successful operation[1] of the Berkeley 184-inch cyclotron to produce useful beams of deuterons and helium ions of approximately 200 and 400 Mev, respectively, has made possible the radiochemical identification of many of the transmutation products of nuclei excited to very high energies. Products with atomic numbers considerably lower than that of the target were observed in every case. Thus, with 200-Mev deuterons on $_{33}As^{75}$, activities were found representative of all elements from $_{34}Se$ to $_{25}Mn$ with lower elements not excluded. Both negative beta-particle and positron (or orbital electron capture) activities are noted, indicating reactions both of multiple neutron ejection and multiple charged particle ejection. For instance, with deuterons on $_{51}Sb$ (isotopes 121 and 123) both 6.7-hour $_{48}Cd^{107}$ and 2.5-day $_{48}Cd^{115}$ were produced. For the formation of light isotopes, several different reactions are possible, since in most cases the products will not be shielded from formation through a chain of short-lived positron (or orbital electron capture) ancestors following the ejection of different combinations of neutrons and charged particles in the primary reaction. As an example from deuterons on copper, the formation of a new 8-hour iron positron emitter assigned to $_{26}Fe^{52}$ implies that four units of charge and thirteen of mass are lost in arriving at this product.

[1] W. M. Brobeck *et al.*, Phys. Rev. 71, 449 (1947).

Transmutations with High-Energy Helium Ions in the 184-Inch Cyclotron. G. T. SEABORG, B. B. CUNNINGHAM, H. H. HOPKINS, M. LINDNER, D. R. MILLER, P. R. O'CONNOR, I. PERLMAN, AND R. C. THOMPSON, *University of California at Berkeley.* In continuation of the work reported in the previous abstract, uranium was bombarded with 400-Mev helium ions, and a wide range of activities was identified both in the heavy-element region and in the region of the fission products. Representative of the heavy isotopes observed were $_{88}Ra^{223}$ (AcX), $_{88}Ra^{224}$ (ThX), and $_{85}At^{211}$. The fission-product distribution is characterized by a much shallower dip between the maxima than occurs with slow-neutron fission, and the yields of the lightest among the observed fission-products are much higher. The various aspects of this work will be published more fully by the individual groups of investigators. This work and that reported in the previous abstract were performed with the support of the Atomic Energy Commission.

Saturday, July 12, 1947

At the laboratory today I answered a letter from R. C. Mason (Clinton Laboratories), who wrote on June 23 to express an interest in our plutonium sample, which has been irradiated at Hanford for about a year.

Mason asked if mass spectrographic measurements are planned for it since such measurements would give rather conclusive evidence about the cross sections, information needed for pile design. In my reply I explained that one of the purposes for which we have been irradiating the sample is the determination of the cross sections although we plan to do this by another method than the one he uses.

I talked with some of the fellows and then did some reading.

Sunday, July 13, 1947

Much of my morning was spent working on a draft of the talk, "Preparation of Radioactive Isotopes," to be presented at the University of Wisconsin Symposium on the Use of Isotopes in Biology and Medicine on September 10.

I spent the afternoon with Peter, Helen and Jeanette. Jeanette has recently begun to date a nice man, Eino Karpinen, whom she met at a dance at the Finnish Hall in Berkeley.

Monday, July 14, 1947

I checked some of the research and then replied to a July 7 letter from W. P. True (Chief, Editorial Division, Smithsonian Institution). I gave my permission for the inclusion of my article entitled "Plutonium and Other Transuranium Elements" [C & E News 25, No. 6 (1947)] in the General Appendix to the Smithsonian Report for 1947. I enclosed two reprints of the paper and a copy of the photograph of the microprecipitate of americium hydroxide.

Fred Reynolds prepared a memorandum on the two basic types of beta-ray spectrometers and the advantages and disadvantages of each, which I studied. Reynolds' opinion is that the lens type of spectrometer will be more suitable to our needs than the semi-circular type but he would like a meeting to discuss the requirements of such an instrument.

B. H. Willier wrote from Johns Hopkins, in a letter that arrived today, that they have no suitable openings for Leonard Katzin.

A telegram arrived later in the morning from George A. Baitsell (Yale University), inviting me to participate in the twelfth annual Sigma Xi lectureship series for 1948. I immediately wired back, "VERY SORRY I COULDN'T PARTICIPATE THIS NEXT YEAR BECAUSE OF EXTREMELY HEAVY SCHEDULE."

Tuesday, July 15, 1947

In attendance at today's group meeting were Asprey, Barton, Covey, Conway, Eyring, Garden, Goeckermann, Hicks, Huffman, James, La Chapelle, Lindner, Magnusson, Milton Moore, Morgan, Newton, O'Connor, Orlemann, Perlman, Redmond, Seaborg, Stewart, Street, Templeton, Werner, and Westrum.

Asprey reported that the last of the americium in the barrel has been purified from lanthanum by passing a solution through a nalcite resin column. X-ray spectrographic analysis of Am_2O_3 was unsuccessful because

the gamma-ray background of the americium interfered with the x-ray pattern. He described the results of the separation, and I said that we hope that element 97, if tripositive, may be eluted between Pu(IV) and americium.

Goeckermann talked about his recent bombardment of Bi^{209} with 200 Mev deuterons, in which only an iodine fraction was removed and iodine extractions were made from time to time during the course of several days on the dissolved bismuth. So far he has found only the 25-minute I^{128}, but he mentioned what he hopes to find. Perlman explained that we want to find out how the yield varies with mass number for a given element and if the yield is like that observed in slow neutron fission. In addition, we want to learn, Perlman said, if the yield of the 30-hour tellurium is different from that of its 8.0-day iodine daughter; i.e., do we find different yields of isobars or is the mass number alone the most important thing as in slow neutron fission.

Westrum announced that the new microcalorimeter has been tested by measuring the heat of solution of plutonium metal and the agreement with previous runs is good.

O'Connor talked about his bombardment of uranium metal with 400 Mev helium ions, in which chromium and tungsten fractions were removed. He found a 4-hour activity and a 26-hour activity in the chromium fraction and a 24-hour and a 70-hour activity in the tungsten fraction; the latter is undoubtedly molybdenum contamination. O'Connor said the purity of the uranium must be checked. I remarked that the work is of great interest since the heaviest activity thus far found in fission occurs around Z = 63 and the lightest at Z = 33. Here we have preliminary evidence for activities from Z = 24 to Z = 74. O'Connor told of some gold and osmium activities in a previous bombardment, and I remarked that apparently nearly every element in the periodic table may eventually be found in the bombardment of uranium with 400 Mev helium ions.

Morgan described some new ideas about the assignment of 12-hour americium (now assigned to Am^{240}) and 50-hour americium (now Am^{239}). From a study of excitation functions, he believes that the reactions may be written $Pu^{239}(d,2n)Am^{239}$ (12-hour) and $Pu^{239}(d,3n)Am^{238}$ (50-hour). I mentioned that the 12-hour activity has a 0.1% alpha branching and they should observe a Np^{236} beta particle if it is due to Am^{240} or the neptunium fraction should show K capture if it is due to Am^{238}. Failure to get either of these would show that the 12-hour activity is Am^{239}. James reported they found neither.

Magnusson reported that the extraction of Np(V) from a solution 1 M in HNO_3 and 9 M in NH_4NO_3 into ether is ≤ 0.003, and Perlman said that this is good proof that the oxidation state is less than VI. Magnusson also has studied spectrophotometrically a series of Np(V) solutions in which the total NO_3^- was constant at 9 M and the HNO_3 varied. In addition, he has done more work on the absorption band of Np(V) occurring at 981 μ.

* * * * *

In the mail was a note from Leonard Katzin, thanking me for my letter

of July 10 and announcing that he plans to be in Berkeley July 31, August 1, and August 2.

Later in the afternoon Bill Dauben and I went to the Richmond Golf Club for a round of golf: Dauben 104, Seaborg 117. Bill mentioned that he will be married on August 8 in Vancouver to Carol Hyatt, who has a degree in physics from the University of California. Bill said that Carol would like some part-time work utilizing her training. I told him that we want to hire someone with some scientific training to work as an assistant to Dave Templeton in his x-ray crystallography work. Bill thought that this would be ideal, and I promised to take care of the preliminaries.

Wednesday, July 16, 1947

Considerable time today was spent on my correspondence. In reply to W. H Sullivan's query of July 8 about a change of date for the next meeting of the Subcommittee on Nuclear Constants, I said today that any of the dates (October 1, 2, or 6) are satisfactory since there is an insufficient amount of difference between them for me to make a choice. Sullivan also asked for information about the reliability of the work under reference D9 in my 1944 table. To this I explained that Perlman and I have reevaluated the work under this reference in our recently issued "Table of Isotopes," making use of all our available knowledge in each case. Then, with regard to his question about Van Voorhis' communication on As^{72}, I said that we found a 9.5-day orbital electron capturing selenium from which there grows a 26-hour positron emitting arsenic in our irradiation of arsenic with 200 Mev deuterons; this does not confirm the mass assignment but does confirm the existence of a 26-hour arsenic as a separate entity from As^{76}.

I answered J. H. Manley's recent letter and suggested a few items that should be put on the agenda of the GAC meeting at the end of the month. I mentioned that a few points in the declassification area need to be clarified; i.e., use of organic solvents as a general method of separation in papers on basic chemistry and the fissionability of "unavailable" nuclei, such as AcX, Pa^{230}, etc., and the question of clearance delay due to the Patent Department.

I also replied to an inquiry of June 24th from Henri Corbière of Ville D'Anray, France, who is writing a book on "literary and scientific 'debut' of the scientists and writers of the whole world." The questions and answers were: 1. Was your scientific "debut" easy or difficult? Not too difficult; 2. Did means of living enable you to make yourself known in science, or did you live solely through your work in the laboratory? Lived solely as a result of scientific endeavor except for very earliest positions before starting at the University; 3. What work made you more famous? Which do you consider as your masterpiece? For both questions the answer would be work on the transuranium elements.

In a note to Jim Crowe (C & E News), I told him I shall arrive in Washington by plane about noon on Sunday, July 27, for a GAC meeting; I suggested that he and Bob Gould call me at the Statler Hotel around 5 p.m. on Sunday if they are interested in getting together for dinner.

I explained in a memorandum to George Everson about the changes we are instituting in our x-ray diffraction program: Dr. David Templeton is replacing Professor J. D. McCullough, who is returning to UCLA. We want to replace our present technician with someone with a more complete scientific background next fall. Therefore, we want him to send a PSQ form to Carol Hyatt, who has a master's degree in physics from Berkeley. Miss Hyatt, I explained, will soon be the wife of Dr. William H. Dauben of the Chemistry Department.

Helen had an appointment with Dr. Lois Wilson, her obstetrician, at 1:15 this afternoon. Everything seems to be in order.

Thursday, July 17, 1947

This morning I worked for a while on my talk called "Preparation of Radioactive Isotopes" for the University of Wisconsin Symposium on September 10th.

Another letter (classified) arrived from W. H Sullivan at Chalk River, in which he expressed much pessimism about our being able to irradiate a small sample of Am^{241} in the Chalk River pile. He will, however, try to work out the problems. In addition, Sullivan mentioned that October 6 is the date now planned for the meeting of the Subcommittee on Nuclear Constants in Washington.

As usual, I made the rounds to check on the status of the laboratory work.

Friday, July 18, 1947

After checking on the research, I continued to work on revisions to PPR chapters that Joe Katz and I agreed should be made. Later I answered a note from Maurice Goldhaber that I received today, in which he said that he and his wife will be in California from early August until about the third week of September and would like to see me. I told Goldhaber that I shall be in Berkeley throughout August and through the first week of September.

Another telegram arrived from George A. Baitsell, "REGRET YOUR INABILITY TO ACCEPT 1948 LECTURESHIP OFFICERS NOW INVITE YOU TO GIVE THE ANNUAL SIGMA XI ADDRESS AAAS MEETING CHICAGO DECEMBER 29TH HONORARIUM AND TRAVELLING EXPENSES PLEASE WIRE ACCEPTANCE." This is an important address, and I immediately wired back, "HAPPY TO ACCEPT INVITATION TO GIVE ANNUAL SIGMA XI ADDRESS."

In the minutes of the June 2 meeting of the Solvent Extraction Group at Argonne, I noted a discussion of the recovery of neptunium in the operation of the Redox Process. Today I wrote Herbert Hyman and said:

> I have grave doubts concerning your conclusion that plain (nitrated) Np(IV) is relatively insoluble in hexone. It seems to me that it would be amazing if Np(IV) is so different from Pu(IV) in this respect. Could you explain your results on the assumption that only the hydrazine-ferrous ion combination permanently reduced the Np(V)

to Np(IV), while in the case of ferrous ion alone and the hydroxylamine-ferrous ion combination, the Np(V) remained unreduced or the Np(IV) was reoxidized during the course of the experiments.

Saturday, July 19, 1947

At the lab this morning I looked over the typed version of "The Actinide Series" for the Comprehensive Inorganic Chemistry volume by Professor Sneed. After making a few changes and corrections, I made my usual tour of the labs.

Sunday, July 20, 1947

Bill Dauben and I went to Mira Vista for a round of golf (WGD-106, GTS-101) in the morning.

Later I worked on my writing projects for a while--the Wisconsin Symposium talk and my talk for the General Assembly at the ACS meeting in September.

Monday, July 21, 1947

I took care of some of my pending correspondence early this morning. I answered a June 26 letter from Edward Orban of Monsanto in Dayton. To his question of when Dr. Perlman and I plan to publish our book, I wrote that we plan to publish it with Prentice-Hall, Inc. at some indefinite time in the future but not before 1949.

John F. Flagg (General Electric, Schenectady) asked me in a letter dated July 14, for suggestions of a co-author for a monograph on the use of tracers in analytical chemistry, which he has been asked to prepare by Professor I. M. Kolthoff for publication by Interscience in their "Chemical Analysis" series. In my reply today I said that my first choice is Dr. David Hume (MIT), my second choice is Dr. Gordon Leader (University of Kentucky), and my third choice is Dr. David C. Grahame (Amherst College). I explained that, although Grahame's work in radiochemistry was done a number of years ago, he may have some available time since he is at a small college and, in addition, there is the advantage of geographic proximity. I told Flagg that I couldn't possibly undertake such an assignment because of my schedule.

I noted a letter from George Watt, who as a member of the Canvassing Committee for Award Nominations for the American Chemical Society, asked for a nomination for the ACS Award in Pure Chemistry.

Fred Albaugh, who is on his way to his new position at Hanford, and I got together and talked about his new job and various other things. Later I went around to see how the research was progressing.

Tuesday, July 22, 1947

Present at this morning's group meeting were Albaugh, Asprey, Barton, Conway, Covey, Cudaback, Eyring, Garden, Goeckermann, Hicks, Huffman, La Chapelle, Lilly, Lindner, Magnusson, McCullough, Morgan, Newton, O'Connor, Perlman, Seaborg, Stewart, Street, Templeton, Werner, and

Gifford Young.

La Chapelle described his apparatus to be used in the preparation of dry compounds of Np(V). I suggested that practice runs should be made on uranium in HCl, and La Chapelle said that he will first work with neptunium(V) chloride solutions, whose product may be NpO_2Cl. Conversion to $NpCl_5$ will then be attempted.

Lindner talked about his bombardment of antimony with 200 Mev deuterons, in which the silver, palladium, rhodium, ruthenium, technetium, molybdenum, and yttrium fractions were studied. The silver fraction contained only the 3.2-hour beta-emitting Ag^{112} and the 8.5-day K-capture Ag^{106}. The palladium fraction was studied by milking out the daughter activities and showed the 40-second I.T. Ag^{109*}, whose parent is the 13-hour beta-emitting Pd^{109}. Lindner saw the 3.2-hour beta-emitting Ag^{112}, daughter of 21-hour beta-emitting Pd^{112}, in yield so as to show independent formation of the isobars of mass 112, also true in the case of mass 109 where Cd^{109} and Pd^{109} have been identified. A 7-8-day silver daughter of palladium was found, probably the 7.5 day beta-emitting Ag^{111}. The rhodium daughters were examined by precipitating out the palladium and silver and mounting the supernatant. He saw Sullivan's 24-hour Rh^{100} and a 5-6 day Rh^{101}; the second milking gave only the 5-6 day activity indicating its Pd^{101} parent was longer-lived. The palladium fraction decay curve also showed a long-lived x-ray emitter that may be 17-day Pd^{103}. The ruthenium fraction showed the 4.5-hour beta-emitting Ru^{105} and its 36-hour rhodium daughter; this is the lowest mass number positively identified. The molybdenum fraction contained 3-hour and about 3-day activities.

I summarized the results from the recent Hanford americium bombardment (51NB): 4 mg of Am^{241} were irradiated in the center of a pile for 15 months. About 15% transmuted to Am^{242}, decaying by 18-hour beta emission to 5-month Cm^{242}, about 2/3 of which decayed to Pu^{238} and about 1/3 remains--about 10^{12} c/m; about 1% fissioned; ?% transmuted to long-lived Am^{242} and higher isotopes of americium and curium. Using the remote control apparatus in the hot lab, Thompson dissolved the sample in HNO_3, then neutralized 3/4 of it with NH_3 and precipitated $Am(OH)_3$. This was dissolved to a known HNO_3 concentration, oxidized with $NaBiO_3$ and precipitated with $BiPO_4$--this should contain the 97 if +4 valence--and Cunningham and Thompson are attempting to separate the components with a Nalcite column. The supernatant from the $BiPO_4$ precipitation should contain Np(VI), Am^{+3}, Cm^{+3}, and the 97 if +3 valence. This too will be column separated, and Werner will take enough fractions to get 90% of the curium, James will take a sample (fraction 68) to study curium isotopes, Morgan and Street will study americium isotopes (from a sample from fraction 85). Thompson and Cunningham will take some of fractions (43 to 66) and put the fractions through another column. Morgan and Street are working on the detection of the long-lived Am^{242} by looking at its 2-day Np^{238} alpha-decay daughter and its subsequent Pu^{238} daughter.

* * * * *

Later, as I often do, I stopped in the Department of Chemistry office to check in with Miss Kittredge and to talk with some of my colleagues.

Back on the hill I answered a July 17 letter from my old friend Cliff Garner at UCLA, who wrote to ask suggestions for an electronics technician. In my reply I said that it may be difficult to find someone at the salary of $300/month that they can offer although Southern California may be a counterbalancing attraction. I suggested the names of Howard Parsons, C. J. Borkowski, and Jack East at Clinton Laboratories, Alan Jarrett, who now lives in the Los Angeles area, and Chuck Blanchard, who is also in the Los Angeles area (whose address he can find from someone at Oak Ridge).

Recently Walter J. Murphy (Editor of Analytical Chemistry) sent me a paper, "Purity of Radioisotopes Used as Tracers," for review. Today I wrote Murphy and said that I believe the paper is not worth publishing and is written in a naive manner, suggesting that radioactive tracers should be tested for impurities--this should be obvious, I wrote to Murphy. I then suggested that, if a general review paper on the question of radioactive impurities in radioactive tracers is worthwhile, Murphy should contact Dr. P. C. Aebersold (Chief, Isotopes Branch at Oak Ridge) and ask for suggestions for someone to prepare an authoritative article.

The following letter arrived from David Lilienthal, posing the question, "What exceptions from the civil service laws are necessary to the discharge of the Commission's responsibilities for the efficient administration of the atomic energy program?" He wants the GAC to consider the question for the next GAC meeting and supplied some specific applications of civil service rules for our information.

GTS

UNITED STATES
ATOMIC ENERGY COMMISSION
WASHINGTON 25, D. C.

Noted GTS 7/22/47
Action
Recd. JUL 22 1947 GTS
Ans.
File S-A-C

July 14, 1947

Professor Glenn T. Seaborg
Department of Chemistry
University of California
Berkeley 4, California

Dear Professor Seaborg:

The Atomic Energy Commission has before it a question of great importance and difficulty on which it would like to have the advice of the General Advisory Committee. The purpose of this letter is to present the question for consideration by the individual members of the Committee prior to the Committee's next meeting in Washington on July 28, when joint consideration can be given it.

As the members of the Committee no doubt know, Section 12(a) (4) of the Atomic Energy Act provides that the Commission shall appoint its employees and fix their compensation in accordance with the civil service laws "except that to the extent the Commission deems such action necessary to the discharge of its responsibilities, personnel may be employed and their compensation fixed without regard to such laws." The Commission is now considering the question: "What exceptions from the civil service laws are necessary to the discharge of the Commission's responsibilities for the efficient administration of the atomic energy program?"

In asking the Committee's advice upon this question, the Commission does not wish to impose upon you the task of becoming civil service or personnel experts. We seek rather your judgment as scientists and as men of administrative experience who are very familiar with our operations. We recognize also that some of the members have observed the federal civil service system in operation and have made some appraisal of its effectiveness, while others may have some impression of its workability through direct or indirect experience with other governmental activity.

The Commission has already established some major premises in considering this question which will perhaps help provide a frame of reference for the members of the Committee.

The first of these premises is that the Commission fully endorses the major objectives of the civil service laws; namely, that employees shall be selected and advanced on the sole basis of merit, that personnel policies shall be objectively stated and administered without favoritism or caprice, that employee rights (including preference for veterans) shall be scrupulously recognized. The question, therefore, is not about objectives but about the adequacy of civil service methods to accomplish these objectives in a manner which assists the Commission in the performance of its responsibilities for the atomic energy program.

The second premise is that the Commission's responsibility for its program and its unique problems will make necessary some exceptions from the civil service laws and regulations. The question still to be decided is how wide the exceptions shall be to provide the Commission with a personnel program which will insure the selection and retention of a staff equal to the demands of the atomic energy program. The third premise is that our personnel policies must serve and assist our whole program of administration, not only in our own staff operations but also in our relationships with contractors and in our management of the atomic energy towns of Oak Ridge, Los Alamos, and Hanford.

The general question of the Commission's future relationship with the civil service system can perhaps best be considered in terms of some specific applications of civil service rules to agencies within the system.

1. Recruitment and Selection from Civil Service Registers. On May 1, 1947, new civil service rules were issues which supersede the relatively flexible war-time procedures. Under these new rules, appointments to all classified positions (including positions held by the war-time employees without civil service status who are subject to displacement) must be made from civil service registers. The Civil Service Commission or an agency committee under the supervision of the Civil Service Commission establishes such a register by the procedure of announcing an examination, receiving applications from all candidates who meet the specified qualifications, determining a numerical score for each candidate's training and experience by examining his application, and establishing from such numerical scores a ranked register. The civil service examining procedure emphasizes quantity rather than quality of training and experience. Selections from ranked registers must be made

in accordance with the "rule of three" which limits the agency's or supervisor's choice to the top three available eligibles on a register.

2. Classification and Compensation. Compensation rates under the civil service system are determined by classifying positions in salary grades in terms of organizational status and level of difficulty and responsibility. Scarcity of qualified candidates in particular fields, the professional potential of individual employees, and competitive salaries offered by other employers are not recognized factors. The duties of a position, as recorded in an official job description is the sole factor in grading the position. The main objective of the classification procedure is to insure equity (ordinarily seen as uniformity) among federal salaries.

3. Advancement and Recognition. For advancement to a higher salary grade an employee must meet qualifications requirements established and administered by the Civil Service Commission. These requirements are quantitative rather than qualitative, and are in terms of length of service in the next lower grade. The minimum time requirement increases for the higher grades to as much as two years. Salary increases within the grade are given periodically (each 18 months for employees in higher grade positions) and are limited to one salary step of about $250.00. An additional increase of one step may be given for superior accomplishment, subject to special justification and review based on procedures approved by the Civil Service Commission.

4. Efficiency Ratings. Under the civil service system every employee in the classified service must be "rated" annually on a uniform service rating form and under regulations prescribed by the Civil Service Commission. The form consists of 31 elements, ranging from such general personal traits as "industry" to such supervisory elements as "ability to make decisions." The practice is to establish a pattern of elements for each class of positions and to rate each employee on each element in the pattern as "outstanding," "adequate," or "weak." These ratings are then translated, by mathematical formula, into an over-all adjective rating of the employee on a five-step scale -- "Excellent," "Very Good," "Good," "Fair," "Unsatisfactory." The ratings are a factor in promotion, salary increases and retention of employees.

5. Reduction in Force. When staff of an agency is reduced in size, the civil service rules require that employees who occupy interchangeable positions be laid off in terms of their numerical scores on a retention register. Such a register is constructed by first placing each employee in a preference category, the highest being a veteran with civil service status and the lowest a non-veteran employee without status; scores are then assigned in relation to seniority, with minor weight given to the latest efficiency rating.

The Commission would like to have your judgment about the applicability of the above practices based on your knowledge of AEC problems and your familiarity with AEC working conditions and staff requirements. There are, of course, other aspects of civil service management on which the Committee may wish to express its judgment. It is invited to do so. The above aspects were chosen because they are both illustrative and of central significance.

The Committee, being composed of members whose great interest has been in science, may at first feel inclined to confine its judgments to the problems of scientific personnel. The Commission urges you to include in your considerations, however, the problem as it relates to all personnel, especially to those administrators and staff groups who create the environment in which the scientists do their work.

It would be difficult to overstate the importance which the Commission attaches to this problem. We are convinced that the inescapable responsibilities of the Commission require that we be staffed with persons of the greatest competence, managerial skill, and motivation. The method of contracting for research and production operations does not in any way diminish our responsibility. Obviously technical and managerial skills of the highest order are needed in the employ of both the contractors and the Atomic Energy Commission. We are concerned about our needs for scarce personnel highly valued by other employers and recognize the necessity of positive steps to make AEC employment increasingly attractive to those of the highest talents.

We look forward to your comments at the Washington meeting and shall value your considered judgment on this whole matter.

Sincerely yours,

David E. Lilienthal
Chairman

Wednesday, July 23, 1947

I mailed to Professor M. C. Sneed "The Actinide Series" for inclusion in his Comprehensive Inorganic Chemistry. I then wrote to Joe Katz to send him a copy of the Sneed chapter. In addition, I mailed a corrected copy of Chapter 1, the introductory chapter, (Volume 14A, PPR) to Joe and told him that I shall revise Chapter 2 on the radioactive isotopes of plutonium as soon as I receive a report copy from him; however, I shall wait to revise Chapters 21 and 22 on the nuclear and chemical properties of the transplutonium elements because the current work is so applicable. I reminded Joe that papers 22.1 ("The New Element Americium") and 22.2 ("The New Element Curium") of Volume 14B will be revised to include the work of the next few weeks. Helene Voyer, I said, will soon begin checking references on the next chapter.

Thursday, July 24, 1947

Again, much of my day was spent on my writing projects. I prepared an outline for a rather popular article to be called "The New Synthetic Elements" for the journal, Technical America. This is something that I promised the editor, Beryl Gilman, in April that I would do, but I haven't had time to even think about it before today.

Friday, July 25, 1947

I took care of some administrative matters and then worked for a while on some revisions to the PPR papers. Later I looked in on the research going on in the labs.

Saturday, July 26, 1947

I spent the morning at the lab, looking in on the men and their experiments, particularly the work on the fractions from the Hanford americium bombardment.

In the mail today I received a notice that Rayburn W. Dunn has been hired, effective July 1, to work under Huffman's direction in Donner Laboratory.

Later I went to San Francisco, took a limousine to the airport, and caught United Airlines Flight 610. In Chicago we were transferred from our DC-6 to a DC-4 plane, which delayed our arrival time in Washington.

Sunday, July 27, 1947

I arrived in Washington about noon and took a limousine from the airport to the Statler Hotel, where I checked in.

Later I met Jim Crowe and Bob Gould of the American Chemical Society and had dinner with them.

Monday, July 28, 1947

In Washington. I took a taxi from the Hotel Statler to the GAC meeting. Robert Oppenheimer convened this session at 9:10 a.m. with all

members present except Enrico Fermi and Hood Worthington. John Manley, Anthony Tomei, and LaPlante (a security man) were also present. Oppenheimer said the only topic the AEC put on the agenda was the matter about personnel policies (letter from Lilienthal of July 14). Oppenheimer suggested that we might also discuss the weapons test program, the complex of security questions, and the question of balance of non-programmatic research. At 9:30 a.m. the GAC was joined by David Lilienthal, Lewis Strauss, William Waymack, Carroll Wilson, Lyle Belsley, Fletcher Waller, and Richard Niehoff. Wilson began by pointing out the difficulty of obtaining key personnel, informing us that W. E. Kelley has been made Area Manager of New York Directed Operations, Carroll Tyler of Los Alamos Directed Operations, and J. C. Franklin of Oak Ridge Directed Operations, but that other important positions remain unfilled--Area Manager for Hanford and an AEC Director of Information. Most of the meeting was taken up with the matters of Information and Personnel, with Oppenheimer expressing the view that the AEC should make a public statement giving "a realistic evaluation of atomic power," and we agreed that a statement would be drafted for discussion with the Commission. Waller outlined some problems about defining a personnel policy, and Lilienthal asked whether the special circumstances of the AEC operations are such as to require a tailor-made personnel administration. The Commissioners and staff left at about 10:35 a.m.

In the continuation of our meeting, we talked about the membership of the Reactor and Hazard Committee, whose appointments we recommended at our last meeting. Since two of the suggested members refused the appointment, we added the following as possibilities; Joseph W. Kennedy (Washington University), Hood Worthington (du Pont Company), John C. Warner (Carnegie Institute), John R. Ruhoff (Mallinckrodt Chemical Co.), and Roger Williams (du Pont Company). We then talked about future tests of atomic weapons in the light of the decision to establish a proving ground in the Pacific area. Oppenheimer stated that he feels, in addition to proof testing present stockpile weapons, it is time to make plans for more complicated tests to give more detailed information on the behavior of present and future weapons and to obtain data essential for the design of thermonuclear weapons. Over the lunch hour we continued talking about the nature and content of the statement on atomic power that we are to prepare.

At 2 p.m. we were joined by Lilienthal, Sumner Pike, Waymack, Strauss, Wilson, James McCormack, and James Russell in order to discuss the Los Alamos problems. The discussion revolved around the secrecy aspects--or even top secret aspects--of the weapons test program and the effect that this, and other considerations, has on the lack of enthusiasm at Los Alamos for a test program. The people there are disturbed by the lack of any definite news on the test program. The need for long-range planning in the matter of tests was emphasized.

We returned to Executive Session at 2:55 p.m. and continued talking about the draft of our statement on the feasibility of atomic power. After a number of suggestions, Oppenheimer agreed to redraft the statement for presentation to the Commission at tomorrow morning's sessions.

Starting at 4 p.m., the GAC again met with members of the Military

Liaison Committee. Lieutenant General Louis H. Brereton served as chairman for this meeting, which was also attended by Rear Admiral Thorvald A. Solberg, Rear Admiral Ralph A. Ofstie, Major General L. R. Groves, Colonel John H. Hinds, Lieutenant Colonel R. R. Ploger, and Commander F. L. Ashworth. Brereton asked about the GAC's recommendation concerning the application of the "de minimis" principle to weapons; Conant replied that this is to discourage great effort towards a small increase in the yield of weapons, and Oppenheimer remarked that it does not relate to reliability but to performance. Brereton then asked for an evaluation of the Redox Process and its effect on plutonium production; I replied with an estimate of the elongated time scale for installation. I said I see no difficulty if personnel can be procured. Groves suggested that the MLC might meet with Worthington and me for additional discussion of this process. Brereton then asked about the production of polonium for initiators, and Oppenheimer replied that investigations are under way of possible substitute materials. The next question concerned our recommendation against further consideration of a penetrating weapon, to which Oppenheimer and Conant responded that the absence of any evidence for the need of such a weapon suggests a low priority for it. Groves asked why more than one weapon assembly needed to be exploded considering the great concern over expenditure of fissionable material. Oppenheimer's response was that there are two real needs: (1) to make good use of existing material ratios in the stockpiling of weapons and (2) to assure a proven stockpile. Before adjourning at 5 p.m., we also discussed the question of reducing the weight of the present weapons.

Then, for the next hour and a half (until 6:30 p.m.), we worked on Oppenheimer's preliminary draft of our statement on atomic power. After dinner, we reconvened in Executive Session at 8:20 p.m. and again worked on our statement on atomic power. We also talked about our recommendations concerning classified areas and declassified subjects, plus the problem of Clinton Laboratories. We concluded that we shall express no opinion on the matter of the takeover of the management of Clinton Laboratories by the University of Chicago. This session concluded at 10:20 p.m.

Tuesday, July 29, 1947

In Washington. I again took a cab to the AEC Building for the 9:10 a.m. session. The letter of the General Advisory Committee on Personnel Policy follows:

Letter of the General Advisory Committee
on
Personnel Policy

July 29, 1947

The members of the GAC have considered individually and together the questions raised in Mr. Lilienthal's letter of July 14 concerning the adaptability of the Civil Service to the Commission's personnel problems.

Without pretending competence to advise the Commission on the administrative problems arising from operating under Civil Service or setting up separate personnel arrangements, the GAC does recognize very clearly the novel if not unique features of the AEC personnel problems. We also believe that an adequate and satisfactory system of handling these problems is basic to the success of the whole AEC program. Never before, we think, has a Government agency been assigned a task so full of technical and administrative complexities, and at the same time fraught with so many possibilities of both beneficial and disastrous effects on the national welfare.

The tasks of the Commission cannot be accomplished simply by having a certain number of people. They can be carried on at all only if they attract some of the greatest minds and finest talents in the country and they must be chosen to work together as a team. Even the nature of some of the tasks cannot be specified or foreseen. The number of qualified and experienced men and women in this field is extremely limited in the face of a tremendous demand for their services.

Consequently, we believe that at this stage it would be disastrous for the AEC to attempt to freeze personnel policies and procedures into any pre-existing pattern. The maximum of flexibility must be retained in selection, assignment, promotion, and transfer of personnel. No rigid system of scores, examinations, efficiency ratings can possibly be depended on to select the proper combination of competence, experience, responsibility, personal integrity, and loyalty which will be required for most of the Commission's key positions.

We therefore recommend that the AEC establish its own set of broad and flexible personnel policies and that these be followed until such time as experience shows that these can be made more specific and more definite. Eventually jobs may be more definitely describable, qualifications and ratings more clearly codified, and then it can be determined whether Civil Service procedures can be adapted to AEC use.

Finally we must point out that we know that many of our scientific colleagues are unwilling to accept, under present conditions, Civil Service appointments. Whether they are right or wrong in this the fact remains that their services are essential to the Commission's program and ways must be found to attract them into AEC positions. The same is true of many non-scientists. In any case a mixed set of personnel policies in any particular agency will hardly be conducive to the attainment of that extremely high state of personal morale required to carry on so difficult a job.

For the Committee

J. R. Oppenheimer, Chairman

At this session all the members of our committee were present as well as Waymack, Wilson, Walter J. Williams, Fisk, Colonel Kenneth E. Fields, Ralph Johnson, Belsley, and Spofford English. The meeting began with Fisk talking about the situation at Clinton Laboratories. He traced the history of the attempts to find a suitable contractor for Clinton, leading up to the present negotiations with the University of Chicago, which appear to be acceptable to both parties. I expressed some dissatisfaction with the present situation because of the impact it will have on the future problems of the plutonium separation process, which will require a closer collaboration among all parties than seems to be forthcoming from the present situation. Johnson led a discussion of the problems of the distribution of efforts in research in basic nuclear science in the Division of Research. He summarized the talks that have been held with the Office of Naval Research about their proposal to the Commission to support the completion of high energy accelerators. He raised the question of whether a large-scale support of accelerators was appropriate to the interest of the AEC, compared with the need for efforts in metallurgical and chemical research. English then summarized the situation about the distribution of tritium to research workers; we agreed this should be done with the proviso that the scale of production of this isotope should not be indicated.

Oppenheimer brought up the question of our draft statement on atomic power, and we agreed to transmit it to the Commissioners for their comments at our joint meeting. The statement read:

> In the course of our duties as advisors to the Atomic Energy Commission we have re-examined the situation as it appears today with regard to the generation of atomic power for industrial uses. It may be that this situation is not fully understood by our scientific colleagues and the public; and it therefore seems desirable to give a brief account of it. We are aware that in this field as in other new fields discoveries and inventions may make our present views obsolete--indeed,this is likely. Nevertheless, it is desirable that what is foreseeable on the basis of present knowledge be distinguished from hopes which are necessarily vague as to future changes in that picture.
>
> It does not appear hopeful to use natural uranium directly as an adequate source of fuel for atomic power. The reactivity of systems based on natural uranium is low; even the fraction of U^{235} which can be consumed without replenishing the fuel is small. Because of this the raw material requirements, if such reactors are to play an important role in power economy, are economically prohibitive. It is true that one could re-enrich this natural fuel, but the power expenditure involved in such isotope separation by any present methods makes this likewise prohibitive.
>
> For these reasons the hopeful approach for the future lies in the development of reactors which (a) generate power at high temperature, and (b) create more fuel from U^{238} or thorium than they consume. Although both of these requirements appear to be capable of being met in principle, neither has so far been realized in fact. An intensive development program will be required to achieve these ends. No one can estimate how long this program will take, but it appears to be

reasonable to us to anticipate a period of about 10 years before the series of very difficult metallurgical, engineering, and chemical problems can be solved.

Even when reactors can be designed and built along these lines it must be anticipated that the fractional increase of nuclear fuel per year will be quite small. It can be increased by increasing the specific power of the reactors (that is the rate of energy generation per unit of nuclear fuel invested), by maintaining a good neutron economy, and by a good system of chemical techniques to prevent the loss of material when it is removed and reprocessed. This chemical requirement again demands both a long time between reprocessings, and high standards of chemical operation to reduce losses in an operation which will of necessity involve much recycling of material. These many problems, all of which involve their own difficulties, must be solved together if a reasonable economy of nuclear fuel is to beachieved. To date none of them has been solved at all.

When and if these problems are solved, the generation of industrial power may indeed become profitable, but it will still be on a small scale. Even according to the most optimistic estimates the rate of accumulation of nuclear fuel by regeneration will be slow. Thus even if the years of development are also used to accumulate a substantial initial stockpile of nuclear fuel, and even if favorable technical answers appear for the designs of these reactors, decades will elapse before stocks of nuclear fuel can have accumulated which will supplement in a significant way the present power resources of the industrial nations of the world.

Other items discussed at this session were the plans to increase the production of initiators through the use of the new facility at Dayton, which will be in operation by about June 1948, and the situation at Hanford: two replacement piles, the enlargement of the town, the water treatment plant, the Redox installation, and additional school facilities. There was a brief amplification of the Redox program before we adjourned at 12:45 p.m.

We then reconvened at 2:15 p.m. with all members of the GAC present, as well as Manley. We discussed further the nuclear weapons test program, agreeing that one of the main items to be included should be thermonuclear experiments designed to test the existence of a thermonuclear reaction by observation of the effects produced. Another conclusion was that we do not feel that this is the time to inject the development of the decrease in size of the weapon into the Los Alamos program because of the difficulties involved.

At 3:30 p.m. we were joined by Lilienthal, Waymack, Strauss, Pike, Wilson, Fisk, McCormack, Williams, and Belsley, as well as General Brereton and Admiral Parsons. Oppenheimer summarized our opinions about nuclear weapons tests, the problems with reactor development, the civil service question, the pattern of research distribution, and our statement about atomic power. The following discussion was concerned primarily with our statement on atomic power, which Strauss felt was very strong and so pessimistic that it will adversely affect the ability of the Commission to operate, for example, in regard to the use of public

funds. Waymack said the statement will be meaningless to the general public and will not result in a gain of education toward the understanding of atomic energy problems. Pike remarked that this means the Commission is on trial and to say now that atomic power is very far distant means that we now have, in atomic energy, only a military tool. Lilienthal said the last paragraph holds no hope for peaceful uses of atomic energy. Strauss suggested that it would be prudent to consider the statement in the interval between now and the next meeting of the GAC; we agreed to this suggestion. Finally, Fisk said that he thinks the real shock comes from the implied situation about the non-existence of adequate raw materials for real commercial exploitation of atomic energy unless fuel can be obtained through breeding and utilization of fuels other than U^{235}. The joint meeting ended at 5 p.m.; we went into Executive Session at 5:10 p.m. for a further discussion of our statement, considering the implications and the factual basis of the draft. We agreed that Oppenheimer will prepare a new draft incorporating some of the suggestions and changes. The meeting was adjourned at 6:20 p.m., and I went to the airport to catch a flight home.

(A copy of Oppenheimer's July 29 report to Lilienthal on this Fifth Meeting of the GAC follows.)

Wednesday, July 30, 1947

I arrived back in San Francisco about 1:30 this morning and took the limousine to downtown San Francisco, where I got a ride to Berkeley.

At home Helen reported that she is feeling well and that young Pete is doing fine. I found a notice from Robert Underhill that my salary for the year ending June 30, 1948, has been fixed at $8,100.00.

When I went to the laboratory, I noted a letter from W. P. True, who thanked me for my permission to reprint my Harrison Howe Lecture, "Plutonium and Other Transuranium Elements," in the 1947 Smithsonian Report. True said that I will receive proof later in the year.

A letter was also awaiting me from John F. Flagg (General Electric, Schenectady), thanking me for my suggestions for a co-author for his monograph. Flagg said he plans to contact Dr. Hume, but that he considers David Grahame, whom he knows, as a good second choice.

The following telegram was received from Norris Bradbury at Los Alamos:

> WOULD APPRECIATE YOUR CONFIRMATION OF THE FOLLOWING: WE HAVE HEARD UNSUBSTANTIATED REPORT THAT EASTMAN KODAK CO FURNISHED YOU, WITH THE AID OF LT COL FRIEDELL, SOME SUPPOSEDLY RADIOACTIVE STRAWBOARD OR STRAW WHICH MIGHT THEN HAVE BEEN SUBJECTED TO ANALYSIS BY YOU. THIS OFFICE IS DESIROUS OF OBTAINING COPY OF YOUR FINDINGS IF DOUBT EXISTS AND IF THE ANALYSIS DID TAKE PLACE. IT WAS RUMORED THAT EASTMAN KODAK FURNISHED THIS AFTER THE TEST AT ALAMOGORDO.

I signed a memorandum to George Everson, requesting that he place Thomas Hicks on the payroll full time as of July 28. I explained that Hicks, a chemical engineer, has been working under Dr. T. Vermeulen on a half-time basis on the Chemistry Group payroll.

UNITED STATES
OMIC ENERGY COMMISSION
WASHINGTON 25, D. C.

July 1947

Mr. David E. Lilienthal, Chairman
U. S. Atomic Energy Commission
Washington 25, D. C.

Dear Mr. Lilienthal:

The Fifth Meeting of the General Advisory Committee was held in Washington on July 28 and 29, 1947. Two members of the Committee, Dr. Fermi and Mr. Worthington, were unable to attend.

This report on our considerations can be brief since many of them we had occasion to discuss with the Atomic Energy Commission during the course of the meetings.

I. A. In the report of our Third Meeting we recommended on advice of the Director at Los Alamos that the amount of active material in FM units be frozen DELETED It appears that more recent studies at Los Alamos suggest that it is both profitable and safe to DELETED DELETED We do not wish our earlier recommendation to be construed as opposing this procedure; on the contrary, we recommend that it be explored.

B. We have discussed briefly some considerations bearing on the forthcoming test program. We concur entirely with the plans which would make of the forthcoming tests a materials proof, without elaborate instrumentation, and devoted primarily to establishing the performance of stockpile items. We very much hope that the weapons can be tested in the order of decreasing novelty; that is, we should recommend that the DELETED 25 weapon be tested first and the composite levitated weapon second. Should performance of these two be satisfactory, they should constitute the basis for our stockpile; and no further tests at that time need then be undertaken. Clearly, provision should be made for holding other tests in the event of unsatisfactory performance on either of the first two.

Ltr 7/5/1

We regard it as quite likely that future development at Los Alamos will make possible the design of far more effective weapons. We also think it likely that tests giving more information about the detailed performance of atomic explosives will be required for guidance in future weapon development. We should therefore suggest that plans be now undertaken for instrumenting more far-reaching, more detailed tests and tests of more radical weapons perhaps two years from now. To this end it would seem desirable to establish a permanent test section of the Los Alamos laboratory, and to start preparations for the instrumentation of the second series of tests, more or less concurrently with the execution of the first series. As examples of desirable instrumentation whose feasibility seems assured, we would mention the measurement of multiplication rate, of initiation time, and of very fast neutron intensity. We hope that the testing and study of atomic explosives will become an integral and stimulating part in the work of the Los Alamos laboratory.

II. A. We note that two men suggested by us for the Committee on the hazards of reactors have not been willing to serve. We should like to add a few names in the field of chemistry and chemical engineering:

Dr. J.W. Kennedy, Washington University
Mr. Hood Worthington, DuPont Company
Dr. J. C. Warner, Carnegie Institute
Dr. John R. Ruhoff, Mallinckrodt Chemical Company
Mr. Roger Williams, DuPont Company

We hope that from these and our first suggestions an adequate committee can be selected.

B. We should like to recommend that the Commission appoint a panel to assist its Research Division by studying the problems of support of unclassified basic nuclear science. Specifically, we should suggest that this panel examine the current activities of the Commission, the requests made of it for support and grants-in-aid, and the associated activities of the Office of Naval Research, on the basis of specific budgetary allocation. We believe that after this has been done it will be possible to fix a budget for the support of this work more reasonably than can at present be the case. The General Advisory Committee would be glad to recommend possible members for this panel.

C. We should like to recommend that the difficult, but in our opinion, pressing questions of the establishment of non-secret fields of study, and, if possible, non-secret areas of work, within certain of the Commission's laboratories be explored with the laboratory directors during the forthcoming meeting of these directors on August 18th. As we previously said, we regard the establishment of such non-secret areas not merely as an aid to the furtherance of the work, but as a positive improvement in security.

III. We have studied Mr. Lilienthal's letter on the applicability of the Civil Service system to the personnel of the AEC. Our views on this matter necessarily reflect no technical competence in personnel problems. Nevertheless, we were quite unanimous in recognizing the serious and perhaps fatal hindrance which conformity with the Civil Service procedure might make for the work of the Commission. We attach a letter which we have written in answer.

IV. As we have reported at earlier meetings, we have been much concerned that the expectations of many of our colleagues and the public generally with regard to atomic power would not in any way correspond to a reasonable evaluation of future prospects. Time has only convinced us of the seriousness of this situation. We see countless illustrations, many of which we had occasion to discuss with you, where misapprehension on this matter complicates and perhaps embitters the relations of the Commission with many other agencies with whom it must deal, and with industry and the public. We have therefore attempted to draft a rough statement of our own views in this matter, revealing as little of classified information as possible and conforming as scrupulously as possible to what we know of the facts. We should propose that this draft be discussed with competent workers within the AEC in order to establish whether our views in the matter correspond to those generally held and in order to explore whether we have omitted any hopeful possibilities. Should it emerge that the situation really does conform to our evaluation of it, we would see many advantages in releasing a carefully edited draft statement along the lines of this preliminary one which we append. We should wish to do this only insofar as it conduced to the health of the Commission and only in such a way as

to be helpful to the Commission. In any case, we believe that it is essential to prepare a sound evaluation of the prospects of power, and to attempt to make this evaluation generally available.

V. At the request of the Military Liaison Committee, and with the concurrence of the Commission, the GAC met briefly with the MLC and discussed certain questions of their raising. In general, no considerations beyond those which we have already communicated to the Commission were developed during this meeting. We were asked to give the reasons for some of our recommendations on weapon development, on the operation of reactors at Hanford, the prospects for Redox, and on the possibility of obtaining polonium from natural sources. We discussed briefly the proper nature of the forthcoming tests, and the advantages, from the point of view of using Services, in reducing the weight of the present weapon. In subsequent discussion, the Advisory Committee agreed that although a substantial reduction in weight - at some unknown sacrifice in yield - could doubtless be achieved by a major development program, and although this would undoubtedly be of very great importance to the Services, the injection of this problem into the Los Alamos program at the present time would have highly adverse effects on the work of the laboratory.

Many matters were touched on in this meeting of the Advisory Committee to which we shall have to return at future meetings. We hope that ultimately we shall be able to give useful advice to the Commission.

J R Oppenheimer

J. R. Oppenheimer
Chairman
General Advisory Committee

I also wrote a note to Robert Gould (C & E News) to send him a copy of the fission paper and a copy of the 4n + 1 paper for his use.

This afternoon Helen had an appointment with Dr. Lois Wilson, who confirmed Helen's diagnosis that she is doing well (and feeling fine).

Thursday, July 31, 1947

The following were in attendance at our group meeting this morning: Asprey, Ballou (who has just arrived from Chicago to spend a postdoctoral year here), Covey, Cudaback, Eyring, Ghiorso, H. Hicks, Huffman, La Chapelle, Lilly, Lindner, Magnusson, McCullough, Miller, Moore, Morgan, Newton, Perlman, Seaborg, Stewart, Street, Werner, and Westrum. Werner spoke first and gave an extensive report on the processing of the curium fraction of the latest neutron bombardment of Am^{241} in the Hanford pile. He accounted for 88.85% of the curium. I commented that since Cm^{242} has about the same number of x-rays per alpha particle as Am^{241}, then per unit weight it will have about 1,000 times as many x-rays as americium and it will be difficult to do crystal structure work with x-rays. There was then a discussion about how to make the x-ray diffraction analysis.

Morgan talked about the preliminary results obtained with Street on the counting of a purified americium sample (by ion exchange and fluosilicate cycles) from the latest Hanford americium bombardment (51NB) compared with data from the five-month bombardment (51NA) and an Am^{241} standard. They have determined the relative amounts of Cm^{242} formed with the hope of determining its loss due to n,γ and fission reactions. I said the calculations look like an n,γ cross section for Cm^{242} of some thousands of barns. Morgan then discussed the cross section for destruction of Am^{242} by fission, comparing 51NB with 51NA. If the cross section for fission is as high as 5,000 barns, as the data indicate, then the cross section for formation from Am^{241} might be as low as 50 barns, and corresponding half-lives for the long-lived Am^{242} isomer are 60 years for beta decay (as determined from the amount of Cm^{242} daughter) and for alpha decay 3×10^4 years (as determined from the amount of Np^{238} daughter). More fission measurements on the recent samples are needed to clarify the data. I said that, if Cm^{242} has a capture cross section of some thousands of barns, more than 10% should have transmuted to Cm^{243}; but since the half-life of Cm^{243} should be 100 times longer than that of Cm^{242}, Cm^{243} will be difficult to observe.

Lindner presented data on relative yields of some isotopes from his 200 Mev deuteron bombardments of $Sb^{121,123}$. The results are on an atom basis, and In^{111} (2.4 d K) was taken as 1.0.
7

	Relative yield	Possibly Produced by	
$_{49}In^{113}$	48 d I.T.,β^-	2.	d,αp4n or d,αp6n
$_{48}Cd^{109}$	330 d K	0.2	d,2α6n or d,2α8n
Cd^{115}	2.5 d β^-	0.05 - 0.10	d,2α or d,2α2n
$_{47}Ag^{112}$	3.2 h β^-	0.1	d,2αp2n or d,2αp4n

Ag^{106}	8.2 d K, e^-,γ	1.0	d,2αp8n or d,2αp10n
$_{46}Pd^{112}$	21 h β^-	0.00006	d,2α2pn or d,2α2p3n or d,3αn
Pd^{111}	26 m β^-	0.002	d,3α or d,3α2n
Pd^{109}	13 h β^-	0.007	d,3α2n or d,3α4n
$_{45}Ru^{105}$	4.6 h β^-	0.006	d,3αp5n or d,3αp7n

Lindner said the relative yields from helium ion bombardments are about the same as those from deuteron bombardment yields.

Eyring discussed the calibrations of the microcalorimeter. Chemical calibrations were made by dissolving plutonium metal in 1.5 M HCl at 25°C; the results on four samples ranged from -149.2 to -149.6 kcal/mole, but the result by Westrum and Robinson in Chicago was -141.6 kcal/mole for dissolution in 6 M HCl at 25°C, which gives -141.8 kcal/mole at 1.5 M HCl. They made a run with 2.2 mg neptunium metal yesterday.

* * * * *

Later I stopped in and talked with Wendell Latimer about the statement the GAC has prepared to counteract the misconception that atomic power was "just around the corner." Latimer agreed wholeheartedly that such a statement was needed.

In my hill office I checked with some of the men, called Chicago, and then sent the following to Norris Bradbury in response to yesterday's wire:

> CAN'T REMEMBER ANY ANALYSIS OF THE TYPE MENTIONED IN YOUR JULY 30 MESSAGE. HAVE CHECKED WITH MEN HERE AND CHICAGO AND FIND NO SUCH RECOLLECTION. JULIAN WEBB OF EASTMAN KODAK COMPANY, ROCHESTER, NEW YORK, HAS GENERAL KNOWLEDGE OF THIS MATTER. I HAD HEARD THAT SUCH MATERIAL WAS FOUND IN NEIGHBORHOOD OF SOME RIVER LIKE THE WABASH AND HAVE HEARD RUMOR IT WAS DUE TO PURDUE CYCLOTRON.

Dan Wilkes sent me a copy of the press release he prepared on the 4n + 1 paper for my approval. Since he wants to release it on August 1st, he asked that I return it to him by the end of this week.

Leonard Katzin arrived in Berkeley on his way to Los Angeles. I spent some time talking with him this afternoon.

Friday, August 1, 1947

The manuscript for my talk at the University of Wisconsin Symposium is more or less completed, and today I gave it to Mary for typing.

Leonard Katzin accompanied me on my rounds of the labs this morning. After lunch we went to the Mira Vista County Club for a round of golf (LIK-107, GTS-107). We discussed a number of items during the game, including his future--he plans to talk with people at UCLA about a position with the Department of Zoology there. Katzin also mentioned that the University of Chicago has agreed to accept Martin Studier's work on the "Protactinium Series" as thesis material.

Saturday, August 2, 1947

I made my usual tour of the labs and talked with the men about their research and then spent some time reading.

Recently I talked with Ernest Lawrence about the statement the GAC prepared about atomic power. Lawrence's opinion was that it was unnecessary.

Sunday, August 3, 1947

Instead of helping Helen with some of the odd jobs connected with home ownership, I took off for Mira Vista and played a round of golf with three fellows I met there--Harry (91), Charlie (90), Howard (90), GTS (99). Howard and I played Harry and Charlie in a match (low ball--low total, two points per hole), which Howard and I lost, 6 and 4.

Monday, August 4, 1947

Much of my morning was spent catching up on my correspondence. I wrote to J. Robert Oppenheimer (Institute for Advanced Study, Princeton) to inform him that I have discussed the statement the GAC prepared about atomic power with Professor Latimer, who is very much in favor of it and feels strongly about the need for such a statement, and Professor Lawrence, who is far from convinced that such a statement is needed. I also told Oppenheimer about the recent visit to the Radiation Laboratory of Captain Rickover and his enthusiastic assistants from the Bureau of Ships, who are championing the Navy's need for the power pile program. I suggested that we (the GAC) must face this problem at one of our early meetings. [Rickover met with me, and I must say I have never encountered a more enthusiastic, dedicated and persistent individual. He is encountering resistance by many of the Navy brass to his conviction that nuclear power is ideally suited to Navy ship propulsion and must be developed in the face of any obstacles. I feel that a man with his conviction and force of personality will prevail.]

My usual letter of recommendation for Leonard Katzin went to Professor Stafford L. Warren (Dean of the Medical School, UCLA). I explained that Katzin is planning to talk with Professor Albert W. Bellamy about a future position with the Department of Zoology at UCLA, and I thought he might be interested in my opinion of Katzin.

I wrote to Marty Studier (Institute for Nuclear Studies, in Chicago) saying that I learned from Leonard Katzin that his work on the "Protactinium Series" has been accepted as thesis material but is being treated as a secret thesis. I said that I can see no reason why it should not be submitted for clearance. At the present time the parts dealing with solvent extraction would have to be deleted, but I hope, within a few months, to have even that rule rescinded.

I answered a July 31 letter from Miss Lillian E. Carlson, who sent me an unsolicited manuscript they had received; this was a biographical account of my career. Miss Carlson said that they are considering publishing it in The American Swedish Monthly and asked for my comments on the accuracy and for a little information on my lineage. In my reply I explained that I have marked the inaccuracies in the text and suggested that "Seaborg--Nuclear Chemist Extraordinary" might be less flashy than the one suggested by the author ("Seaborg--Nuclear Wonderman"). I added that I believe the article may use too flattering an approach. Finally, I gave Miss Carlson information about my parents and grandparents.

George Baitsell, in a letter dated July 29, gave me more details of the annual Sigma Xi lecture--it is scheduled for Tuesday evening, December 30, in the Ball Room of the Hotel Sherman in Chicago. In my reply today I gave him the requested title--"The Eight New Synthetic Elements." [I plan to include an historical account of the elements and a detailed discussion of the eight synthetic elements: 43, 61, 85, 87, 93, 94, 95, and 96.] I told Baitsell that I shall want the use of a projector for ordinary slides and a blackboard. I also agreed to the publication of the talk in the American Scientist and asked about the length of the address.

I learned that Henry Neumann's clearance has come through. I immediately sent a note to Neumann, a prospective graduate student from the University of Minnesota, informing him of this and saying that we are looking forward to seeing him next month.

Tuesday, August 5, 1947

Present at this morning's group meeting were Asprey, Ballou, Barton, Covey, Cudaback, Eyring, Garden, Ghiorso, Goeckermann, Hagemann (visitor from Argonne), Hicks, Howland, Hopkins, James, La Chapelle, Lindner, Magnusson, McCullough, Milton Moore, Morgan, Newton, Perlman, Reynolds, Seaborg, Street, Werner, and Westrum.

Lindner reported that Miller has found a 10-minute positron emitter in the chlorine fraction from his copper + 200 Mev deuteron bombardment but it may be due to an impurity; he also found the 30-minute beta emitter in the chlorine fraction. In reporting on his own work, Lindner said that the palladium positron emitter from his antimony + 200 Mev deuteron bombardment mentioned last week has a 1.8-hour half-life. The palladium parent of the 6-day Rh^{101} appears to have a half-life of about 8 hours; the palladium parent of the 1-day rhodium seems to have a half-life of the order of a number of days. Lindner is planning a bombardment of rhodium with 50 Mev deuterons in order to pin down the activities.

Magnusson reported on his work on the behavior of Np(V) in fluoride

solutions. His conclusions include: 1. Disproportionation is much faster in HF solution than in other acids, therefore the mechanism must be different. 2. There is compound formation of Np(V) and fluoride. 3. There appear to be two competing rates which determine the composition of the precipitate--rate of disproportionation and rate of compound formation. 4. Evidence seems to indicate a "double salt." 5. The depression of the Np(V) optical absorption peak in 0.1 M HF indicates complexing with the fluoride.

Goeckermann talked about some preliminary data in his experiments to determine the distribution of fission products from bismuth that was bombarded intermittently for a month with 200 Mev deuterons.

Fraction	Period	c/m on step 2
Cu^{67}?	58 hr	12,000
Fe	long	2,000
Ni^{66}?	56 hr	18,000
Ba^{133}	40 hr	2,000
Pd^{112}	21 hr	1,500
Ru	long	14,000
Tc	6 hr	100
Zn^{72}?		500

The Ni^{66} is virtually certain since the 5-minute Cu^{66} daughter has been milked several times from it; it is a new isotope. The 40-hour barium appears to have a 0.1-0.2 Mev particle, x-rays, and a 0.5 Mev gamma ray. He said that growth was noted in the zinc fraction and that the chemistry for the technetium was questionable.

Werner described the chemistry he has been performing, directed toward the isolation of a pure curium fraction (from the Hanford-irradiated Am^{241}, bombardment 51NB). He now has a yellow-orange precipitate, presumably curium hydroxide, which needs more purification because of probable contamination with iron. However, this consists of about 100 micrograms of Cm^{242} and means he is very close to isolating the element curium in the form of a pure (carrier-free) compound for the first time.

Westrum announced that fluosilicate was found to be an excellent catalyst for the dissolution of neptunium metal. Newton then described some film work. Howland talked a bit about the bismuth fraction from a Pb^{206} + 40 Mev deuteron bombardment that contained 12-hour Bi^{204}, among other things. A 54-hour lead daughter was obtained from the bismuth faction and is presumably Pb^{203}.

French (Pete) Hagemann, who is visiting from Argonne, then reported

on the isolation of actinium at Chicago. The source was one gram of radium (as $RaBr_2$) irradiated with neutrons in a Hanford pile to produce Ac^{227} by the reactions: $Ra^{226}(n,\gamma)Ra^{227}$ (β^-, short) → Ac^{227}. A purification procedure involving extraction with TTA resulted in the isolation of about 1 mg of quite pure Ac^{227} from a total of about 3 mg (as calculated from the neutron exposure and the neutron capture cross section of about 15 barns measured by Sigfred Peterson).

* * * * *

Later I took care of some correspondence: I answered a letter, dated July 17, that I received from Hornell Hart, Professor of Sociology at Duke University. Hart enclosed a questionnaire, "What Will the World Be Like, 25 Years from Now?" which he would like to be answered by scientists who have made studies related to the atomic crisis. I asked Tom Morgan for his comments; today I wrote Hart and said:

> We have found that we do not feel equal to making any estimates that appear sensible to us for as long as a few days after we have made them, and therefore I would rather not submit such. If, in spite of this, you would care to send us a copy of the results, we shall be very interested to see them.

On July 18 James O. Maloney, now Executive-Director of the University of Kansas Research Foundation, and an old friend from Met Lab days, wrote suggesting that a group of internationally recognized American scientists visit Russia to "thrash" out the problems of the control of atomic energy. It is an interesting idea, but today I wrote him the following:

> I have reluctantly come to the conclusion that there is not sufficient hope that anyone could put across a conference of this type to make it worthwhile to put as much effort into it as would be necessary. Have you approached other scientists on this matter--I wonder if, in particular, you have obtained the reaction of Oppenheimer?

I also replied to an August 1 letter from George A. W. Boehm (Radio Editor of the ACS News Service). Boehm asked me to appear as a guest on the radio broadcast of "Hi Jinx" on Tuesday, September 16, at 8:30 a.m.; the program is hosted by Jinx Falkenburg and Tex McCrary. Boehm said that, because of my position on the GAC, he explained to Alan Handley, Director of the program, that I must approve of the questions beforehand. If I agree to appear, Handley will meet with me, Jinx Falkenburg, and Tex McCrary a few days before the show to go over the questions. In my reply I agreed to his conditions and said that I plan to arrive in New York about noon on Sunday, September 14.

After lunch Pete Hagemann and I went to Mira Vista for a round of golf (FTH-100, GTS-105) and conversation.

Wednesday, August 6, 1947

I read a copy of a telegram that arrived late yesterday afternoon from James Fisk: "REQUEST FOR CLEARANCE FOR WILKINSON HAS BEEN APPROVED BY COMMISSION STOP REGULAR CLEARANCE PROCEDURE IS NOW IN PROGRESS STOP"

I then called Geoff Wilkinson to give him the good news.

I located Chuck Blanchard's address and sent it on to Cliff Garner at UCLA. [Blanchard is one of the possibilities I suggested to Garner for a electronics technician position Garner wants to fill.]

My talk called "Preparation of Radioactive Isotopes" for the Wisconsin Symposium has been typed; today I proofed it, making only a few minor changes.

The rest of the day was spent reading and talking with the men.

Thursday, August 7, 1947

In my mail today was a note from S. L. Katcoff, M.D., who wrote, "I am writing to express to you my sincere gratefulness for the instructive and inspirational course in Nuclear Physics, which I had the pleasure of taking under your auspices. You deserve congratulations and esteem for your unusual ability and beautiful manner of teaching the subject." [Katcoff, who is an old friend, is referring to my talk at the Medical Center in July.]

Our "Letter to the Editor" entitled "The (4n + 1) Radioactive Series: The Decay Products of U^{233}" by F. Hagemann, L. I. Katzin, M. H. Studier, A. Ghiorso, and G. T. Seaborg appeared in the August 1st issue of The Physical Review [Phys. Rev. 72, 252 (1947] along with the "Letter" by the Canadian workers. This is a description of the work we did at the Met Lab during 1944-46. As the result of a news release by Dan Wilkes, today's papers carried articles about the work:

San Francisco News, August 7, 1947

UC Scientists 'Find' Elements

Two Radioactive 'Links' Discovered

Two radioactive elements that existed two billion years ago were reported "re-discovered" in Berkeley today by cyclotronic bombardment.

Elements 85 and 87, the "missing link" in the periodic table of elements, have been created synthetically by cyclotron and atomic oven transmutation for the first time. The discoveries end a 76-year search for the key to one of the fundamental mysteries of science.

Glenn T. Seaborg and Albert Ghiorso of the University of California said the two "missing" elements, now called astatine and francium, existed in nature more than two billion years ago but decayed into other elements.

ACCOUNTS FOR SERIES

Mr. Seaborg said the cyclotron discovery accounted for the missing fourth series of heavy radioactive elements in the periodic table, which has been named the "neptunium" series since it has the longest period of radioactivity of any of the other elements in the series.

The other series contain elements in the uranium, thorium and actinium groupings—all found previously in nature or in the laboratory.

Mr. Seaborg said the inability of scientists to find elements 85 and 87 had been one of science's mysteries since the table of elements was established in 1871. The table lists elements in the order of their atomic "weight," determined by the number of protons in the nuclei.

'FATHER' OF ELEMENTS

He explained that neptunium was the "father" of the series and of the two elements. Like all radioactive elements, they decayed at a fixed rate which in the course of millions of years wiped them out of existence, he said.

Actually, Mr. Seaborg said, there was an even more remote ancestor of the neptunium series—synthetic element 95, or americium 241, which was discovered by University of California scientists. But it decayed so rapidly, with a "half-life" of 500 years, that it quickly became extinct soon after the earth's beginning.

Iz, Pete Hagemann, and I went to the Richmond Golf Club for a round of golf in the afternoon (FTH-98, IP-98, GTS-108).

Friday, August 8, 1947

A good part of my day was spent dictating to Mary Bender my talk entitled "Nuclear Transformations in the New High Energy Ranges." This will be my response to the receipt of the ACS Award in Pure Chemistry at the New York ACS meeting next month.

Saturday, August 9, 1947

This was a usual Saturday. I talked with some of the graduate students and other researchers about their work and then had lunch with Stan Thompson and Tom Morgan at Larry Blake's on Telegraph Avenue.

Sunday, August 10, 1947

Iz Perlman and Pete Hagemann again went with me to Mira Vista for a game of golf (FTH-105, IP-104, GTS-104).

Later in the afternoon I worked on the draft of "Nuclear Transformations in the New High Energy Ranges," the talk I plan to give before the General Assembly of the September ACS meeting. I also worked on a draft of a manuscript for the journal, Technical America, which is entitled "The New Synthetic Elements"--this is something that I promised in April to prepare.

The lab photographer recently took a nice picture of Iz for publicity purposes:

Iz Perlman, 1947

Monday, August 11, 1947

I spent the morning dictating to Mary Bender responses to some of the letters I have received--I'll sign them tomorrow.

In today's mail I received a copy of a manuscript called "The Limits of Beta Stability" from Truman Kohman. Kohman mentioned that he talked with Joe Katz, who told him that I am revising the two chapters for Vol. 14A of the Plutonium Project Record (PPR). He also said that he hopes the new isotope chart can be revised with only minor changes when it is declassified. Kohman reported that nothing came of the job possibility (Cornell) so he will stay at the University of Chicago for another year.

A thank-you note arrived from M. C. Sneed for my manuscript on the actinide elements. Sneed said that he hopes to see me at the ACS meeting in New York.

After reading a few reports, I made the rounds of the labs, as usual, to see how the research is progressing.

Tuesday, August 12, 1947

The following were present at the group meeting this morning: Asprey, Ballou, Barton, Conway, Covey, Cunningham, Eyring, Garden, Ghiorso, Goeckermann, Hicks, Howland, Huffman, James, La Chapelle, Miller, Moore, Morgan, Newton, O'Connor, Seaborg, Stewart, Street, Templeton, and Westrum. First, Howland reported a revision of his results of the bombardment of Pb^{206} with 40 Mev deuterons, saying that the isotopes formed in highest yield are Bi^{204}, Pb^{203}, and Tl^{202} and probably all are formed directly.

Then Asprey described the method being used in the attempted preparation of $SmCl_3$ and the future plans to prepare $AmCl_3$, $SmCl_2$, and $AmCl_2$.

La Chapelle discussed his plans for the preparation of compounds of Np(V); he will prepare the Np(V) solution by oxidizing Np(IV) to Np(VI) with 0.01 M Cl_2, followed by reduction to Np(V) with an approximately equivalent amount of N_2H_4 (excess N_2H_4 will be destroyed by heating). When I asked if the N_2H_4 didn't reduce the Np(VI) all the way to Np(IV), he replied that it did but the rate of reduction to (V) is much faster than the rate of reduction to (IV), which is very slow.

O'Connor talked about some activities resulting from the bombardment of uranium with 400 Mev helium ions. He found a 22-hour tungsten activity (W^{187}); a 32-hour Os^{191} and possibly a 17-day Os^{193} were also observed; and a 14-hour Au^{196} and a 3-day gold (Au^{198} and/or Au^{199})--all beta-emitting isotopes. O'Connor also discussed the yields found in the astatine and radium-barium fractions from the bombardment of uranium with 200, 150, 100, and 50 Mev deuterons; he found At^{211} in all four bombardments (loss of 29 mass units). I suggested that one should be cautious about calling the gold, tungsten, and osmium activities fission products.

Westrum described his plans to determine the energy released by

curium in decay by a micro-calorimetric method in order to obtain the specific activity and power output. I commented that he will be using the calorimeter as essentially a very low geometry alpha counter. Westrum also plans to determine the relative vapor pressures of americium and curium metal.

Street discussed very tentative results dealing with the destruction cross section of long-lived Am^{242}; the relative yield of Am^{242} in bombardment 51NB is low compared with that expected on the basis of the first Hanford bombardment (51NA) and the destruction cross section is about 8,000 barns for Am^{242}. I suggested that another interpretation might be that there was only two-thirds as much neutron flux as reported, but Street said the flux calculated from the yield of the 16-hour isomer was 20% higher than reported to us. Also, if he uses Morgan's value of 16.8 hours for the half-life instead of the 16.4 hours he used, the calculated flux is almost exactly that reported from Hanford.

Cunningham talked about the current status of the investigation of the Hanford-bombarded Am^{241} for 97 and 98 activities: they have exhausted the possibilities of finding 97, assuming that it is capable of oxidation to the tetravalent state under the conditions used. They have had difficulties with the Nalcite when they looked for elution ahead of curium in experiments in which they have assumed element 97 was not oxidizable: they now have a maximum of 200 c/m of 98 and a maximum of about 3,000 c/m of 97 remaining, with more separations necessary.

Hicks discussed possible heavy rare earth fission products of Am^{242} (from Hanford irradiation of Am^{241}) separated by elution from a Nalcite column run.

Finally, Templeton told the group about the mass spectrograph program and results at Chicago; information he learned from his visit there last week.

* * * * *

In my office I signed the letters that I dictated yesterday: I answered a lengthy July 25 letter from Charles Coryell, who sent me a copy of the paper by Marinsky and Glendenin, "The Chemical Identification of Radioisotopes of Neodymium and Element 61," which they will present at the September ACS meeting. Coryell mentioned that Dr. Coe did not want them to propose their name "prometheum" in this publication. In addition, Coryell sent me a copy of their fission product tables, asked for reprints of mine, and asked for more information about the study of exchange of iron radioactivity between ferrocyanide and ferricyanide--prewar work I did with Kennedy and Ruben and never completed. In my reply I said that I hope Marinsky and Glendenin present their names for element 61 soon. I sent Coryell the reprints he requested, but I said that I do not know how soon we can have our new "Table of Isotopes" declassified because of the papers cited and the declassification rules about heavy isotopes. Finally, I said that Roy C. Thompson repeated our early work on the ferrocyanide-ferricyanide exchange and the work is in publication someplace--I gave Thompson's address to Coryell. As a P.S., I mentioned that some time ago I asked our Information Division to send him a copy of the secret, "Table of

Isotopes."

Norris F. Hall asked on July 31 for a rough outline of the topics I intend to talk about at the 2 p.m. panel discussion on "Chemical Reactions" on September 11 at the University of Wisconsin as part of the Symposium on the "Use of Isotopes in Biology and Medicine." I replied today that I do not have any strong preferences and could be quite flexible. I suggested that I might discuss the use of the tracer technique to study the chemistry of artificially produced elements such as numbers 43, 61, 85, 87, 93, 94, 95, and 96, or I might talk about the use of tracers to study exchange reactions. I have, I wrote, no good suggestions on the conduct of the discussion other than to call on each of us at the beginning to speak in turn on some application of tracers to chemistry. Finally, I asked for a list of train departure times from both Chicago and Madison.

I wrote to Francis W. Test (Chicago Patent Group). Test sent me on August 4 patent case S-2288 for execution; in my reply I said that I believe some of the work described in this case was done by Roy Heath and suggested that he discuss the case with Dr. Harrison S. Brown, who is now at the Institute for Nuclear Studies.

In reply to an August 4 letter from W. H Sullivan, I wrote that we are looking forward to seeing him around August 21; I told him to come equipped with a V-2 pass marked "All areas including Building 4." I ok'd October 6 as the date for the Washington meeting of the Subcommittee on Nuclear Constants. Sullivan said that he hopes to have information about our requested irradiation (Am^{241} in Chalk River pile) by the time of his visit, and I suggested that a way be cleared so that we can send our sample up ahead of time.

I answered an August 1 letter from George Manov, a friend since graduate school days, who wrote that he is interested in getting into the field of atomic energy and would like to talk with me at the New York ACS meeting in September. In my reply I said that I shall be glad to talk with him then and noted that there are a number of opportunities in which he might be interested.

Warren Weaver reported to me on August 7 that U. S. Rubber has decided not to sponsor the Philharmonic-Symphony radio broadcasts for which I recently prepared a script. However, the Campbell-Ewald Company would like to proceed with the publication of the most recent scripts in the same fashion as they used to do before. Therefore, Weaver asked me to approve of the editorial changes they have made in my manuscript. In my reply today I said that I made a few minor changes and then added that Marinsky and Glendenin may suggest a name for element 61 by late September. If this manuscript does not go into print before then, I said, I can send the change needed on element 61 in order to include this item.

I met Professor Gibson and went out to Mira Vista for 18 holes of golf (GEG-103, GTS-107).

Wednesday, August 13, 1947

In today's mail I received, via transmittal by Foster York of the Chicago Patent Group and William E. Elliott of the Berkeley Patent Group, a letter from Robert A. Lavender (AEC Adviser on Patent Matters), signed by Roland A. Anderson (AEC Deputy Adviser in Patent Matters), enclosing an 11-page detailed memorandum (Memorandum Opinion No. 39), dated July 25, 1947. This analyzes the possible competing claims of Theodore T. Magel (OSRD Case No. S-2721, Richard D. Baker (No. S-3643), Glenn T. Seaborg (No. S-764), H. L. Baumbach (No. S-2243), and Paul L. Kirk (No. S-2951) for the invention of the method of producing plutonium metal by the alkali metal or alkaline earth metal reduction of a plutonium halide. Lavender's conclusion is:

> The analysis of the evidence submitted on behalf of the parties involved in this conflict as indicated above establishes conception on all of the counts for the party Seaborg as of 30 October 1942 and reduction to practice of count A at least as early as 6 November 1943 and counts B, C, and D on 14 April 1944. None of the other parties in the conflict has alleged or established any date earlier than that established by Seaborg, therefore, priority of invention of the subject matter in issue must be accorded the party Glenn T. Seaborg.

In addition, a tax-exempt certificate arrived from Edgar Gordon (University of Wisconsin) to be used for my travel to their Symposium on Isotopes in September. Gordon said that he will reimburse me for the travel and give me my honorarium during my visit.

I received, signed, and returned to Campbell-Ewald Company the contract for my radio script that I prepared for the Philharmonic Symphony programs.

George A. W. Boehm (Radio Editor, ACS) asked, in a letter that arrived today, for a time on Monday, September 15, for me to meet with Mr. Handley of the "Hi Jinx" program for a preliminary interview. I wrote back, suggesting about 1:30 p.m.

Thursday, August 14, 1947

I made my regular tour of the labs and then took care of a little correspondence. On August 8 George Baitsell wrote, acknowledging the title for my Sigma Xi talk in December and my need for a projector and a blackboard. Baitsell requested a publicity photograph, which I mailed to him today.

I mailed a copy of my manuscript, "Preparation of Radioactive Isotopes," which I plan to present at the University of Wisconsin Symposium on the Use of Isotopes in Biology and Medicine at Madison at 11 a.m. on Wednesday, September 10, to Winston Manning. Since Manning told me that he plans to attend the entire symposium, I asked him to read the manuscript if I am unable to make it at that time. [I am becoming increasingly concerned about the arrival date of our new baby.] I also mailed a copy of the manuscript to Paul Aebersold at Oak Ridge, who is also appearing on the program. I explained that I have tried to make my talk very general and told him that I have enjoyed receiving the

documents his office has been sending me.

In a note to J. C(lark) Hindman at Argonne, I wrote that I have heard he plans to vacation in California. If he plans to visit Berkeley, I said, we would like him to visit us. In case he should want to do this, I suggest that he come equipped with a V-2 marked "All areas including Building 4."

One of the girls in the office checked the references in my article on the actinide elements that I prepared for Professor Sneed's book and uncovered a few inaccuracies. Today I mailed these minor changes to him.

In the late afternoon Professor Gibson and I went to Mira Vista for nine holes of golf (GEG-54, GTS-51).

Friday, August 15, 1947

Ernest Lawrence has talked with me several times recently about plans for the visit of the AEC Commissioners and the laboratory directors next week; they will be in the area for the first meeting of the Research Council. Lawrence has arranged to hold some of the meetings in the Bohemian Grove. Today I received carbons of letters, written by Don Cooksey, to Eugene Wigner and C. Guy Suits, describing the arrangements for me to pick them up next Monday and transport them to the Grove.

A note arrived from C. W. Metz, acknowledging my letter about Leonard Katzin and saying that they do not have an opening at the University of Pennsylvania for a man of Katzin's status and training.

I looked over some of the recent reports that have arrived in our office.

Saturday, August 16, 1947

At the lab today I again worked on my talk, which is called "Nuclear Transformations in the New High Energy Ranges," for the ACS meeting next month. Since the paper contains the results of many researchers, I have discussed it with the men and incorporated their suggestions into the talk.

Sunday, August 17, 1947

I again worked on "The New Synthetic Elements" for the journal, Technical America. It is in reasonable shape, and I shall give it to Mary tomorrow for typing.

Helen is feeling well, but I did try to help with Peter.

Monday, August 18, 1947

I picked up Eugene Wigner and Frank Spedding at the Durant Hotel at 9 a.m. and drove to San Francisco and the Fairmont Hotel, where I picked up Guy Suits; the four of us then proceeded on to the Bohemian Grove on the Russian River, arriving in time for lunch. Among those in attendance at the Grove are Lilienthal, Lawrence, Cooksey, Fisk, Walter Zinn, Philip M.

Morse, Norris Bradbury, Lewis Strauss, William Waymack, Robert Bacher, Sumner Pike, Robert Oppenheimer, and James McCormack.

The informal discussions today centered primarily on the role the AEC should play in the support of basic research.

Tuesday, August 19, 1947

At Bohemian Grove. The group met to discuss a number of issues, including the time scale for the development of nuclear power for the production of electricity. The Commissioners met privately in order to debate the question of the foreign distribution of radioisotopes. Lewis Strauss is adamant in his view that radioisotopes should not be allowed to go to European countries because they can use them to further their military research. The members of the GAC and most of the other Commissioners strongly disagree with him about this.

Wednesday, August 20, 1947

In Bohemian Grove. I took part in some continuing discussions and later drove back to Berkeley.

Thursday, August 21, 1947

Back at the lab today, I checked on the status of the research and looked over my correspondence. Lillian E. Carlson, in a letter dated August 15th, thanked me for the revisions I did to Mr. Gordon's manuscript about me for The American Swedish Monthly.

A lengthy letter and report arrived for me from Herb Hyman, describing their experiments on the Np(IV) behavior in the Redox Process. I have expressed some skepticism about their results, but he believes their results are right and provided some cogent arguments to support his belief. Hyman explained that a more detailed report is being prepared; however, I marked this preliminary report for circulation to Magnusson, Cunningham, and Thompson.

I read a reply from Winston Manning to my recent request that he read my paper at the Wisconsin Symposium in September in the event that I am unable to be there. Manning agreed and then asked if I plan to use slides and if the paper has been declassified.

Another note arrived from George A. W. Boehm, giving me the arrangements for my interview for the Hi Jinx program: 1:30 p.m., September 15, corner of Press Room on the mezzanine floor of the Hotel Pennsylvania.

Friday, August 22, 1947

I placed a phone call this morning to Winston Manning to discuss not only the questions he raised about my paper for the Wisconsin meeting but also some questions about some of the papers in the PPR. Later I sent him a note to point out that a year ago Ralph James said that he believes the gamma ray of Np^{234}, the isotope analogous to Am^{238}, has an energy appreciably lower than 1.9 Mev.

Later I wrote to Joe Katz in order to describe some of the revisions necessary in order to make some of the papers in Vol. 14B of the PPR up to date and declassifiable--No. 22.8 "Products of Helium Ion and Deuteron Bombardment of U^{235} and U^{238}" by James, et al., No. 22.15 "Products of the Deuteron and Helium Ion Bombardment of U^{233}" by Hyde et al., and No. 22.16 "Bombardment of Np^{237} with Deuterons and Helium Ions" by James, et al. I pointed out that none of the papers entitled "Fission Properties of ..." for isotopes above Z = 89 will be declassifiable even under the new declassification guide. I then asked Joe to call me some time to talk further about the status of the project.

W. H Sullivan, who is visiting the Bay Area for a few days, stopped in to visit me and to discuss some of the problems he has with his isotope chart. We also talked about the Am^{241} irradiation that we want to have done in the Chalk River pile and which he, as the U.S. Representative at Chalk River, is helping us arrange.

Saturday, August 23, 1947

As usual, I spent the morning at the lab.

A note arrived from W. H Sullivan's office at Clinton, requesting two reprints of an old reprint of mine on the radioactive isotopes of germanium, which Sullivan needs for a project by the Subcommittee on Nuclear Constants. I told Mary to send him the requested copies and, in addition, to mail an extra copy of all of my reprints.

I immediately answered a letter that arrived today from Jim Crowe, who informed me that they plan to broadcast over Station WNYC the award portion of the ACS meeting. Therefore, he explained, that I should prepare my acceptance talk with the idea that a large radio audience of non-technical people will be listening. In addition, Crowe would like me to announce some sensational news! I told Crowe that I have already written the talk and am enclosing both a long version and a shortened one, but it will have to be rewritten if it is to be directed to the radio audience. I pointed out that I have not heard from Jim Stack about the radio broadcast and mentioned that we are not certain that we will use the term "splintering," which I am tentatively using in the enclosed drafts, to describe the new type of reaction we have found with the bombardments at the 184-inch cyclotron.

Sunday, August 24, 1947

I proofread "The New Synthetic Elements," which I prepared for the journal, Technical America, and then spent the rest of the day with my family. Since our new baby is due soon, I tried to help entertain Peter.

Monday, August 25, 1947

After some phone calls and other administrative problems, I took care of some of my correspondence. I answered an August 13 letter from Harry V. Welch (Chairman of the Program Committee of the Los Angeles Section of the ACS). Welch referred to my letter of June 27 to my friend Reed Brantley, in which I agreed to speak at the October 1948 meeting of their

section. Welch said that the meeting will be Friday, October 5, but they will keep the date provisional because of my commitments. Welch went on to say that one of the reasons they want to set a specific date is that they believe I may get the Nobel Prize and, if I do, it may be difficult to arrange for an address. I told Welch, in my reply, that I shall speak at the first meeting next academic year, but I shall leave the question of the title open until a few months before that meeting.

On August 15 Foster York replied to my query of August 12 about case S-2288. [I had pointed out that Roy Heath had been involved with the work described.] York has decided to make two cases out of the case he sent me: one case S-2288 in the names of Heath and Florin on the trifluoride process, and the other S-5664 in the names of Seaborg and Brown on the reaction of plutonium dioxide with hydrogen fluoride. Today I returned the original papers on S-2288 to York.

A note arrived from George Baitsell with abstract forms to be filled out for publicity for my December Sigma Xi talk; Baitsell also requested a publicity photograph.

Another letter arrived from Charles Coryell, who is presently visiting at Brookhaven. Coryell commented on the use of the word "isotope"--he believes the word has been badly distorted--and did not use it in the discussion to the Siegel tables. He said that he would have used Truman Kohman's word "nuclide" if Truman had his discussion in print. Coryell then said that he would like to hear more about my objections to the word "promethium." He cannot support the word "poseidinium" because he does not accept the detail of my analogies between the series La-Eu and that Ac-Am. In his opinion the 5f transition group first becomes normal at curium and the name "Actinide Series" is inappropriate for the series to this point. Coryell pointed out that he favors the name "thassium" (from "thanos," death) because the isotope first discovered was formed in the death of a fissile atom, it too dies in a few years, and the discovery was associated with a major effort to create instruments of death.

I made the usual rounds of the labs to look in on the research.

Tuesday, August 26, 1947

At the group meeting this morning the following people were present: Ballou, Barton, Conway, Cudaback, Eyring, Garden, Goeckermann, Hicks, Hopkins, Huffman, James, Lindner, Magnusson, Miller, Moore, Morgan, Newton, O'Connor (this is the last meeting for Morgan and O'Connor--Morgan has accepted an assistant professorship at the University of Texas and O'Connor has an assistant professorship at the University of Minnesota), Perlman, Seaborg, Stewart, Street, and Templeton.

Lindner recently reported finding, in a 200 Mev deuteron bombardment of antimony, the following activities in the palladium fraction: a 2-hour positron emitter, an 8-hour palladium with a 6-day rhodium daughter, and a several-day palladium with a 1-day rhodium daughter. In order to establish the assignments and genetic relationships, he bombarded rhodium with 40 Mev deuterons. His results were:

(1) No 2-h β^+
(2) Pd^{101} (β^+, K, 9 h) → Rh^{101} (K, 4.2 h) Ru^{101} →
(3) Pd^{100} (β^+ or K, 3.8 d) → Rh^{100} (β^+, K, 20 h) Ru^{100} →.

Perlman said they next plan to examine the ruthenium, technetium, and molybdenum regions.

Eyring reported that he has measured the heat of solution of neptunium metal at 25°C in 1.5 M HCl containing 0.001 M Na_2SiF_6 and using five pellets of about 1.5 mg each--all gave 165.9 ± 0.3 kcal/mole for Np^o → Np(IV). He pointed out that the latest neptunium metal density is 19.7. Perlman commented that the densities of protactinium and americium will be interesting in view of the high densities of the three elements between them.

James reported on his so-far unsuccessful attempts to look at Cm^{243} in the curium fraction from the pile-irradiated Am^{241} (51NB) by studying the Geiger activity and the alpha decay and ranges. In the curium fraction from the helium ion bombardment of plutonium he has seen the 27-day Cm^{240} that appears to branch--0-20% alpha and the rest K-capture. Therefore, the 50-day activity he has seen is either Cm^{241} of the Am^{240} daughter of the Cm^{240} K capture. He believes the 46-hour Geiger activity he has observed is due to either Cm^{241} or Cm^{239}.

Conway announced that he has seen about two dozen prominent lines in his spectrographic analysis of a 5 μg repurified curium sample from Werner.

Miller then reported on his study of some yields of chlorine isotopes (37-minute Cl^{38} and 60-minute Cl^{39}?) prepared in one bombardment of copper with 400 Mev helium ions and two with 200 Mev deuterons.

Templeton talked about his efforts on the structure of a compound precipitated from a solution containing Np(IV), F^-, K^+, and Cl^-. It may be $NpOF_2$, but the work is not yet completed. Templeton also reported on other studies that he is making.

Hicks mentioned further results on rare earth fission product Geiger activities eluted from a Nalcite column. A large peak observed is probably 57-day Y^{91}. Hicks is also making a column separation of an Am-Cm mixture at 78° (jacket of boiling CCl_4) at three times the normal rate.

Magnusson reported that the Np(IV) fluoride described by Templeton was the result of another attempt to prepare a Np(V) fluoride. He said that he has encountered the not unexpected polymer form in a number of solutions of Np(IV) in relatively low concentrations of HCl.

Goeckermann said that he has taken 21 elements out from a target of bismuth, which had a long helium bombardment. The known isotopes range from $_{26}Fe^{59}$ to $_{56}Ba^{133}$ and can be used to establish a yield curve. He made a few specific remarks about the various fractions.

* * * * *

Even though it is not yet declassified, Morgan gave me a bound copy of his thesis after the meeting. He also told me an amusing story that I hadn't heard. When he went to pick up his thesis after giving it to Professor Brode for approval, Professor Brode told him that it would have to be rewritten for it was written as though Morgan himself had helped discover the element. Morgan explained that he had, so Professor Brode signed the cover page and then went on to tell him that they had 1200 applicants for graduate school in physics and could accept only 50.

In today's mail I received a note from a Janet Vernon (Publicity Chairman, St. Joseph Valley Section of the ACS at Notre Dame), asking for a photograph of me to be used for publicity in connection with my Nieuwland Lectures.

A carbon copy of a letter from W. Albert Noyes, Jr. to Walter J. Murphy (Editor, C & E News) arrived for me. Noyes expressed his displeasure about the arrangements for a radio broadcast of the awards ceremony since the program was not designed to appeal to a radio audience. Noyes said that he will cooperate since the broadcast has been arranged, but he hopes that such a thing will never be arranged again. [I agree with opinions expressed by Noyes; in this case, I had already prepared my talk for a scientific audience when I learned that it was to be broadcast.]

I received the following telegram from K. H. Kingdon (General Electric, Schenectady): "WOULD BE GLAD TO HAVE YOU VISIT US OCTOBER 7 PLEASE WIRE CONFIRMATION AND HOTEL RESERVATION DESIRED."

Jim McCullough came to the house for lunch with Helen and me. He will soon leave to return to UCLA.

Soon after I returned to the lab, Helen phoned to tell me that she thought her amniotic sac had broken. Since this would mean that she is likely to give birth soon, I returned home and drove her to Alta Bates Hospital, where we met her obstetrician, Dr. Josephine Borson. I waited expectantly for a couple of hours; Dr. Borson told me that she believes it is a false alarm; however, she kept Helen overnight for observation and sent me home.

Wednesday, August 27, 1947

As Dr. Borson thought, Helen's trip to the hospital yesterday proved premature, and Dr. Borson told me to take her home. The baby is due within a week or two, and we are somewhat nervous about being able to get to the hospital on time.

Later at the lab in my role as a reviewer for declassification of documents, I wrote a memo to Don Cooksey about the declassification of BC-75, a paper by Calvin and J. C. Reid on (I.) "Some new beta diketones containing the trifluoromethyl groups" and (II.) "Keto-enol relationship in beta-diketones containing trifluoromethyl groups." I pointed out that although there may be some difference of opinion as to the suitability of this paper for declassification, I think that it should be declassified since it deliberately does not discuss the question of the formation of compounds between these compounds and metallic ions and is a paper in

pure organic chemistry. Other non-Project scientists will certainly publish similar papers soon, I pointed out.

In reply to an August 22 letter from William G. Young (UCLA), I said that I shall be glad to write a letter supporting his nomination of Saul Winstein for the ACS Award in Pure Chemistry, and I shall do it within a week or two. I then said that within the next couple of years I intend to suggest Isadore Perlman as a recipient of this award.

Thursday, August 28, 1947

I worked on some writing projects for a while today and then did some reading..

Today I mailed Beryl Gilman the manuscript I promised him in April--"The New Synthetic Elements" for publication in Technical America. I mentioned that the article is somewhat longer than he suggested but said that I am willing to shorten it. I also emphasized that I presume the article will be handled according to the customs of scientific journals and not be used for any other purpose than that for which it is being submitted.

Friday, August 29, 1947

I talked with some of the men, made a few phone calls, then did some reading.

I mailed a draft copy of my address called "Nuclear Transformations in the New High Energy Ranges" to Jim Grady of the ACS News Service, saying that it is probably a bit too long for the talk at the ACS General Meeting on September 15. I mentioned that I have just learned that the talk may be broadcast and that I am not sure whether I should change it. In addition, I asked to look over his press release in case he decides to make one. I added that we have not yet decided on the use of the term "splintering" rather than the use of an equivalent term although we should be able to decide within a few days.

In addition, I wired K. H. Kingdon (General Electric): "MY PLANS DEPEND ON UNSETTLED DATE FOR OAK RIDGE INFORMATION MEETING. DO YOU PLAN TO HAVE THE WHOLE GROUPS OF CONSULTANTS MEET?"

Helen had a doctor's appointment this afternoon; she is doing well.

Saturday, August 30, 1947

A lengthy letter arrived from Jim Crowe, who explained that after a discussion with Dr. Noyes and the News Service staff, it has been decided not to broadcast the awards ceremony at the ACS meeting. [Dr. Noyes certainly had some input.] Instead they plan a transcribed broadcast in which I am to give a fifteen-minute address. The program will be recorded in advance of Monday evening, and Crowe made a few suggestions as to the topics I should cover for this more popular-type broadcast. I replied to this letter, saying that I will prepare about fifteen minutes worth of material for radio transcription. I then informed Crowe that I shall arrived in New York late Saturday night, but I do not yet know

where I shall stay in New York.

I received an invitation from Alden Emery to attend a dinner for the medalists given by President Noyes at 6 p.m. in Parlor C of the Hotel Pennsylvania on September 15. Emery suggested wearing a light-weight suit.

A reply arrived from Kingdon (General Electric): "NOT PLANNING LARGE CONSULTANTS MEETING OCTOBER BUT THINK VISIT FROM YOU WOULD BE PROFITABLE THEN."

Sunday, August 31, 1947

Stan Thompson and I played golf today with Professor Gibson at Mira Vista Country Club (GEG-95, SGT-100, GTS-111). My scores have been getting progressively worse lately--maybe they will improve after the baby arrives.

Monday, September 1, 1947 (Labor Day)

Except for time on my up-coming talks, I spent most of the day with Helen, Peter, and Jeanette.

Tuesday, September 2, 1947

This morning's group meeting was attended by Asprey, Ballou, Barton, Conway, Eyring, Hicks, Hopkins, Howland, Huffman, La Chapelle, Magnusson, Miller, Perlman, Reynolds, Seaborg, and Street. I announced that the next meeting will be Monday, September 8 because I am going out of town on Tuesday and there will be no meeting on September 16. I also said that we are considering changing the meeting to Thursday; most of those present agreed with the change.

La Chapelle talked about his efforts to prepare Np(V) compounds. He finds that he can selectively oxidize Np(IV) with Cl_2 at room temperature. Using 0.0002 M Np, 0.025 M HCl, and 0.001 M Cl_2, he found that, after one week, the neptunium was 98% Np(V), 2% Np(IV), and no Np(VI). Upon dehydration yellow crystals were formed. He has not yet obtained an x-ray analysis of the crystals, but he does not anticipate knowledge of the structure of the compound entirely from the x-ray structure because no MO_2Cl compounds are known for comparison--he expects to find NpO_2Cl compounds are known for comparison--he expects to find $NpO_2Cl \cdot H_2O$.

Ballou reported that in his continuing work on O'Connor's problem on the products of high energy particle bombardments of uranium (200 Mev deuterons), he has isolated rhenium and osmium fractions. He has no results yet.

Hopkins talked about some unreported results of older bombardments of As^{75} with 200 Mev deuterons: milkings of the germanium fraction for gallium gave the 68-minute Ga^{68} (β^+) whose parent is a 17 ± 2 day Ge^{68}. He has found a 40-hour activity and an 11-day activity by milking the arsenic fraction for germanium. Both are assigned to Ge^{71}; the half-life for the parent of the 40-hour activity is about 50 minutes and the 11-day activity may have a parent of at least 50 hours. He found a 50 ± 1 minute arsenic daughter in the selenium fraction (As^{71}). The parent (Se^{71}) has a half-life of 44 ± 3 minutes; parent and daughter are positron emitters. In a longer 200 Mev deuteron bombardment of arsenic, he has found a 62-hour copper activity that decays by emission of 0.55 Mev negative particles; Perlman said that Goeckermann and O'Connor have found this as a fission product and assigned it to Cu^{67}. Hopkins also found a 58 ± 1 hour nickel activity, parent of the 5-minute Cu^{66}.

Asprey announced that he believes that they have prepared a small amount of red $SmCl_2$ by heating $SmCl_3$ with H_2 at 600°C.

Hicks reported on some work with the Geiger activity eluted from a Nalcite column run of the second Hanford neutron-irradiated Am^{241} (51NB). He believes the activity, which elutes under the curium peak, is the 15.4-hour Eu^{157} and the 15.4-day Eu^{156}; he plans to identify the activities chemically.

Eyring told about his attempts to prepare $NpCl_3$ by starting with the reaction of NpO_2 with H_2 at 700°C, his plans for the determination of the absolute vapor pressure of americium metal and the relative vapor pressures of americium and curium metals, and his attempt to determine whether neptunium metal undergoes phase changes on heating.

Howland talked about his plans to bombard Pb^{206} with 55 Mev deuterons tomorrow in his study of the alpha emitters of astatine polonium, and bismuth. Conway mentioned that he now has a total of 45 lines due to curium on a spectrographic plate.

* * * * *

Back in my office I wrote a lengthy letter of recommendation for Burris Cunningham to Professor Latimer (for promotional purposes--Burris is presently an assistant professor). After listing Burris' abilities and achievements, I concluded with "I believe that the University of California is very fortunate to have a man of his ability and that every effort should be made to give him the recognition for his achievements which is due him."

In today's mail I received a copy of "An Appeal to Reason" from David McCoy who asked me to read it, saying that it explores the case for the People's World Constitution Convention.

Professor Norris Hall (University of Wisconsin) sent me a list of travel connections between Chicago and Madison and then said that, since he has not heard from Coryell, the final organization of the discussion will have to wait until my arrival in Madison. Hall said he believes the application of tracer techniques to the artificially produced elements is perhaps the most interesting of my suggestions for a topic for the panel discussion.

Wednesday, September 3, 1947

At the office today I mailed a corrected copy of Chapter 2 ("Isotopes of Plutonium and their Radioactive Properties") of Volume 14A of the Plutonium Project Record (PPR) to Joe Katz, suggesting that if anyone wants to look at it, they should do it without removing it from his office. I also gave Joe advice about some of the referencing problems and suggested we title Volume 14, "The Transuranium Elements."

I wrote to E. R. Weidlein to inform him that I shall not be able to attend a meeting of the ACS Temporary Committee on Publications on Friday, September 12, since I shall be in Madison. I mentioned that I believe some of the dissatisfaction with the publication policies of the ACS journals include slow publication of "Letters to the Editor" in journals such as Journal of the American Chemical Society and the fact that the present rules for articles are too sterotyped for articles in the nuclear chemistry field. I explained that I hope we shall be able to talk about this during the ACS meeting.

Another note went to H. B. Hass, in which I said that I shall be able to attend the meeting of the Committee on Foreign Compendia at the Hotel

Pennsylvania, on Monday, September 15 at 11 a.m., although I do have another appointment at 1:30 p.m.

Melvin Calvin sent me a note, which I received today, describing some of the achievements of Saul Winstein. This is to be used in connection with the nomination of Saul for the ACS award in Pure Chemistry.

From Walter M. DeCew, Technical Editor of Nucleonics, I received a folder describing this new journal, which is being supported by McGraw-Hill. The journal appears to be a promising publication.

Thursday, September 4, 1947

At the office today I received a telegram from Jim Grady, asking that I prepare a 15-minute talk about my longer address, "Nuclear Transformations in the High Energy Ranges." This will be prerecorded on Monday, September 15, at 2:15 p.m. for broadcast on radio station WNYC during the time of the General Assembly.

I made my usual round of the labs to check on the status of the research.

Friday, September 5, 1947

At the lab today I took care of some of my correspondence. I answered a letter from John Willard, who said they have planned a smoker for the evening of September 10, that Daniels and Urey will discuss domestic and international implications of atomic energy on the evening of September 11, and that he and Adelaide would like me to come out to their home on Friday evening, September 12. Willard remarked that he hoped "Helen's event will be timed for optimum convenience in connection with your trip." [Actually, I am beginning to be quite concerned that our new baby will decide to arrive at an inopportune time.] I replied that I plan to leave Madison Friday afternoon in order to see some of the Argonne people on Friday night and Saturday morning, and then fly to New York on Saturday afternoon; therefore I must reluctantly decline their Friday night invitation.

In response to an August 29 letter from Foster York about case S-5625 ("Preparation of Neptunium Hexafluoride"), I suggested that he check to see if he has the correct inventors (Harrison Brown and Glenn Seaborg) listed. I explained that I believe I recall that Alan E. Florin was the first to make neptunium hexafluoride.

I mailed the corrected copy of Chapter 14 (on the isotopes of neptunium) of Volume 14A to Joe Katz, explaining that the references, with the exception of the paging of references N1 and P3, have been checked; I believe it is ready to issue as a secret report. I also commented on Paper 22.8 of Volume 14B--"Products of Helium-Ion and Deuteron Bombardment of U^{235} and U^{238}."

Helen had a doctor's appointment late this afternoon and reported that the doctor told her that the baby could be born at any time. I'm keeping my fingers crossed for I would hate to have to cancel my trip to Wisconsin and New York.

Saturday, September 6, 1947

When Helen awoke this morning, she felt somewhat nauseated. I left for my Rad Lab office feeling rather concerned--this is the day, according to Helen's estimate, that our baby is due and is the day after the due date, as estimated by her obstetrician, Dr. Lois Wilson. Consequently, I went for an early lunch time in order to make certain things were o.k.

I then returned to the lab to check on the research and also write to K. H. Kingdon to tell him that I shall visit Schenectady on October 7 since I expect that the October Information Meeting at Oak Ridge will be held the following week.

When I arrived home in the late afternoon, Helen told me that she felt that the birth was imminent. Using our neighbor's phone (we have not yet been able to get our own), she called Dr. Wilson at Alta Bates Hospital, who suggested that she come in to the hospital.

We arrived at the hospital about 6 p.m., and Dr. Wilson had Helen placed in a labor room. Dr. Wilson then decided to break the amniotic sac since dilation had already started. While I stayed by nervously, Helen immediately went into labor and was wheeled into the delivery room. Our 7 pound 11 ounce daughter was born at 8:09 p.m. When the baby was shown to me, I was struck by her head of jet black, scraggly hair and her very expressive face. The baby interrupted my inspection with vigorous crying, but she looks very healthy and normal. After talking with Helen, who came through the birth well, I drove back home and gave Jeanette, who had been baby sitting with Pete, the news. I also told Pete about his sister, but he did not appear particularly interested. Helen and I decided beforehand to name a girl Lynne Annette--with the first name spelled with an "e" to make the sex clear and the second name in honor of my sister Jeanette. Today is one just one day short of Lynne's grandfather's (my father's) birthday.

Sunday, September 7, 1947

I looked over my talk for the Wisconsin Symposium, entertained Peter a bit, and visited Helen and the baby at the hospital. I am very pleased that Lynne decided to arrive at this time and not interfere with my upcoming trip.

Monday, September 8, 1947

Present at today's 9 a.m. group meeting were Asprey, Ballou, Barton, Bradley, Covey, Cudaback, Cunningham, Eyring, Ghiorso, Hicks, Hopkins, Huffman, James, La Chapelle, Lindner, Magnusson, Miller, Newton, Perlman, Reynolds, Seaborg, Stewart, Street, Templeton, Thompson, Voyer, Werner, and Westrum. The first item was my announcement of baby Lynne's arrival on Saturday--I was appropriately congratulated (cheered).

Werner reported that his curium has been purified by the removal of lead (spectrographic analysis shows only 1% lead). A pure curium nitrate solution was evaporated to dryness, then the residue was heated to 700°C (in a platinum capsule) to convert it to Cm_2O_3. He went on to say he

is having difficulties in his attempts to weigh the sample--the sample gains weight over a period of several days although other factors remain constant.

Magnusson gave a lengthy report on the hydrolysis of Np(V) and compared his work with that of Kraus and Nelson at Clinton. He said that if a Np(V) ion of the type $Np(OH)_3^{++}$ is the stable species in relatively high acid rather than the type $Np(OH)_4^{+}$, one may draw the following conclusions: (1) Np(V) should be more stable than previously expected in increasing acid for any reaction in which there is hydrogen ion dependence. (2) Np(VI) ions in strong acid solution should also show less hydrolysis than is denoted by the formula XO_2^{++}, since there is evidence showing lack of hydrogen ion dependence for the V-VI couple in strong acid solutions. (3) There are now more possibilities for straightening out a number of equilibria and reaction rates which have been apparently inconsistent. The hydrolysis of the V and VI states of uranium and plutonium should be studied more extensively. (4) If ions of the type $X(OH)_3A^{++}$ exist, it may be possible to account for color changes in the +6 state, e.g., in $HClO_4$ Np(VI) is pink, but in HCl Np(VI) is green. Pu(VI) shows a wide variety of colors depending on the solvent composition.

At this point I announced the situation is rather deplorable about graduate students, in particular, keeping up with the literature. I added that, with the fall semester about to start, we want to see everyone's study programs.

Ghiorso talked about a theory being tested that elements above uranium should undergo spontaneous fission at an increasing rate--if this is so, it may be used to look for new heavy isotopes and elements. He then talked about recent spontaneous fission measurements on Hanford (51NA and 51NB) and Clinton samples.

Ballou talked a bit about a bombardment of uranium metal with 200 Mev deuterons in which he is looking for some rhenium isotopes. He also intends to look at the tantalum and europium fractions.

There was considerable discussion when I mentioned that Sullivan has suggested the word, "spalling," (from "spall" to chip) for the reactions we have been seeing.

Hicks talked about this column work--25 cm column, Nalcite, at temperature of boiling CCl_4, pH 3.19 to separate americium and curium. The results look promising for fast separations.

Perlman made a few comments about Miller's yield data on copper bombarded with 200 Mev deuterons and 400 Mev helium ions, suggesting that the data indicate that the individual nucleons give rise to some of the products rather than the deuteron or helium ion as a whole.

* * * * *

Back in my office I found a letter from Joseph Kaplan asking me to give either a 30-minute invited paper at the January 2 and 3, 1948 meeting of the American Physical Society to be held at UCLA or an one-hour address at a joint meeting of the American Physical Society and the Acoustical Society of America on December 12 in Los Angeles.

A memorandum arrived from Don Cooksey announcing that Spof English, now in the AEC Division of Research, has arranged some special seminars on solvent extraction, to be chaired by Frank Spedding and to be held on October 16 and 17 at Clinton Laboratories (after the Information Meetings). English suggested I attend. I discussed this with Perlman, who agreed.

Another note went to Joe Katz today to suggest a referencing change in Chapter 2 of Volume 14A, which I returned to him on September 3. I told Katz that paper 19.1 of Volume 14B ("The Tracer Chemistry of Americium and Curium in Aqueous Solutions" by S. G. Thompson, L. O. Morgan, R. A. James, and I. Perlman) is being revised.

For a number of months I have been trying to buy a new car. This has been extremely difficult, not only because of the shortage, but also because most dealers want to make a little extra money on the deal, something to which I am adamantly opposed. However, I finally found one dealer who put me on the waiting list and agreed to sell me a new car at list price. Today I took delivery of a 1947 Hudson sedan from H. W. McKevitt Company, Inc. in Berkeley.

Tuesday, September 9, 1947

I left San Francisco on a 9:30 a.m. flight (United Flight 602) for Chicago, where I arrived about 6:30 p.m. There I took a limousine and cab to the Chicago and Northwestern Railway Station and caught a late train to Madison. I am staying at Elizabeth Waters Hall (a student dormitory) on the University of Wisconsin campus.

[In Berkeley, Mary Bender wired a reply, over my name, to Jim Grady's September 4 telegraphic invitation: "WILL PREPARE RADIO SCRIPT AND AM AVAILABLE FOR RECORDING AT TIME INDICATED."

Iz Perlman answered the memo from Don Cooksey that I received yesterday, saying that I shall attend the solvent extraction seminars in Clinton on October 16 and 17, but that I will not present a paper.

A telegram arrived from L. S. Deatly of the Kansas City ACS Section, requesting that I stop in Kansas City on my return from the New York meeting and speak before their section during the week of September 22. Mary immediately wrote back, explaining that I have to return to Berkeley on the 18 and that she doubts that I can do this, but that she will transmit his invitation as soon as she can.

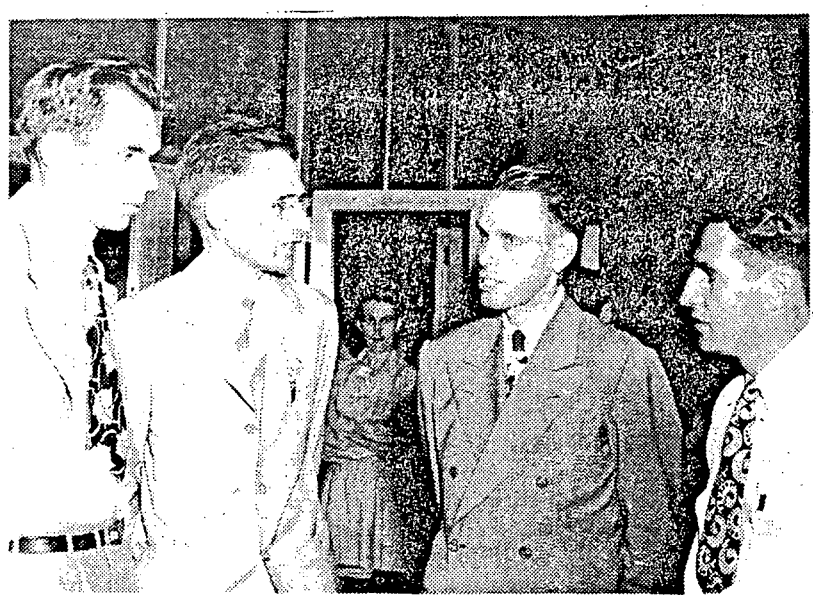
Mary also replied to Joe Kaplan's invitation and said that she doubts that I can speak on either date because of other meetings, but that I will write him when I return to Berkeley.]

Wednesday, September 10, 1947

In Madison, Wisconsin. I attended the Symposium on "The Use of Isotopes in Biology and Medicine." Following Harold C. Urey's talk on "Preparation of Stable Isotopes," I spoke at 11 a.m. on "Preparation of Radioactive Isotopes." I began by saying:

The use of radioactive isotopes as tools in research has found widespread application and the present indications point toward an ever increasing use in the many fields of science and industry. In this connection the subject of the preparation of radioactive isotopes is, of course, an important one. The large number of radioactive isotopes now known and the new methods for their production are rapidly leading to such specialized techniques in their preparation that this may soon be regarded as a field in itself extending, in some cases, into operations of such character that they may be considered industrial in nature. This is a far cry from the earliest modest researches which utilized radioactive isotopes, and it seems interesting therefore in a discussion of the preparation of radioactive isotopes, to devote a few words at the beginning to the pioneer work which eventually led to the present wide area of endeavor....

Speakers in the afternoon program, over which Farrington Daniels presided, were Paul C. Aebersold, Alfred O. Nier, Charles D. Coryell, and Martin Kamen. There was a smoker and social hour at 7 p.m.



Glenn Seaborg, John Willard, Conrad A. Elvehjem, and Robert H. Burris, University of Wisconsin, September 10, 1947

[In Berkeley, Stan Thompson drove Helen and Lynne home from Alta Bates Hospital. When they arrived home, he insisted on carrying Helen upstairs to our second story, front bedroom. Helen has hired a baby nurse, Miss Emma Stelzer, for a couple of weeks to help take care of Lynne.]

Thursday, September 11, 1947

In Madison, Wisconsin. Again the Symposium took up the whole day.

The afternoon session consisted of panel discussions, and I took part in the third one on "Chemical Reactions." Norris Hall presided, and the other participants were John Willard, Charles D. Coryell, and Farrington Daniels. In the evening Daniels and Urey spoke in a discussion session on international and domestic implications of atomic energy.

I saw Donald McPherson of John Wiley & Sons, Inc., and we had a brief conversation about the publication of the book of tables for nuclear science, which his company would like to do. I suggested that he contact Dr. L. F. Curtiss of the Bureau of Standards.

[In Berkeley, Horace Hopkins prepared a list of the nuclei formed in the 400 Mev helium ion bombardment of arsenic for me.]

Friday, September 12, 1947

In Madison, Wisconsin. After attending the morning session of the Symposium, I caught a train for Chicago. When I arrived, I took a taxi from the Chicago and Northwestern Railroad Station to the Hotel Windemere and checked in.

Saturday, September 13, 1947

In Chicago. I took a taxi to the University of Chicago campus to talk with the fellows at New Chem. Joe Katz and I went over some of the problems connected with the PPR volumes. We also talked with Clark Hindman about the feasibility of a symposium at the spring meeting of the ACS on the chemistry of the transuranium elements.

Later I went back to my hotel, picked up my luggage and took a taxi to the Chicago airport where, at 3:30 p.m. I caught United Flight 526 for New York City. I arrived at about 9 p.m. When I checked in at the Hotel Biltmore, where I will share a room with Iz Perlman, I found a letter from E. R. Weidlein, who is sorry that I cannot attend his committee (on publications) meeting on September 12 but hopes to see me while I am in New York. Weidlein is staying at the Chemists' Club and will leave New York on Monday evening, September 15.

Sunday, September 14, 1947

In New York. I went by taxi from the Biltmore Hotel to the Hotel Pennsylvania and registered for the ACS Convention. There I saw and talked with many acquaintances.

Among the people I saw was Gerhart Friedlander, a former graduate student of mine. Friedlander mentioned that he is trying to find a new job, preferably academic--he is now working for General Electric in Schenectady. I also talked for a while with George Manov who is considering changing fields to something in the nuclear area.

James M. Church (National Secretary of Phi Lambda Upsilon) spoke to me about attending the Phi Lambda Upsilon luncheon on Thursday so that I can be initiated into honorary membership. When I explained that I have to return to Berkeley before then, he suggested that this initiation take place in Los Angeles by my Alpha Kappa Chapter.

I also spoke with Saul Winstein about Bill Young's plan to nominate him for next year's ACS Award in Pure Chemistry. Saul promised to send me a list of his publications.

Monday, September 15, 1947

In New York. I took a taxi from the Hotel Biltmore to the Hotel Pennsylvania to attend the meeting of the ACS Committee on Foreign Compendia at 11 a.m. Present were R. Adams, V. du Vigneaud, H. Hass, C. Marvel, A. V. Grosse, and I. Roger Adams gave a brief resumé of his trip to Japan. The Committee then unanimously voted to recommend to the International Union's Commission on Chemical Compendia to request funds to buy and send books, journals, etc., to the Beilstein and Gmelin Editorial Offices. C. S. Marvel reported on and distributed copies of the second Report of the International Union of Chemistry's Commission on Chemical Compendia. The Committee decided to invite a member of the ACS's Committee on International Relations to become a member of our Committee to replace Dr. Whitmore who has died. H. Hass reported a highly gratifying response to his appeal for chemical literature for the the Beilstein and Gmelin Editorial Offices and other European Scientific Libraries.

At 1:30 p.m. I had a preliminary interview with Alan Handley, Jinx Falkenburg [she is most attractive], and Tex McCrary in the Press Room on the mezzanine floor of the Hotel Pennsylvania, preparatory to my appearance on the "Hi Jinx" program on Tuesday morning. George A. W. Boehm, Radio Editor of the ACS arranged this.

Then at 2:15 p.m. I recorded at WOR studios (Broadway and 40th Streets) a 15-minute talk, "An Account of Nuclear Research Developments." This is for broadcast tonight over radio station WNYC during the awards ceremony. I began by saying:

> I am going to speak to you this evening about atomic or nuclear research including a number of aspects of this important subject which is in the news so much these days. I suppose that when the subject of atomic research is mentioned it brings to mind for most people the atomic bomb, the most spectacular product of research work in this field. However, I am not going to speak about this tonight, but rather I shall confine my remarks to the many constructive results which are being achieved in this field. In particular I shall speak about three of these aspects--first, the prospects for the application of atomic energy to peacetime industrial uses, second, the use of the by-product radioactive material for important research in medicine and biology and other scientific fields, and third, I shall say a few words about the interesting new ultra high energy machines which are being built in this country...

I returned to the Biltmore Hotel to rest and freshen up and then attended a 6 p.m. dinner for the medalists given by President W. Albert Noyes, Jr., in Parlor C of the Hotel Pennsylvania. This was followed by the ACS General Meeting at Manhattan Center (34th St. just west of 8th Ave.). There Professor Warren K. Lewis of MIT was awarded the Priestley Medal, I was given the ACS Award in Pure Chemistry (presented by Foster D. Snell representing Alpha Chi Sigma), and Mary Lura Sherrill of Mount

Holyoke College was awarded the Francis P. Garvin Medal for Women in Chemistry. My acceptance address entitled "Nuclear Tranformations in the New High Energy Ranges" began:

> Nuclear science is about to enter, and in fact has to some extent already entered, an important new phase in its development, namely the area of ultra high energies. A limited entree into this region had already been permitted through observations with high energy cosmic rays, but these are of such low intensity as to limit seriously both the rate and the scope of progress which is possible. New ideas for design, together with new techniques for building, have led to prospects for the construction in the rather immediate future of a number of machines capable of accelerating electrons and light positive ions to an energy region comparable with that of the most interesting of the cosmic rays.

G. T. Seaborg receives Award in Pure Chemistry from F. D. Snell, representing Alpha Chi Sigma

After the meeting there was a general mixer, and I again saw and spoke with many old friends. I talked briefly with Fred Albaugh, who is attending the meeting in order to recruit new employees for the General Electric Nucleonics Project at Hanford. I told him about my recent purchase of a Hudson.

Professor Sneed also came up to me and, after congratulating me, told me how pleased he is with my contribution to his book. I also had a few words with Eric A. Löf of American Cyanamid. When we digressed into the subject of our Swedish backgrounds, I mentioned that my parents want to visit Sweden next year but are having difficulties securing passage on the Swedish-American Line; Löf offered to contact a friend with the company.

After the mixer I returned to my hotel. It has been a full day.

Tuesday, September 16, 1947

In New York. At 8:30 a.m. I took part in the "Hi Jinx" program (Station WNBC) with Jinx Falkenburg and Tex McCrary. I returned to the Biltmore, called Berkeley, and talked with Mary Bender, who told me about the invitation to speak to the Kansas City ACS Section on my way home and her letter declining the invitation. I told Mary that she was absolutely correct.

I then took a taxi to the Hotel Pennsylvania. Here I met with Hoylande Young to discuss declassification of "Chart of Nuclear Species" by Kohman, Seaborg, and Sullivan. She will mail me some copies and I shall mark one to make it declassifiable and return it to her. We also talked about the much bigger job--declassification of Volumes 14A and 14B of the Plutonium Project Record, and I insisted that she arrange for additional Responsible Reviewers to help handle the papers. I suggested the names of Edgar Westrum and Edwin Orlemann as men highly qualified for the job.

After a busy day, I returned to my hotel to rest and freshen up; then I caught a cab to the Hotel New Yorker, where I was a guest of the Professional Branch of Alpha Chi Sigma at their dinner in the Ballroom.

Wednesday, September 17, 1947

In New York. Most of my day was spent at the Symposium on Ion-Exhange Separations in the Ballroom of the Hotel Pennsylvania at which I heard papers by E. R. Tompkins and his group at Oak Ridge, F. H. Spedding's group at Iowa State, and B. H. Ketelle. J. A. Marinsky and L. E. Glendenin presented their paper called "The Chemical Identification of the Isotopes of Neodymium and Element 61." They suggested their name of "prometheum" for element 61; until the question of the discoverer is settled, the element will have to remain unnamed. It was a most satisfying meeting.

[In Berkeley, Helen and I received the following warm letter from my parents about the birth of Lynne--the first part is from my father (Ted) while the second part is from my mother (Selma):

South Gate, Calif.
Sept 14' 47

Dear Children:-

Of course it was with considerable delight & satisfaction that we heard Roe again have become Grandpa & Grandma. However I think that girl Helen played a trick on me which I'll find it hard to forgive her, as she could have waited a measly four hours, thereby giving me the honor of becoming Grandpa on my Birthday. Well it was pretty close anyway so we can practicly celebrate the event on the same day. I want to thank you heartily for the "Money" you sent me on my Birthday. Plus together with what I received from a bunch of people who came to our home last Sunday was enough to pay for one of two suit-cases which we have bought.

Well, we at last have suit-cases now, whether we ever use them or not. Well it was nice for Peter too, to have a little sister to play with, or probably I should say to tease a little later on. It certainly would be nice to see them and we do hope that things will be in such a shape that you can come down here and spend the Xmas holidays with us. I want you to thank Jeanette for the nice shirt she sent me.

With Love & Best Regards to all of you. "Grandpa & Grandma."

Dear Children.
I want to write a line too, and let you know how glad we are to have another Grandchild and hope that you are all well and happy. I am mailing a gift for the new baby - to play with with lots of love. Grandma Grandpa

Thursday, September 18, 1947

In New York. Part of my morning was spent at the ACS meeting at the Hotel Pennsylvania, talking with various friends. One person with whom I spoke was Alan Clifford, former Met Lab colleague who is now a graduate

student at the University of Delaware. Clifford told me about his work on the preparation of $SmCl_2$ by the reduction of an anhydrous solution of $SmCl_3$ with magnesium plus HCl. Clifford promised to send me a copy of his master's thesis. I renewed my invitation to him, first extended last spring, to come out to Berkeley to work for his Ph.D. degree. [I know from his performance at the Met Lab that he is an unusually able scientist.]

In the late morning I took the limousine to the airport and caught American Air Line Flight 9 to San Francisco. After my 10 p.m. arrival in San Francisco, I took a limousine downtown, where Herman Robinson met me and drove me home.

Helen reported that she, Peter, and little Lynne are doing well--I took a brief peek at Lynne.

Friday, September 19, 1947

After talking with Pete and admiring Lynne, I went to the lab and checked on the research of the various men. I told Asprey about Clifford's method of preparing $SmCl_2$ by reducing an anhydrous solution of $SmCl_3$ in absolute alcohol with magnesium plus HCl.

Later I looked at and handled some of my accumulated correspondence. I read a September 5 letter from H. W. Pearsall (Assistant Editor, Record of Chemical Progress), who asked that a sharper copy of Figures 4 and 5 of my Wayne University talk be sent to the printers. Mary took care of this while I was out of town.

David McCoy of World Republic wrote on September 10 to inform me of the visit to this country of Henry Usborne, British M.P., who will speak in various cities next month on the subject of world federation.

I looked over a preliminary draft of the program for the Solvent Extraction Conference on October 16 and 17 at Oak Ridge that arrived from Frank Spedding. Spedding included a list of participants.

C. E. Center (Plant Superintendent) sent me an invitation to visit the K-25 plant during my visit at Oak Ridge during the week of October 12.

I dropped a note to F. T. Hobbs (AEC, Washington) to explain that I cannot supply the information Curtis A. Nelson (U. S. Administrative Representative, Chalk River) requested about actinium because the copy of the NRC's requisition was not attached to my copy.

Saturday, September 20, 1947

I made the usual rounds of the labs during the morning and, in the afternoon, went to the first football game of the season. California has a new coach--Lynn O. "Pappy" Waldorf--and this year's team looks promising. It beat Santa Clara 33 to 7. Waldorf comes to us from Northwestern University, where he developed such outstanding players as quarterback Otto Graham and established a reputation as a fine leader of men and molder of character, the type of coach we want at Berkeley.

Sunday, September 21, 1947

I spent the day at home with the family, where I did a lot of reading. I enjoyed getting acquainted with our daughter Lynne and watching Pete's reaction to his sister. I also took an occasional turn at giving Lynne her bottle and walking her when she fussed. Our nurse, Miss Emma Stelzer, has worked out well, and she is a good cook. She also arranged for a substitute, Florence Henderson, when she had to be absent for a couple of days.

Monday, September 22, 1947

The first thing I did this morning was to take care of a few administrative matters. I answered a September 8 letter from Ira M. Freeman (Rutgers University), who is preparing an elementary physics textbook for McGraw-Hill. Freeman asked for the proper way to list the transuranium elements in a periodic table. I told Freeman that this is a question that has been the subject of a great deal of discussion, but I believe it is best handled by listing the elements from actinium on as a second rare earth group outside the body of the periodic table as the Welch Company has done in its 1947 wall chart. I gave Freeman the mass number of the longest-lived isotope of each element to be used in place of the atomic weight of the element.

As the result of my appearance on the Tex and Jinx program, I received a letter from a Ruth Van Voast of Brooklyn, New York, who asked that I give her an honest answer as to how much torture of house pets, as opposed to mice and guinea pigs, goes on in our university laboratories.

While I was in Madison, Edgar S. Gordon (State of Wisconsin General Hospital) asked for a picture of me. This I had mailed to him, saying in the covering letter that I enjoyed the Symposium and have heard many favorable reports about it.

I wrote to K. H. Kingdon (General Electric, Schenectady) to confirm the date of October 7th for my visit to Schenectady and to ask for a hotel reservation for the night of October 6.

Tuesday, September 23, 1947

Present at this morning's 8 a.m. meeting were Asprey, Ballou, Conway, Cunningham, Eyring, Ghiorso, Goeckermann, Hicks, Hopkins, Howland, James, La Chapelle, Lindner, Miller, Newton, Moore, Perlman, Redman, Seaborg, Street, Templeton, Thompson, and Vermeulen. First to report was James, who discussed his work with Street on a sample of plutonium that had a 40 Mev helium ion bombardment. The curium fraction showed an alpha decay period of about 30 days, while its Geiger activity is showing a 15-day period. The americium fraction shows a 50-hour period (Am^{238}), which may be formed from deuteron impurity in the beam or may be a daughter of Cm^{238} decaying by K capture.

Newton talked about the fission yields of isotopes formed in a 40 Mev helium ion bombardment of thorium. Newton isolated 130-day $Sn^{121,123}$, 43-day Cd^{115}, 55-day Sr^{89}, 12.8-day Ba^{140}, 275-day Ce^{144}, 1-year Ru^{106}, and 70-year U^{232} and determined the relative yields as a function

of alpha energy, using successive layers milled from the target.

La Chapelle discussed his preparation of what is probably $NpO_2Cl \cdot xH_2O$ (a green crystalline substance) and his plans to check the Cl/Np ratio and to prepare a sample for x-ray analysis. He also plans to prepare the pentachloride. I suggested that we should try to prepare Np_3O_8 as they have done in Chicago.

Goeckermann showed his curve of the fission yields for various isotopes formed by 200 Mev deuterons on bismuth. Perlman said that the accumulating data indicate that the heavy fragments are neutron deficient while the light fragments have a neutron excess like normal uranium fission products. A possible explanation for these results is that the nucleus splits with the same n/p ratio in each fragment.

Asprey told about his preparation of $SmCl_2$ by preparing anhydrous $SmCl_3$ by heating the hydrated salt in HCl at about 300°C, then reducing it to $SmCl_2$ by heating in H_2 for two days. So far, he has not been successful in preparing it by a method described by Clifford.

Ghiorso reported on spontaneous fission measurements on the two curium samples from Hanford-irradiated americium and the Clinton sample. He has no evidence for spontaneous fission of Cm^{243} and concludes the spontaneous fission is due to Cm^{242}.

Templeton reported cell constants for a sample of $NpCl_3$ (prepared by Eyring) and a sample of AmF_3 (prepared by Eyring). He also announced that his x-ray and camera system for making diffraction measurements at high temperatures (up to 500-600°C) has been set up and is now operable.

* * * * *

At 10 a.m. I met with the Chemistry 123 class, which is being held in Room 219, Gilman Hall. Students this semester include John C. Alley, Carlos B. Amspoker, Gordon M. Barrow, George W. Barton, Michel J. Berger, Nathaniel I. Berlin, Herman J. Bradley, Richard G. Breuer, Fred N. Briggs, Edward P. Bullwinkel, Meyer Chessin, Leland J. Chinn, James B. Chubbuck, Donald R. Cochran, James A. Cook, Arthur M. Crestfield, Dean W. Criddle, Clarence Cunningham, Milton W. Davis, Thomas E. Davis, James A. Dyer, Emanuel Epstein, William H. Eustis, Lyman L. Forker, Abraham J. Fradkin, George J. Fritz, Marjorie H. Goodwin, John Gruber, Martin E. Haet, George F. Hardy, Herman R. Haymond, Cline H. Hayward, Warren J. Heiman, Harold L. Helwig, Thomas G. Hennessy, Earl H. Hoerger, Donald F. Howard, Julian Huebscher, Rex L. Huff, Leland F. Hunt, Carl R. Hurley, Edwin L. Iloff, Alice J. Irish, Yoshiko Ito, Gordon L. Johnson, Betsy Mae Jones, Lieselotte J. Kamm, Arthur Karler, David G. Karraker, Robert W. Kaufmann, Jack D. Knight, Robert D. Knopes, J. Eugene Kunzler, Richard M. Lemmon, A. Albert Lewis, Harold R. Lohr, Aaron Lukton, Luis Marquez, Kenneth L. Mattern, Alice L. Mazman, William W. Meinke, Carl F. Miller, Lewis W. Myers, Robert A. Naumann, Fred M. Nelson, Henry M. Neumann, Abraham Ottenberg, John S. Peck, Robert L. Penn, Valerie C. Phillips, Joseph F. Pinnick, Ernest A. Pinson, William W. Reynolds, Esther D. Richert, Guy Rocco, Guido B. Rossi, James Robertson, David A. Salzberg, Gwendolyn M. Shaw, James N. Shoolery, Dale H. Smathers, Howard L. Smith, Samuel Solomon, R. Sree, John M. Talbot, James E. Tash, Rodes Trautman,

Leonard W. Tregillus, Jose R. Velasco, James Vlamis, Jack M. Vogel, Patricia C. Wallace, James C. Wallmann, Allen N. Webb, John W. Weldon, D. Emerton Williams, and Richard D. Wolfe.

I checked in with Miss Kittredge and then went to the hill. There I read a note from Warren Weaver, dated September 15, who thanked me for returning my corrected radio script and said that he will let me know what is going to happen with the scripts as soon as he knows.

After some routine matters, I made the rounds of the labs.

Wednesday, September 24, 1947

On campus Perlman, Cunningham, and I talked with a number of incoming graduate students who have indicated they are interested in doing their graduate research with our nuclear chemistry group.

Later I caught up on some of my journal and report reading.

Thursday, September 25, 1947

I looked over my notes and then at 10 a.m. I gave the introductory lecture to the Chemistry 123 class.

In today's mail a letter arrived for me from a Benjamin Max Franklin of Enid, Oklahoma, beginning, "Attraction is inconceivable. Repulsion can be explained as matter in motion,..." I told Mary to file it in the crack-pot file.

Friday, September 26, 1947

Considerable time today was spent on my correspondence. Lynn White, Jr. (President of Mills College) asked on September 18 that I address the student body on November 11 on the subject of atomic energy and its implications. In my reply today I said that I will be happy to speak at Mills College on the subject, "Peacetime Applications of Atomic Energy," but that I would rather not say anything about the political and international implications because of my official connection with the Atomic Energy Commission.

To Martin Kilpatrick (Illinois Institute of Technology), who asked on September 19 for an evaluation of Herbert H. Anderson, I wrote my usual letter, saying that Anderson has a great deal of experience in the synthesis and identification of new inorganic compounds but that he is a lone wolf type of worker--probably because of his shyness and introversion. I suggested that Kilpatrick have a personal interview with Anderson.

In reply to a September 22 request from George Baitsell that I accept a limited number of engagements for Sigma Xi lectureships, I explained that I have tied myself up with so many engagements this year that I am finding it almost physically impossible to fill them. In fact, I said, I am scheduled to be away from home about half of the time in spite of the fact that I am supposed to be teaching an undergraduate course and directing the research of a substantial group of graduate students.

I also wrote to L. S. Deatly (Kansas City ACS Section) to verify that Mary Bender, who contacted me in New York, was correct in saying that I could not stop in Kansas City for a talk.

I wrote to H. W. Pearsall (Assistant Editor, Record of Chemical Progress) and explained that the redrawn figure 5 that was sent to him while I was out of town was not entirely correct and that I am now sending him a corrected figure. I also asked if I would receive proof on the article.

A note arrived from Alan F. Clifford, saying that he is sending me, under separate cover, a copy of his master's thesis. Clifford reported that he has decided to stay at Delaware, where the pace is slower, rather than come to California because of his health problems--Clifford has colitis. However, he said, he may want to study in California after he receives his Ph.D.

After telephoning Kenneth Priestley and discussing the consulting contract for Edgar Westrum for the next year, I wrote Priestley a confirmatory memorandum, in which I suggested a pay rate of $25.00 per day.

I mailed 11 copies of a paper entitled "Search for Elements 94 and 93 in Nature. Presence of 94^{239} in Carnotite" by C. S. Garner, N. A. Bonner, and G. T. Seaborg to Hoylande Young for submission for declassification. I explained that Garner wants to publish this prewar work concurrently with the paper, "Element 94 in Nature," by G. T. Seaborg and M. L. Perlman--that paper was declassified a year ago.

I proofread the abstract that I recently wrote for Paper 22.1 ("The New Element Americium (Atomic Number 95)," by Seaborg, James and Morgan) of Volume 14B:

> Several isotopes of the new element 95 have been produced and their radiations characterized. The chemical properties of the tripositive element are similar to those of the typical tripositive lanthanide rare earth elements. Element 95 is different from the latter in the degree and rate of formation of certain compounds of the complex ion type, which makes possible the separation of element 95 from the lanthanide rare-earths.
>
> The name americium (after the Americas) and the symbol Am are suggested for the element on the basis of its position as the sixth member of an actinide rare-earth series, analogous to europium, Eu, of the lanthanide series.
>
> The isotopes found and studied in this work are: (1) Am^{241}, which decays by the emission of alpha particles (energy--5.45 Mev) with a 510 ± 20-year half-life and is produced by the beta-decay of Pu^{241}, which, in turn, is produced by the α,n reaction on U^{238}; (2) Am^{242}, which decays by the emission of beta-particles (~0.8 Mev maximum energy) with a 17-hour half-life or, in another isomeric form, by branching decay with the emission of alpha particles (energy unknown) and beta particles (~0.5 Mev maximum energy) in the

ratio ~0.002 alpha particles per beta particle; both isomers are produced by neutron capture in Am^{241}; (3) Am^{239}, which undergoes branching decay, decaying (a) by orbital electron capture with a 12 hour half-life and emitting 0.285 Mev gamma rays and conversion electrons in addition to the characteristic x-rays and (b) by alpha-particle emission (energy unknown) in the ratio ~0.01 alpha particles per electron capture. This isotope is produced by the d,2n reaction on Pu^{239} and by the α,2n reaction on Np^{237}; (4) Am^{238}, which decays by orbital electron capture with a 50-hour half-life, emitting 1.3-1.4 Mev gamma rays and conversion electrons in addition to the characteristic x-rays. Am^{238} is produced by the d,3n reaction on Pu^{239} and by the α,3n reaction on Np^{237}.

Saturday, September 27, 1947

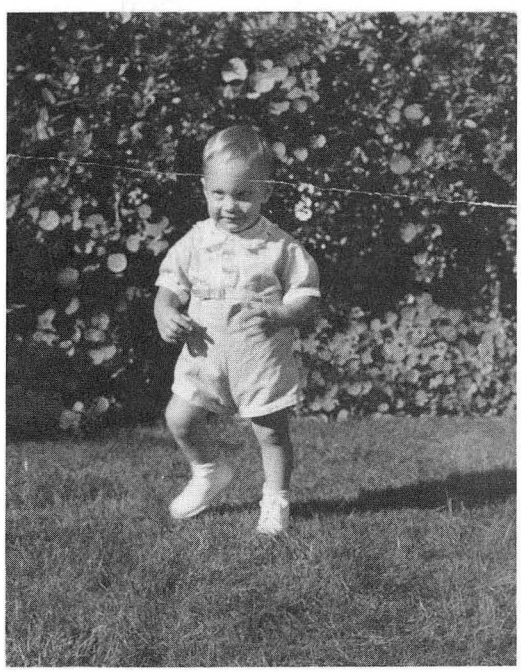
I spent the morning at the lab; in the afternoon I went to Memorial Stadium to watch California beat Navy by a score of 14 to 0. It is apparent that we have a first-class football team this year. This is interesting because many of this year's players were on last year's team which, under coach Frank Wickhorst, lost seven out of nine games.

Sunday, September 28, 1947

During the afternoon Herman Robinson came by and took some pictures of our family:

Helen, Peter and Glenn Seaborg, back yard of 2808 Ellsworth Street, Berkeley, September 28, 1947

Lynne, Glenn, and Peter
September 28, 1947



Peter Seaborg, 2808 Ellsworth Street
Berkeley, September 28, 1947

Monday, September 29, 1947

I made the rounds of the labs and then looked over my correspondence. I received a copy of a letter from Francis W. Test (Chicago Patent Group) to W. E. Elliott (Berkeley Patent Group) regarding my query of September 5 about case S-5625 (neptunium hexafluoride). Test noted that case S-58, in the name of Seaborg and Brown, discloses the higher fluoride of neptunium and a process which would make neptunium hexafluoride. This was filed on January 30, 1943, but Alan Florin, whom I suggested as a possible inventor, was not a member of the Project until August 21, 1943.

I answered a September 24 letter from Fred Albaugh, who asked for my opinion about Gerald W. Sears, who worked with Simpson and Phipps during the Met Lab days. I told Fred that I know that Simpson and Phipps held Sears as almost an equal with Seifert (Ralph L.) and that I recall Sears as having a pleasant but quiet personality; I believe Sears would be a good bet for his organization. In reply to another query, I said that I have just about decided to be satisfied with our Hudson car.

A note went to K. H. Kingdon (Schenectady) to inform him that my reservation on AAL Flight 754, scheduled to arrive in Albany at 9:20 p.m. on October 6th, has been confirmed. I suggested that I take a taxi from Albany to Schenectady to save him the trouble of meeting me.

I also put in a call to Joe Kaplan (UCLA) about the invitations in his letter of September 4. I agreed to arrange to talk on January 3 at the UCLA meeting of the American Physical Society on "Nuclear Transmutations with High Energy Particles."

The text of my talk, "Nuclear Transformations in the New High Energy Ranges," which I gave at the New York Meeting of the American Chemical Society on the occasion of the presentation to me of the ACS Award in Pure Chemistry on September 15, appeared in the September 29th issue of Chemical and Engineering News [C & E News 25, 2819 (1947)].

Tuesday, September 30, 1947

The following people attended this morning's 8 a.m. group meeting: Asprey, Ballou, Barton, Bradley, Cunningham, Eyring, Fries, Garden, Ghiorso, Glenn (an electrical engineering graduate student who will do his graduate research with our group), Goeckermann, Hicks, Hopkins, Howland, Huffman, James, La Chapelle, Lindner, Magnusson, Miller, Neumann (our new graduate student), Perlman, Robinson, Seaborg, Stewart, Street, Templeton, and Werner. I began by announcing that the joint Tuesday afternoon meetings with the Gilman Hall group will resume on October 7 and our own meeting will be shifted to Thursdays at 8 a.m. I also told the group that a new accelerator, being designed by Bill Brobeck, will be built here at Berkeley; it will accelerate protons to about 5 Bev (5000 Mev); this is exciting news.

Street told about his new results from a bombardment of Pu^{239} with 40 Mev helium ions. The americium and curium were separated from rare earths by fluosilicate cycles and from each other on a Nalcite column; no 46-hour curium activity was found this time, and only the 50-hour activity was found in the americium fraction. I commented that

this confirms the assignment of the 50-hour isotope to Am^{238}, if we are correct in our belief that Cm^{240} decays in part by K capture. If the 50-hour americium were Am^{240}, it should have appeared in the curium fraction as a decay product.

Werner described his successful weighing, using a double capsule made entirely of platinum, of 25.5 micrograms of curium oxide from the second Hanford irradiation (51NB). The alpha assay corresponds to 75% of Cm^{242} by weight; the remaining 25% is attributed to Cm^{243} or impurities or both. Werner said that the recovery of the remainder of the curium has begun.

Ghiorso said that he has not measured the slow neutron fission of this material yet--the Cm^{243} should show up if there is several weight percent present. Ghiorso is going to take a sample to Chicago, along with americium and curium fractions from a long bombardment of Pu^{239} with helium ions from the 60-inch cyclotron and for comparison, some low gt curium (pure Cm^{242}) requested from a Clinton irradiation of Am^{241}. He mentioned that they were unable to observe the spontaneous fission of curium by means of alpha photographic plates because of fogging of the film by alpha particles.

Robinson reported that Barton will first work on the Cauchois x-ray spectrograph, which will be out of the shop soon. He expects to be able to determine energies of x-rays of elements from copper up to the heavy elements.

Asprey reported that he is attempting to prepare $AmCl_2$ in anhydrous $HCl-C_2H_5OH$ solution by reduction with magnesium--he has now prepared $SmCl_2$ by this method. He found no apparent change in the supernatant liquid and no new precipitate.

Miller announced that they are going to look for possible formation of proton emitters in a short bombardment of copper in the 184-inch cyclotron. I said that even if proton emission is energetically possible, it is improbable that it could compete with positron and gamma emission, and Ghiorso said that protons will not be easy to detect in a sample containing much positron or negative beta activity.

Lindner talked about some of the activities found in the Te, Sb, Sn, In, Cd, Ag, Pd, Ru, Mo, and Y fractions from a long bombardment of antimony with 200 Mev deuterons. It is too early to give quantitative results.

Eyring reported that the bulk of the sample reported last week as $NpCl_3$ contained about 1 1/2 chlorine per neptunium according to a microtitration of chlorine. A sample just prepared in an attempt to produce $NpCl_3$ gives the x-ray pattern of NpO_2 and lattice constants identical to Zachariasen's, but the color is light green rather than dark brown or black. Cunningham suggested the green compound is probably the first pure NpO_2 since dark colors are usually due to mixtures of oxidation states. Eyring also said that they plan to make some EuF_3 as a stand-in for americium in metal production practice runs.

Hopkins reported some quantitative values for the yields of various products formed when arsenic is bombarded with 200 Mev deuterons. Ballou then talked about the study of the products from a uranium plus 200 Mev deuteron bombardment: the yields of Re^{186}, Re^{188}, and Os^{191} appear to be about 100 times lower relative to Ba^{140} than was reported by O'Connor.

Hicks mentioned that they have made an apparently successful Nalcite column run for the separation of americium and curium at increased flow rate at 78°C.

Finally, Huffman reported on progress by Beaufait on the study of the possible separation of zirconium and hafnium in an Amberlite IR 4 resin column.

* * * * *

At 10 a.m. I taught the Chemistry 123 class, discussing such concepts as spin, magnetic moments, quadrupole moments, etc.

Yesterday, I received a letter from E. J. Durham (Secretary of the New York Section of the American Chemical Society), who wrote that I was unanimously elected Nichols Medallist for 1948. The award will be made at a joint meeting of the New York Section of the ACS and the American Section of the Society of Chemical Industry on Friday, March 5, 1948, at the Hotel Pennsylvania. Durham asked for my acceptance, a biographical sketch, and photographs in addition to suggestions for speakers to discuss my personal history and my scientific accomplishments. The award pleases me, but I am in a quandry about possible speakers. Before returning to the hill this morning, I stopped in and talked with Professor Latimer about the problem and asked his advice. Then back on the hill I dictated a letter to Durham, accepting the honor and sending him the materials he requested. I suggested the name of Winston Manning as one who could review my accomplishments. For the other speaker I said I would be pleased to have W. A. Noyes, Jr., do this, but I am not certain whether he knows me well enough. I also offered the name of Ralph Halford at Columbia, as a probable member of the New York Section and someone whom I have known since he was a student here. Finally, I said that possibly the best person would be Wendell Latimer here at the University of California; however, Professor Latimer prefers to have someone from the east give this talk since for him it would entail traveling during the middle of the school semester.

I checked on the various research projects but went home early to spend some time with the family--this coming trip is an extended one.

Later I took the limousine from downtown Oakland to the Oakland airport, where I caught United Flight 610 at about 10:15 p.m. for Washington, D. C.

Wednesday, October 1, 1947

Upon arrival in Washington at about 11 a.m., I took a limousine to the Hotel Statler and checked in. I then went to the headquarters of the American Chemical Society (1155 Sixteenth St., N.W.) and met Alden Emery. Over lunch we discussed a number of items including the symposium on the chemistry of the transuranium elements that Katz, Hindman, and I want to have at the ACS meeting in Chicago in the spring. One problem that needs to be solved is that we do not want the papers published in a journal of the ACS until after they appear in the Plutonium Project Record. Emery said he will write to Arthur Lamb about it.

I also visited with Walter Murphy, Jim Crowe, and Bob Gould; Jim Crowe and I had dinner together.

[In Berkeley, Health Chemistry issued a memorandum over my name to the personnel of our group working with radioactive materials notifying them that it is imperative that they check and record the contamination on their hands.

Carol Hyatt Dauben, Professor William G. Dauben's wife, began working with our group. Mrs. Dauben, who has a background in physics, will assist Dave Templeton with his x-ray crystallography research. Since her clearance has not yet come through, I asked her, when I met her last month, to help me with the literature search for my Sigma Xi lecture in December--"The Eight New Synthetic Elements."]

Thursday, October 2, 1947

In Washington. I took a taxi from the Hotel Statler to the AEC Building and attended a 2 p.m. meeting of the Subcommittee on Research of the GAC (DuBridge, Seaborg, Rabi, and Conant). We decided to propose that the full General Advisory Committee recommend to the members of the Atomic Energy Commission that the AEC support basic research rather broadly in universities in order to fill the time gap pending the establishment of a National Science Foundation.

After dinner at the Statler, I again returned to the AEC Building in order to attend the meeting of the Subcommittee on Reactors of the GAC (Smith, Fermi, and Worthington). The Subcommittee decided to continue its recommendation to the Atomic Energy Commission that a statement be issued to clarify a realistic time schedule for the development of economic production of electricity for commercial use by nuclear piles. The Subcommittee also decided to recommend that the construction of a fast breeder reactor at Argonne be authorized and built as soon as possible. I suggested that a high temperature power reactor also be built as soon as possible in order to identify the technical problems involved; I suggested that Westinghouse be asked to develop and build it. The Subcommittee also discussed the establishment of a reactor development group composed of experts from each of the laboratories and agreed to suggest that the GAC recommend this to the members of the AEC.

[In Berkeley, Iz Perlman gave the Chem 123 lecture. It looks as though he will have to give many of the lectures this semester.]

Friday, October 3, 1947

In Washington. I again took a taxi from the Statler to the AEC Building to attend the 9:10 a.m. session of the GAC. All members were present, in addition to Manley, our support secretary Anthony Tomei, and Palazzalo. Conant reported on yesterday's meeting of the Subcommittee on Research. Smith then talked about the meeting of the Subcommittee on Reactors--the GAC endorsed the recommendation of the Subcommittee that the Argonne reactor be approved and expedited. There was then a long discussion on the subject of a Reactor Development Committee and the question of a chairman. I brought up the subject of a power pile designed to operate at a really high temperature (of the order of 1,000°C). Conant agreed and added that, in view of the need for such a reactor for submarine or airplane propulsion, it perhaps should be built in a "quick and dirty" reactor program by the Commission; he proposed that there was only one individual who could tie together all the elements of this problem and supply the required energy and enthusiasm to accomplish the task--this would be Ernest Lawrence. The session was adjourned at 11 a.m.

We were then joined by Commissioners Lilienthal, Waymack, Pike, and Bacher; Military Liaison Committee members Brereton, Parsons, Solberg, Ofstie, and Hinds; McCormack (Director, Division of Military Application); Fisk (Director, Division of Research); W. Williams (Director of Production); and Fields (Division of Military Application). Williams and Pike reported on the sources of raw material, McCormack spoke on the progress on the forthcoming nuclear test program and new weapons initiator design, and Fisk talked about Division of Research items. Lilienthal told us that authorization has been made for distribution of isotopes to foreign countries and that the Commission has also asked for exemption for its employees from civil service regulations for a three-year trial period. Bacher, in commenting on the statement on atomic power, said the AEC is now grateful for its preparation; he feels it should be rewritten in a style that would make it more understandable to the general public, after which it could be submitted for declassification. Worthington said he believes the statement should include a discussion of the economic aspects of atomic power. This session was over at 12:55 p.m.

At 1:30 p.m. we reconvened, and Fermi commented that there were two items to be considered in the case of the thermonuclear weapons: (1) the production of tritium and (2) some sort of test of the thermonuclear principle. We talked about Edward Teller's report on the subject and decided to recommend that it be downgraded from "Top Secret" to "Secret" so that it could have more widespread circulation. We also suggested that all nuclear properties of tritium be declassified. In a continuation of the discussion on the power reactor program, Conant reiterated his suggestion that we should try to get Ernest Lawrence to head the program, especially if there is a military urgency for such units for propulsion as there seems to be. Rabi was concerned that the implementation of such a proposal would result in grave personnel problems because of competition with other reactor programs. I again brought up the matter of a high temperature power reactor, stating that the problem could be met in an actual reactor rather than along the lines of only component research. An industrial company like Westinghouse, I

said, could be put in charge of development, thus eliminating competition for personnel. Oppenheimer disagreed, pointing out the experience with General Electric and Monsanto and indicating the time is not yet right for a high degree of industrial activity for much still depends on the activities of scientists rather than engineers. We were unable to resolve the differences between the two approaches--Conant's proposal for a "quick and dirty" approach under the leadership of one man, namely Lawrence, and my idea that industry develop a high temperature power reactor for a future source of power in industry. We enthusiastically supported the suggestion made by Oppenheimer and Rabi that Ernest Lawrence be made Chairman of the Reactor Development Committee. Oppenheimer suggested that the report of the Reactor Subcommittee be redrafted to incorporate this suggestion in order to give it a sense of urgency and to include a paragraph emphasizing the need for greater emphasis for high temperature power and the role of industry in such a development. We adjourned at 3:25 p.m.

The GAC reconvened at 3:35 p.m., along with Commissioners Lilienthal, Bacher, Waymack, and Pike; MLC members Brereton, Parsons, Solberg, Ofstie, and Hinds; as well as Wilson; Joseph A. Volpe, Jr. (General Counsel's office); McCormack; and Admiral John E. Gingrich (Director of Security and Intelligence). Wilson opened the session with a discussion of various administrative matters, such as the establishment and functioning of the Medical Advisory Board, a proposed Committee on Exploration and Mining, and an Industrial Advisory Committee. He announced that John K. Gustafson has accepted the position of Director of Raw Materials, and he presented and explained an organizational chart of the Commission's activities. Gingrich then talked about the problems of security and personnel clearances. We emphasized that the Commission should have an enlightened declassification policy in the light of information supplied by intelligence sources. This session was adjourned at 4:40 p.m.

We again met at 4:50 p.m. with only Carroll Wilson present, in addition to our members. Wilson answered our questions about the situation at Hanford, saying that Kellogg-Kellex will undertake the solution of the waste recovery problems, that Standard Oil Development is willing to work on a portion of the Redox process, and that Carbide and Carbon Company will explore the dry fluoride process for waste recovery. After Wilson left at 5 p.m., we talked about the rephrasing of the reactor report and the reasons why the AEC should undertake the support of basic research at this time. We adjourned at 5:40 p.m.

[In Berkeley, I missed the prelims of John Teurney Neu, a student of William D. Gwinn--I was a member of Neu's committee.]

Saturday, October 4, 1947

In Washington. I again went to the AEC Building for the 9 a.m. General Advisory Committee session. Most of the morning was taken up with a discussion of our attempts to revise our statement on atomic power; we finally decided that Fermi and Smith will collaborate on one form of revision and that Oppenheimer and Rabi on another. These will be prepared for comparison at the next meeting of the GAC. We also approved the revised form of the report from our Subcommittee on Reactors, which

incorporated the points we brought out yesterday. This session adjourned at 1 p.m. for lunch.

When we reconvened at 2 p.m., Rabi raised the question of the competition between Brookhaven and Berkeley for a large accelerator. The conclusion we reached was that the Commission should support those activities that are associated with a unique facility of a type beyond the resource of any single institution at a place like Brookhaven; whenever a single university could carry a particular program or enterprise, this should not be incorporated in the Brookhaven activities. We also decided that our priority item for discussion in the forthcoming meeting with the Commissioners will be the organization structure of the Commission staff and our role with regards the technical activities of the Commission. Conant also raised the question of the detection of any possible test of a nuclear weapon by a foreign country. We agreed to take this up with the Commission and the Military Liaison Committee. The final topic today was a brief discussion of radiological warfare. We adjourned at 3:50 p.m.

Later I had a conference with John E. Gingrich, who described the procedure for the new P clearance--a person can be hired for unclassified work after only fingerprinting, but the FBI investigation will go on quickly in the interim. This type of clearance may start next week. We also talked about George F. Hardy and his clearance. Hardy was a member of the AYD in the spring of 1946 at Cal Tech; Gingrich said that he will give the word to hire him. I then brought up the subject of Wilkinson's clearance and asked Gingrich to hurry it along. He agreed to do so.

At 5:30 p.m. Robert Bacher gave a cocktail party at his home at 3611 Kanawak St., N.W., for the members of the General Advisory Committee, the members of the Atomic Energy Commission, and other key staff members.

[In Berkeley the Cal football team defeated St. Mary's by a score of 45 to 6.]

Sunday, October 5, 1947

In Washington. At 10:45 a.m. our committee met at the AEC headquarters with everyone except Conant present. We again went over the revised drafts of the reactor report and the research report; these were approved for transmission to the Commission. We also talked about the problem of declassification and agreed that as much secret information as possible should be declassified in order to make the maximum progress in the research programs. The session adjourned at 12:50 p.m.

After lunch the GAC had a joint session with Commissioners Waymack, Pike, and Bacher; Military Liaison Committee members Parsons, Hinds, Ofstie, Solberg, and Brereton; and AEC staff members Wilson, Fisk, Williams, and McCormack. Oppenheimer then made his usual summary of the deliberations of the GAC over the last two and one-half days. He called attention to the problem of detection any nuclear or atomic bomb tests by a rival country and to the question of radiological warfare, in which one must recognize that defense is not separable from offense. With respect to declassification, Oppenheimer reported our conclusion that it is not

sound policy to retain classification on something that may be of use only in the order of ten years, that declassification policy should be enlightened by information supplied by intelligence reports, and that declassification may be warranted to remove pernicious public misconceptions (such as those current on the power aspects of atomic energy and super bombs). Parsons raised what he considered to be a critical question, namely the staffing and leadership at the Sandia Laboratory. Oppenheimer agreed with him that the man in charge should be a physicist; the names of Professor Joseph Henderson, Edward B. Doll, and Frederick C. Lindvall were suggested as possibilities. This joint meeting was adjourned at 3:20 p.m.

I had several private conversations after the session. In a discussion of controlled thermonuclear reactions, Oppenheimer told me that a temperature of approximately 7 kev would be required to obtain 1% reaction efficiency. Bob Bacher said that the plans for the high flux reactor at Clinton now include provision for a small region of neutron irradiation space where samples can be irradiated at 800°C; the Air Force wants a space capable of accommodating samples at 1000°C. Fermi asked that I tell McMillan that he is worried that McMillan's n-p and n-n (n-d minus n-p) cross sections are too small according to theory.

Monday, October 6, 1947

In Washington. I took a taxi from the Hotel Statler to the National Bureau of Standards (on Connecticut Ave.) in order to attend the 9:30 a.m. meeting of the Subcommittee on Nuclear Constants of the National Research Council in Room 204 of the East Building. Also present were members W. H Sullivan (Chairman), T. P. Kohman (Secretary), K. T. Bainbridge, H. H. Goldsmith, B. T. Feld, L. F. Curtiss, and M. Deutsch. Others in attendance were R. C. Gibbs (Division of Physical Sciences) and I. Feister. Bainbridge reported first on natural isotopic abundances--he plans to list best abundances rather than average values and said there may be systematic errors in Cameron's work. He also told me that he will send me some private results that he obtained in a letter from Mattauch whom he saw in Europe; he gave me Mattauch's values on the isotopic abundances of samarium, neodymium, tungsten, and molybdenum. Feld said that he has been working chiefly on nuclear electric quadrupole moments. Kohman presented his outline for his compilation of radioactive constants of the natural radionuclides. I reported that the Seaborg-Perlman "Table of Isotopes" has been issued as a secret report and that we are waiting for changes in the declassification guide before revising and publishing it. I asked for corrections to it. Deutsch reported on the compilation of data on radioactive disintegration schemes that is being carried out, and Sullivan expects his trilinear chart will appear next spring. Privately, Deutsch told me that a "long lens" magnetic lens beta-ray spectrograph would be our best bet for use in Berkeley and suggested that one of K. Siegbahn's instruments is a good prototype. It was also decided that our Subcommittee will come out with a master compilation of all our tables by about March 1. Curtiss and Sullivan described a set of notes on nuclear chemistry by Art Wahl et al. (some 150-200 pages) that the NRC Subcommittee on Radiochemistry may ask the NRC Committee on Radioactivity to publish in monograph form.

I left Washington via AAL Flight 478 at 5:25 p.m. for New York City,

where I caught an 8:15 p.m. flight (AAL Flight 754) to Albany. After arriving in Albany about 9:30 p.m., I hitchhiked to Schenectady and checked into the Hotel Van Curler.

Tuesday, October 7, 1947

In Schenectady. I took a taxi from the Hotel Van Curler to the General Electric Company (Locomotive Works at 425 Peek St.), where I spent the day consulting with men such as James Marsden, Abe Lincoln Marshall, Edwin Zebroski, John Thomas, Henry Hass, John Flagg, Mike Cefola, et al. We discussed in detail possible improvements in the Redox Process, the solvent extraction process being developed to replace the Bismuth Phosphate Process for the recovery of plutonium at Hanford. The Redox Process has the advantage that it will recover the uranium that now goes into waste at Hanford. I learned that Argonne Laboratory plans to process and recover some low gt plutonium from Clinton for Rudolph Kanne (General Electric, Schenectady), who will use it to help determine the n,γ cross section for Pu^{239}, needed for the design of the intermediate energy pile at Schenectady.

I also saw Gert Friedlander and Morris Perlman, who told me that they and Jaffey use Kodak mounting tissue for their chart of isotopes, fastening by means of a hot soldering iron. Friedlander also mentioned that he has received a job offer from Brookhaven.

At 6:29 p.m. I left Schenectady on NYC Train 65 (Advance Commodore Vanderbilt) for Chicago.

[In Berkeley, Iz Perlman gave the Chem 123 lecture. Unfortunately, I missed another prelim committee meeting today--William Tracy Simpson, one of Pitzer's students.]

Wednesday, October 8, 1947

I arrived in Chicago at 7:15 a.m. and took a cab to the Windermere East Hotel and checked in. After breakfast I again took a taxi and went to Argonne Lab on the campus of the University of Chicago to talk with the men there. Jaffey and I had a discussion about their beta spectrometer, which he thinks will have a 5% geometry and as good resolution as Deutsch has at 1% geometry. Jaffey's spectrometer, however, is very large with high power requirements (~25 KW) with a peak of 100 amp. It uses two Westinghouse magnetic coils. Presently the coils are being rewound to get better heat transfer.

Joe Katz and I got together and talked about the PPR volumes. We also again went over the plans for the symposium on the chemistry of the transuranium elements at the spring ACS meeting in Chicago. At lunch at the Quadrangle Club I ran into J. W. Buchta (Assistant Editor of Reviews of Modern Physics). We talked a bit about publishing the "Table of Isotopes" in that journal as I did with the 1944 version. Buchta also mentioned that Katharine Way is preparing a table of new data that she wants to publish in Reviews of Modern Physics. He promised to send a sample of her sheets for my opinion.

I also looked up Herb Hyman to discuss one of the problems that came

up yesterday in my day at Schenectady. He said that isobutylaldehyde as a holding agent for the reducing agent Fe^{++} would be put in the hexone-uranium solution in the holdup stage for the Redox Process. They have tested this only in batches and found no complexing.

Sherman Fried told me about his unsuccessful attempts to produce AmF_4 or AmF_5 from the reaction of AmF_3 with fluorine gas.

Thursday, October 9, 1947

In Chicago. I again took a taxi to Argonne. One item that I plan to take care of on this visit to Chicago is a problem of recruitment. Iz and I know that Mary Bender, our secretary, will probably leave in the spring when her husband gets his degree. The problems involved with finding a competent replacement are numerous. I talked with Winston Manning and then approached Ruth Rogers, describing for her the many benefits of working in California--the weather, the sights, new horizons, the pleasure of working for me, etc. Ruth, unfortunately, doubts that she will accept, but she will let me know. We did have a pleasant conversation, and Ruth mentioned one possibility--Doral Buchholz, a secretary who worked for Morris Kharasch for a number of years and has worked a few months for Oliver Simpson. Miss Buchholz wants to leave Chicago and go into some other type of work, so she has quit her job with Simpson and plans to visit her brother in San Francisco. Ruth recommends Miss Buchholz--if I can convince her to continue to work as a secretary. I also asked Winston if he would speak on my behalf at the Nichols Medal presentation in New York; he agreed to do so but asked for some biographical information.

Joe Katz and I again worked on some of the problems of the PPR and next spring's symposium.

Later I went back to my hotel, picked up my luggage, and took a cab to the Midway Airport to catch Delta Flight 207, which left at 4 p.m. and arrived in Knoxville about 7:45 p.m. Ed Bohlmann, his girl friend, and another couple met me and drove me to Gatlinburg in the Smoky Mountains, stopping for dinner on the way. I checked into the Mountain View Inn.

[In Berkeley the following people were present at the group meeting this morning: Ballou, Barton, Bradley, Covey, Cunningham, Fries, Garden, Glenn, Goeckermann, Hicks, Hopkins, Howland, Huffman, James, La Chapelle, Lindner, Perlman, Robinson, Street, Templeton, and Werner. First, Cunningham described Asprey's work with the alcoholic magnesium-HCl reduction of $NdCl_3$, $AmCl_3$, and $SmCl_3$.

Howland talked about his experiments with an old target of Pb^{206}, irradiated with 55 Mev deuterons on September 9. He made another bismuth isolation by precipitation methods and found about 10^6 c/m of 6.4-day Bi^{206} in the entire target of 2 g of lead. He repurified the lead by $PbSO_4$ cycles and found a large growth; he believes the lead parent is probably Pb^{200}.

Robinson announced that the Cauchois x-ray camera is finished and has been successfully tested with copper radiation. Glenn said that he has tested the ionization chamber for measuring fission fragments and alpha

particles during the operation of the cyclotron and found it works satisfactorily.

La Chapelle reported a new absorption peak for Np(V) in 0.2 N HCl at about 1098 mμ. Street said that he and James, in order to rapidly separate americium and curium from each other and the rare earths, have been testing a short (1 cm) Nalcite column using fluosilicate as the eluting agent. In an experiment with 61, americium, and curium, the 61 came off in a sharp peak in about 33 hours.

Ballou talked about some calculations and plans for experiments to help determine whether or not an isotope of element 61 can exist in nature. He is planning a Hanford irradiation of samarium to produce Sm^{145}, which will decay to 61^{145}; this he has calculated possibly may be long-lived. Ballou plans to look for Sm^{146}, which he believes may be an alpha-emitter. He also plans to bombard ruthenium in the Hanford pile to produce Ru^{97}, which decays with a 2.8-day half-life to Tc^{97}--this experiment is to check the reported decay of 95-day Tc^{97} by isomeric transition.

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[Perlman gave the Chemistry 123 lecture at 10 a.m.]

Friday, October 10, 1947

In Gatlinburg. Since I am not due at Clinton for the meetings until Monday, I am taking a long weekend here, where Helen and I spent our first vacation (a sort of delayed honeymoon) in September 1942. At that time Helen and I stayed at the picturesque Mountain View Inn and were charmed by its atmosphere and the dinner cuisine of southern fried chicken, mashed potatoes, hot biscuits, and local honey. The Smoky Mountains are beautiful at this time of year, and today I hiked to nearby Cherokee Orchard. After dinner I wandered through Gatlinburg village, its general store, etc.

Saturday, October 11, 1947

In Gatlinburg. I took a long walk into the back country, repeating the hike I took with Helen five years ago, and again saw mountain people with their strange dialect and their mountain cabins. After my fried chicken dinner, I again visited the village of Gatlinburg.

[The California football team under "Pappy" Waldorf defeated the University of Wisconsin in Madison today by a score of 48 to 7.

A copy of Oppenheimer's formal report on the Sixth Meeting of the General Advisory Committee, dated October 10, follows:]

UNITED STATES
ATOMIC ENERGY COMMISSION
WASHINGTON, D. C.

CLASSIFICATION CANCELLED
BY AUTHORITY OF A.B. [illegible], DC, DOE
BY memo DATE 4/18/90

October 10, 1947

326 US ATOMIC ENERGY COMMISSION
SECRETARIAT
1212
GAC

Mr. David E. Lilienthal
Atomic Energy Commission
Washington, D. C.

Dear Mr. Lilienthal:

This letter is our formal report to you on the Sixth Meeting of the General Advisory Committee. It will supplement the informal discussions which the Committee held with the Commission, the Military Liaison Committee, and staff of the Commission. After Subcommittee meetings on October 2nd, the GAC met for three days: October 3, 4, and 5.

Our report to you may be divided into three parts: the first discusses a variety of problems concerning reactors and reactor development, of which the most urgent may well be the authorization to proceed with the construction of a specific reactor proposed by the Argonne Laboratory, the reorganization of the reactor effort to make possible a more comprehensive attack on the problem, and to make progress possible on new designs and new suggestions, and finally, a reaffirmation of our earlier suggestions with regard to Hanford operations. The second part of our report deals with miscellaneous recommendations, many still in a tentative form. The final part of our report expresses the views that we have now reached with regard to the policy of the Commission in supporting basic scientific work relating to atomic energy in the private laboratories and universities of the country.

I. Matters Pertaining to Reactors.

In the absence of a qualified ad hoc committee and in the interest of expedition the Subcommittee on Reactors has studied in detail the "Feasibility Report on the Fast Neutron Pile for a Test or Conversion" by W. H. Zinn (ANL-WHZ-112, August 11, 1947). The General Advisory Committee unanimously recommends the immediate authorization to proceed with detailed engineering and construction, and urges that every assistance be given to the Argonne Laboratory to realize their plans as early as possible. We believe the proposed reactor to be feasible from the nuclear and engineering standpoint. It will provide useful information (not obtainable from other reactors under consideration at the present time) on operation at medium high temperature, on the

use of a new and promising coolant, and on the general feasibility of breeding. It will also provide a relatively early demonstration of useful power production.

Reactor Development Committee

The Committee considered the document "Proposal for Reactor Development Committee" submitted by the Director of the Division of Research. It has been led to endorse unanimously these proposals, not only for the reasons adduced in the working paper, but because of the absence in the present organization of the Commission, its staff and contractors, of any agency which can perform the required functions. Nevertheless, as will appear, the organizational problem of incorporating a committee to provide competent technical planning and policy in this field within the overall organization scheme of the Commission is not without its complexities. We shall return to it.

It is the view of the Committee that the RDC will be effective in the measure in which it acts as a steering committee for the joint enterprise of reactor development. Thus we would hope to see its decisions implemented in the laboratories concerned rather than merely reflected in the transmission of recommendations. To this end, clearly, the majority of the members of the RDC must be actively engaged and technically cognizant in the field of reactor development and must at the same time carry a sufficient authority so that their decisions will in fact be executed.

There are two organizational problems upon the solution of which the success of the proposed committee will depend. On the one hand, in each laboratory represented in the committee, appropriate arrangements will have to be made to enable the delegates of the laboratory to commit it in certain well-defined areas of action. On the other hand, arrangements need to be set up so that the contracting officers and other agencies of the Commission promptly and effectively respond to the decisions of this committee in a manner which will allow these decisions to be implemented in the laboratories concerned. We note merely that no existing structure in the Commission's organization would now appear capable of performing this last function.

Conceivably the committee could elect its own chairman. To us it seems far preferable that the chairman be designated by the Commission and assume immediate responsibility for a program second in importance among the Commission's activities only to weapon production and development. He must be a man of aggressive and enthusiastic personality, of more than usual scientific and managerial ability to whom the Commission can and will entrust broad authority and who can devote nearly full time to this work. Dr. E. O. Lawrence is suggested as an ideal appointee.

Reactor Program

The committee reiterates its belief that no useful purpose would be served by promoting a "quick and dirty" approach to power production for publicity purposes; indeed, it believes that such activity would both harm and retard useful power production. Nevertheless, it is to be noted that no part of the present program is aimed directly toward either large-scale power production or high temperature operation. Since it appears difficult to embody in a single reactor high specific power, high power level, high temperature, good material economy, and good neutron economy, sympathetic consideration should be given to reactor designs which embody some but not all these features. The interest of the Services in both ship and aircraft propulsion makes increased activity in these directions desirable. As a minimum, the basic engineering and materials research on which a workable reactor can be based should be considerably intensified.

A further important program which needs study is the design of power reactors using natural uranium.

When the problems of reactor development are comprehensively formulated and understood as they would be in a central laboratory and as they may be with a suitable RDC, many specific undertakings will appear in which industry will be interested and for which it is at this time qualified. In this way a broader and more satisfactory involvement of industry in the atomic power program will be realized than would be presently possible by the assignment of overall reactor development contracts. It appears that this will greatly multiply the opportunities for industrial participation.

Fabrication Facilities

One of the very important problems of future reactor development is that of obtaining a satisfactory fuel element. The present concentration of fabrication facilities primarily for weapons does not conduce to rapid progress on this problem. It is therefore recommended that a facility be set up for the sole purpose of producing nuclear fuels in the forms needed for the various reactors, and for carrying out intensive studies both of fabrication and utilization of all types of fuel rods. This should be at one of the main sites of reactor activity (either Clinton or Argonne). Though it should have no direct responsibility for bomb production, its existence would naturally strengthen and support the existing facilities for this purpose.

Approval Procedure

The Committee considered the paper "Proposed Procedure Covering AEC Approval for Reactor Construction." It unanimously approved sections I and II of this proposal as an interim policy pending the establishment of the RDC. In the interest of

expedition, it is urged that at the completion of stage II the responsibility for proceeding with construction rests with the laboratory concerned. It is desirable that construction drawings be furnished the AEC as the work proceeds but for information rather than formal approval.

Heavy Water Needs

The Committee had been requested by the Director of the Division of Research to consider the possible needs for heavy water, and to comment on the proposal to discontinue further production. After due discussion it was resolved that the Committee recommends continued production of heavy water at the present scale because of possible future demands for both reactors and weapons. The proposed Reactor Development Committee should promptly reexamine reactors employing heavy water, particularly those for power and plutonium production from un-enriched uranium.

Hanford Operation

The Committee discussed Hanford operations and the difficulties associated with operation to a higher g/t level. The Committee noted with approval the initiation of steps to favor Hanford operation at a higher level. We note with regret, however, that these efforts may not lead to a rapid solution of the problem because of the piecemeal nature of the experimental attack and the long-time scale involved in determining the causes of blistering. We recommend further exploration on a laboratory scale in the interests of a rapid solution.

The Committee believes that the advantages of segmented pushing warrant the institution of this operating procedure as early as possible.

The previous recommendation concerning an increase of the grams per ton in Hanford product is reiterated and emphasized. No plutonium 240 concentration likely to be realized with the present Hanford reactor will have a serious adverse effect on weapons. Urgent arguments of material conservation therefore dictate the highest practical g/t.

II. Miscellaneous Recommendations

E. Thermonuclear Energy. The Committee considered the admirable report of Dr. Teller on the status and prospects of weapons based on thermonuclear reactions. On the basis of this report we concluded that the program of basic studies recommended in the report should be encouraged as a healthy and useful part of the work of the Los Alamos Laboratory, and at least a necessary condition for progress in this field. The Committee wished to study the matter further before endorsing proposals for greatly increased tritium production on the one hand, or for

a weapons test of thermonuclear reagents on the other. These questions were made an order of business for the forthcoming meeting. The Committee did, however, note that even with the most optimistic interpretation of present knowledge the realization of thermonuclear weapons is many years in the future, and that in many cases our knowledge of the subject is fragmentary and inadequate. We believe that the remoteness of the military application and the difficulty of the problem do not justify the high classification -- "Top Secret" -- now attached to this work. We further believe that it is desirable, precisely because of our inadequate understanding, to stimulate further participation in the study of these problems. We therefore recommend to the Commission that scientific work bearing on the development of thermonuclear reactions be classified not higher than "Secret."

B. "Power Draft." The Committee found itself in sympathy with the view of the Commission, that the draft statement on atomic power prepared at our Fifth Meeting gave too incomplete an account of the situation to be useful for public distribution. It was agreed that Drs. Smith and Fermi on the one hand, and Drs. Rabi and Oppenheimer on the other, would draft provisional statements on the situation with regard to atomic power, taking into account only the necessity for an adequate understanding, and paying little attention to the classification of the material used.

C. The Advisory Committee was glad to note the proposed change in urchin design, which it regards as quite sensible.

D. The Advisory Committee, aware of the fact that some present declassification policies may well be retarding our own progress without retarding rival undertakings, considered tentatively certain further criteria, beyond those proposed in the Tolman report, on the basis of which declassification might be recommended. A further study of this problem is an order of business for our next meeting.

E. The Properties of Tritium. The Advisory Committee recommends the declassification of all nuclear properties of tritium. The reasons for this recommendation include:

1. As pointed out above, the military application of thermonuclear reactions, which alone might seem to justify retaining classification on the properties of tritium, is a very long-range problem, to the solution of which a knowledge of the properties of tritium contributes relatively little.

2. Facilities outside the control of the AEC suffice as they in fact were historically, for determining the nuclear properties of tritium.

3. Tritium is from the point of view of theoretical nuclear physics so important a substance that studies of its properties cannot be prevented, and that knowledge of these properties will simplify and fructify its use.

4. Papers already published reveal our possession of sufficiently large amounts of tritium that our failure to report its nuclear properties would indicate an abnormal interest in that material.

F. Test Detection. The Advisory Committee is not informed on the program of making as certain as possible that we could reliably detect a test of an atomic explosion conducted by a rival effort. It regards this problem as worthy of very serious study. In particular, it believes the study should be predicated on a possible desire of our rivals to conceal the test. Therefore, questions arise of which the following are typical: Can an explosion be carried out so deep in the ground that radioactive contamination is unobservable at great distances? If so, can seismograph records reveal the occurrence of the test? Are our own activities in the field of atomic energy reducing sensitiveness of measurements of the radioactivity of xenon in the troposphere? The Advisory Committee has as its primary concern, not that it be informed about progress in this work, but that the progress be adequate and the work in competent hands.

G. Radiological Warfare. Similar considerations apply to the problem of radiological warfare. Here again the Advisory Committee neither is, nor perhaps needs to be, informed of work in progress. Here again, it regards it as its duty to point to the importance of the problem and to indicate that, even though our interest in the problem be entirely defensive, competent study may not be fruitful unless work on the offensive possibilities is also carried out.

H. Raw Material. The Committee recommends that for our Seventh Meeting the Commission arrange for our meeting with the Director of Raw Material, and, if possible, with the Special Advisory Committee on Raw Material.

III. Commission Support of Science.

The General Advisory Committee has given further consideration to the question of the role which the Atomic Energy Commission might play in the support of basic science in this country and believes that the AEC has a great and attractive opportunity for service in this field.

The Atomic Energy Act specifically charges the Commission with the support of research in fields relating to atomic energy, not only within the Commission's special facilities (such as

Brookhaven and Berkeley), but especially in the universities and other research establishments. Though the Act does not indicate that the priority of this task compares with that of weapon or reactor development it does make clear that an important purpose of the Act is to build and maintain a strong free science.

It is reasonable that this should be an important activity of the AEC; for the degree to which the United States maintains leadership in the atomic energy field depends, in the immediate as well as the more distant future, on the virility of its basic science. It is also clear that relief from the present serious shortage of scientific manpower can come only from the universities, and they are already facing a serious shortage of funds and facilities to maintain adequate graduate training programs.

The views of the GAC on this matter have matured and to a considerable extent altered in recent months. It is now clear that the AEC's efforts in the weapon field must remain largely hidden from public view, and its program of development of useful nuclear power plants will not result in direct benefits to the public for many years. A vigorous program in scientific research, however, can bring early and obvious benefits.

It was until recently believed that a Science Foundation might soon be established to assume some of this responsibility. It now appears that such a Foundation may not exist for some time, and AEC policy cannot be predicated on a hope which may not soon be realized. We have thought that in any case that the AEC should share with a foundation the responsibility for insuring the development of the nation's scientific potential.

With the future of the National Science Foundation uncertain we would no longer wish to restrict, as we did in an earlier paper, the interests of the Commission to basic nuclear science. We recommend that it be extended, as provided in the Act, to research relating to atomic energy.

Finally, we have not pressed this matter earlier because we recognized that the AEC must perforce attend to more urgent tasks - the direction of its continuing enterprises, the formulation of operating policies, and the building of an adequate staff. Though we know these tasks are not complete, we feel the time has arrived when action on the support of research is possible; in fact we believe that further delay will cause damage to science and will result in a growing disappointment at the achievements of the Commission.

We do not believe that a large sum of money is required to make a very large contribution in this field. A definite allocation should however promptly be made. A sum of between 10 and 30 million dollars, for example, would make a very important contribution.

We do not believe this program should or needs to interfere with other Commission programs, especially as regards manpower. Projects can and should be selected for support if and when they use personnel not now engaged in AEC activities. It will be only a short time before the universities that are strengthened in this way will be paying dividends in additional trained men.

In particular, we desire to emphasize that these present recommendations do not refer to a large increase in the research facilities within the Commission's regional laboratories. They mean providing support to private institutions of research, primarily the universities; they mean limiting the scope of the regional laboratories to work for which Commission-owned facilities are unique and necessary to the healthy growth of the basic sciences.

We recommend that a panel be appointed to assist in a program of support of research in laboratories outside the Commission. The GAC will be glad to assist the AEC in evolving policy for this program.

We note that the Act stipulates that such activities be not delegated to the Division of Research. Consequently, we propose that a special assistant to the General Manager be named to establish an office and staff to supervise this program with the advice of the aforementioned panel, and in consultation with the Division of Research.

We recommend that this officer be instructed to establish promptly a program of grants-in-aid in support of worthy projects, that he keep in touch with scientific developments in order to seek out men and projects especially worthy of support, and that the areas in which support be placed be described broadly. If and when a Science Foundation or similar agency is established, the AEC will of course need to reexamine its relation to this problem.

We believe it is important to the future of the country, and to the strength and success of the AEC that it move into this field with promptness and with vision.

In the past our predictions as to what we would discuss at a next meeting of the Advisory Committee have been singularly inaccurate. We are now scheduling for our next meeting at least five topics:

A. A further discussion of criteria for declassification.
B. A consideration of draft statements on atomic power.
C. A consideration of the thermonuclear explosive program.
D. A discussion of raw materials.
E. A study of the activities of Z Division at Sandia.

J. R. Oppenheimer

J. R. Oppenheimer
Chairman
General Advisory Committee

Sunday, October 12, 1947

In Gatlinburg. Today I climbed part way up Mt. Le Conte and back, recalling that five years ago Helen and I climbed to the top and spent an extremely cold night in the Lodge there after a rather miserable dinner prepared from canned goods.

In the late afternoon my friends drove me to Oak Ridge, and I checked into the Guest House.

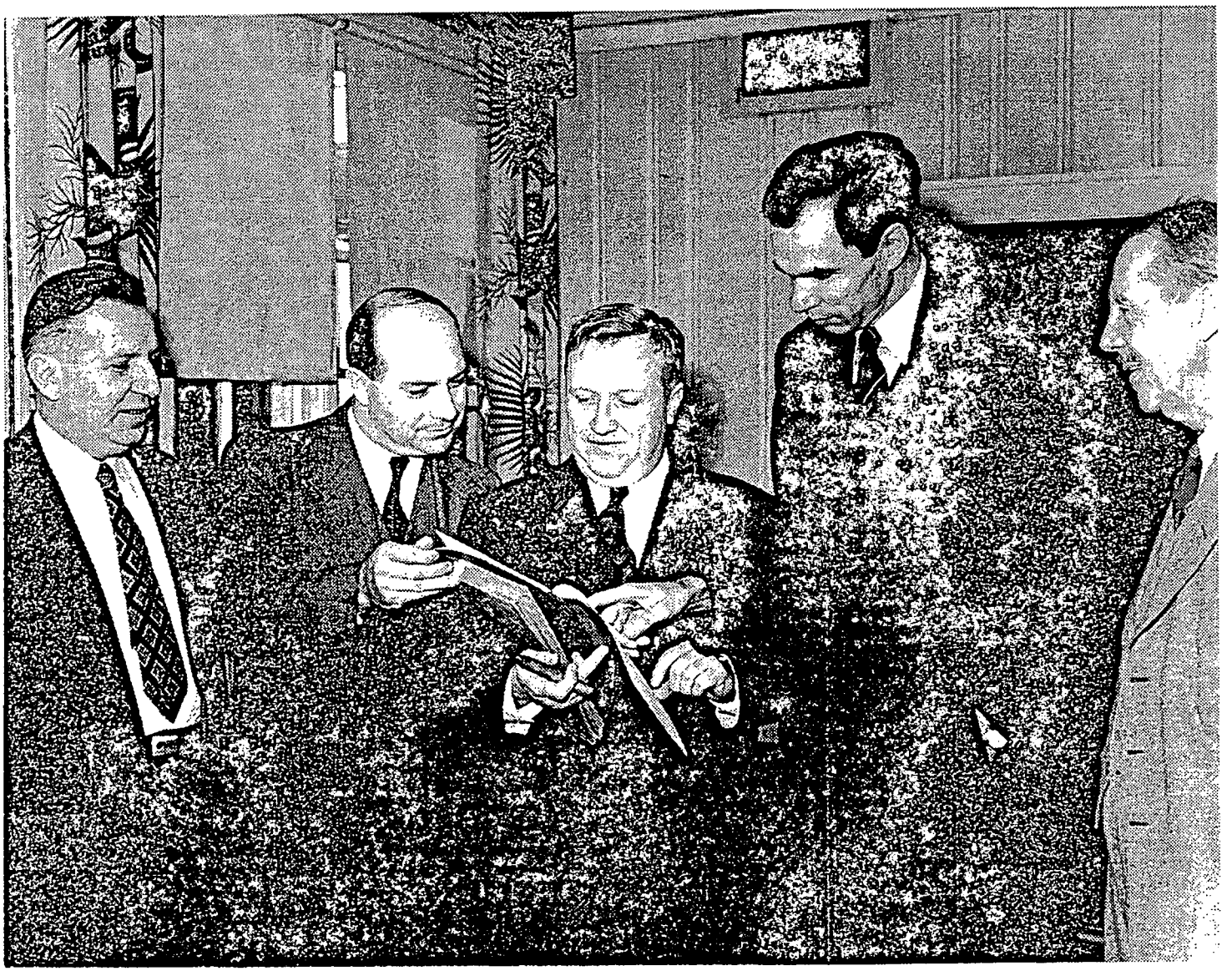
Monday, October 13, 1947

In Oak Ridge. At 8:30 a.m. I went to Conference Room 107 of the Training School Building (735-B) for a Physics Information Meeting, chaired by Norris E. Bradbury. One talk of interest to me was that by Ellis Steinberg (with J. Seiler, A. Goldstein, and A. Dudley), who spoke on "Fission Product Yields from U^{233}". He mentioned that they have found that the central dip in the Pu^{239} slow neutron fission yields is less by a factor of 3 than that of U^{235} or U^{233}. There is a shift of the heavy as well as the light groups.

At 11 a.m. Norman Hilberry became chairman. R. L. Macklin and G. B. Knight reported a half-life of 25.51 ± 0.93 hours for UY. They reported a 34.5 kev gamma ray and 16 kev x-rays (both measured by absorption methods). Art Snell reported on "Progress of the Neutron Decay Experiment" (half-life of greater than 10 minutes). After the session I talked privately with Knight and suggested that their interpretation of the x-rays is wrong--that their effect is due to the rather orthodox and well-known internal conversion process of a nuclear gamma ray.

The afternoon session on "Isotopes" was chaired by Paul C. Aebersold. I had a number of private conversations during the afternoon: Tony Turkevich told me about a 5.4-hour and a 24-minute silver observed in the slow neutron fission of uranium--he mentioned that he noted in Science Abstracts that some Germans found a 20-minute and 3-minute silver. I also talked with Cass Borkowski about the efficiency of our bell jar-type counters. Wendell Peacock mentioned a magnetic lens beta spectrometer described by Robert Osborne at an April 1946 American Physical Society meeting. Jack East of Clinton Laboratories described a vibrating reed electrometer (available for $1,500 from Applied Physics Corporation in Pasadena) that can be used with an ionization chamber for beta and gamma counting. Simon Freed told me that he has found a rough correspondence between the sharp lines of optical absorption between solid $EuCl_3$ and $AmCl_3$, a good confirmation of my view of the analogy between europium and americium. I learned from Ed Tompkins that he is waiting for some resin from Dow for his rare earth separations and that he is taking a year off to work for Paul Aebersold in a travelling capacity.

At one point an Oak Ridge photographer caught up with some of us, and I was recorded in a pose with Lawroski, Dunning, Spedding, and Lind:



Stephen Lawroski, John R. Dunning, Frank H. Spedding, Seaborg, and Samuel C. Lind, Oak Ridge, October 13, 1947

In the evening at a "smoker" in the Grove Recreation Hall sponsored by the Monsanto Chemical Company, I learned that Morse Salisbury will be the AEC Chief for Information, Ed Trapnell will be Associate Chief for Information, Harold Fidler will be Chief for Declassification, and Alberto Thompson will be Chief for Publications.

Tuesday, October 14, 1947

In Oak Ridge. J. R. Coe chaired this morning's 8:30 a.m. meeting on "Chemistry." As the first speaker I talked for 40 minutes on "Summary of Latest Results of the Berkeley Chemistry Group." I covered both our work on the transuranium elements and our work on the transmutations with the 184-inch cyclotron.

After my talk, S. W. Mayer spoke on "The Separation of Heavy Elements by Ion Exchange." He has used 5% tartaric acid to elute protactinium and is working on an adsorption method to decontaminate U^{233}. D. F. Peppard talked about "Extraction of Ionium from Mallinckrodt Raffinates," and F. Nelson discussed his work with K. A. Kraus on "Hydrolytic Behavior of Uranium(IV)." I took over as chairman at 11 a.m. Among the speakers were R. W. Woodard on "Exchange of U Between U^{+4} and UO_2^{+2} in Dilute Acid Solutions" and J. W. Jones on "Activation Measurement of C-neutrons." Frank Spedding was chairman in the afternoon.

I had a number of interesting conversations today: Warren Harris told me he will ship some of the heavy rare earths on Covey's list when they become available. I learned that John Swartout is going to take Waldo Cohn's place at Clinton National Laboratories (research on preparation of radioisotopes) since Cohn is going back into biological research. Ed Tompkins told me about some work Matheson tried on the separation of hydrogen and deuterium with Dowex-50. J. W. Ruch described to me their method of separation of technetium from its carrier rhenium by fractional distillation with $HClO_4$--the technetium comes first. He showed me 5 mg of technetium in the form of ammonium pertechnetate, separated from uranium fission products at the Clinton National Laboratories with the help of George Parker. [Parker is going to visit us at the Radiation Laboratory in order to measure the absorption spectrum of element 61 in aqueous solution using a sample that he will bring with him.]

I was told that the Clinton high flux reactor will be ready by 1950 and will operate at a flux of 2×10^{14}. A. W. Martin (Monsanto Dayton Laboratory) told me about their work on pure polonium compounds. They have prepared yellow-orange PoO_2. E. F. Joy (Dayton) described their preparation of red $PoCl_2$ and yellow $PoCl_4$, and R. Staniforth (Dayton) told me about their work on colloidal polonium.

I met Arthur Fry, a potential graduate student for Berkeley who obtained his bachelor's degree from Montana State (Ray Stoughton said that Fry is very good). Fry said he was accepted last year, but he didn't come to Berkeley then. I explained our setup--Fry said he is interested in organic chemistry and will let me know his decision; he introduced me to his fianceé.

Glen Clewett, a classmate of mine at UCLA, told me that he is now in charge of chemical research at the Y-12 area at Oak Ridge--he has a group of about 60 people.

W. H Sullivan also talked with me. He has recently been appointed Scientific Director of the Naval Radiological Defense Laboratory at Hunters Point and naturally wants to staff it well. Sullivan mentioned that he has approached Nate Ballou, our postdoc, about a position with him.

Wednesday, October 15, 1947

In Oak Ridge. Ernest Lawrence asked me to attend the meeting of the Laboratory Directors for him today. The meeting was held in Wing No. 3, North, First Floor, Main Administration Building at Townsite at 9:30

a.m. There were about 30 people in attendance. Fisk turned the meeting over to Fletcher Waller (Director of the AEC Organization and Personnel Division), who introduced John Gingrich (in charge of security). Gingrich talked extensively on clearances and said, among other things, that most denials of clearance are due to bad character, such as criminal record, habitual drunkard, etc. He hates to deny clearance on the basis that a man is a potential traitor to country. Gingrich sees no reason why scientists should not go abroad. In the past the labs have had (1) controlled, (2) limited, and (3) exclusion areas, but (2) is no longer applicable; they consider the new P clearance (fingerprint only) sufficient for (1). There was a discussion about whether work is born secret if it is supported by the AEC. In addition, we talked about the concept of open research areas; Waller indicated that a committee will be appointed to work out a plan for this. Waller then talked about the Loomis report and the philosophy of decentralization. The next topics were salary structure (which has been revised downward), a structure for travel mileage reimbursement, per diem, consultant fees, etc. I asked about AEC employees being paid as consultants--Waller and others think that it should be left up to the Director of the Laboratory. Vacations, retirement, social security, and insurance were also covered. It was agreed that the next meeting of the AEC Laboratory Directors will be held at Brookhaven about the time of the American Chemical Society meeting.

I looked up Gingrich in order to talk with him again about Sam Naiditch and the problem of secretaries who require Q clearances. He explained that they can get an emergency P clearance in order to be put on the payroll. Gingrich emphasized that he, not the FBI, has the final say with regard to clearances.

Alberto F. Thompson and I talked privately about arranging for some typists for Joe Katz and the PPR papers. He said that he could arrange it if necessary. Hoylande Young offered to furnish some typists who are used for other things.

I learned that the planned pile at Brookhaven will have 110 tons of uranium, operate at 28,000 KW, and have four times the neutron flux of the present Clinton pile.

Later I visited the K-25 area (gaseous diffusion plant for the separation of uranium isotopes) with Clark Center, Superintendent of the plant. There I talked with a number of people: Claude N. Rucker, Superintendent of the Y-12 area (electromagnetic separation plant), Clifford K. Beck (in charge of research at K-25), F. W. Hurd (who will head the Hanford waste study), Sylvan Cromer (in charge of development at K-25), and A. P. Huber (Superintendent of processing at K-25). We discussed in some detail the plans for correlating the operations of K-25, Y-12, and Hanford. The depleted uranium from Hanford can be used as feed material for K-25, perhaps with some prior enrichment at Y-12. Rucker has written a 12-page memorandum on this. I saw Glen Clewett again, learned that he is working on a two-phase solvent extraction method for separating the uranium (using water, chloroform, and cupferon) and has obtained some positive results with a six-stage setup. I was told that George Felbeck (Vice-President of Carbide and Carbon Chemicals Corporation, a subsidiary of Union Carbide) is in charge of K-25 and spends about one week per two months here. There are now 5300 employees

at K-25 and 2300 at Y-12.

Thursday, October 16, 1947

In Oak Ridge. Along with about 45 other people, I attended the conference on solvent extraction, which was organized by Frank Spedding. We heard talks by people such as Francis T. Miles (Clinton, on 25 recovery and decontamination), Merlin D. Peterson (Redox semi-works), Walton A. Rodger (Redox pilot plant), F. R. Ward (23 breeder plant), Edward G. Bohlmann (23 recovery), Merlin Peterson (semi-works for 23 recovery), Winston Manning (on Redox process on which he has 20 people working--on the separation of americium and plutonium--2 men; he has 1/2 man working on 25 recovery and decontamination for the Zinn pile; there are 3 laboratory men and 2 semi-works men working on ionium recovery), Stephen Lawroski (continuous solvent extractor), Louis Kaplan (solvent extraction), Herbert Hyman (chemistry of the plutonium-americium separation), Robert B. Richards (Redox process at Hanford), Weber (from Kellex at Oak Ridge on waste recovery), James Mardsen (Schenectady--has 9 men working on Redox process chemistry), Wendell Latimer (described John Thomas' and Howard Crandall's experiments at Berkeley on TTA process and Theodore Vermeulen's entry into the program), Bob Connick (described TTA process), Vermeulen (semi-works program on TTA process, ready for Hanford scale), Warren E. Winsch (of Brookhaven--no work nor plans on solvent extraction), Wayne E. Hazen (Los Alamos--solvent extraction for uranium and plutonium recovery), William W. Lowe (Los Alamos--solvent extraction recovery of uranium from concentrates and for plutonium purification), Harley Wilhelm (Ames, recovery of rare earths and MsTh), Fisher (Ames, using 20% tributylphosphate--80% dibutyl ether), Charles D. Harrington (Mallinckrodt--described actual column operation in more detail), Glen Clewett (Y-12--described batchwise centrifugal device for use with ether on $Al(NO_3)_3$ solution), and Richard H. Wiswall (K-25--solvent extraction of large amount of uranium in dilute solutions).

Again, I had a number of private conversations. Don Hull told me that they will probably set the date for a meeting on hot labs tomorrow, probably November 20 and 21 in Chicago, and that Nels Garden should attend. Hull will send me the agenda. Spedding informed me that they cannot make 5 mil lanthanum foil, but he will send us some 5/16-1/8" sheet and rod--we should make an official request. Fred Albaugh described his plans for a shielded, air flow protected, 10-stage horizontal extractor at Hanford. I told Ray Stoughton to ask Dewey Norris, a young chemist at Clinton who is working with Art Snell, if he is interested in coming to Berkeley as a graduate student (after first determining whether he is qualified). In addition, I asked Manning to send me the San Francisco address of Doral Buchholz, Oliver Simpson's former secretary; Miss Buchholz seems a likely candidate for a replacement for Mary Bender when she leaves.

[In Berkeley, the regular group meeting was held with the following in attendance: Asprey, Ballou, Barton, Covey, Cunningham, Eyring, Fries, Ghiorso, Glenn, Goeckermann, Howland, Huffman, James, Miller, Neumann, Perlman, Robinson, Street, Templeton, and Richard Wolfe (an Army man who will work for his master's with our group). First to report was Eyring who described his attempts to prepare $NpCl_3$. He has produced a product that may be $NpCl_4$ and a bright greenish yellow product that has the x-ray

pattern of NpO_2. He reported that ignited $Np(OH)_4$ gives a black NpO_2 and then talked about his attempts to prepare EuF_3 and his efforts to reduce EuF_3 to a pellet of metal.

Cunningham mentioned that H. Hicks has completed a Nalcite column run on heavy rare earths, along with yttrium and europium tracer.

Perlman reported that the Clinton-irradiated Am^{241} arrived yesterday; the content of Cm^{243} should be negligible, so it can be assumed to be pure Cm^{242} in our spontaneous fission counting experiments. Ghiorso reported no spontaneous fissions in the first five minutes, but it has been counting overnight and he will soon know.

Glenn talked about the idea he has for a new type of pulse height selector for use with the pulse analyzer and other things. Robinson said that he plans to try some Bi^{206} 6.4-day K capture activity in his Cauchois x-ray spectrograph; he then talked about his tests of the bent quartz crystal. Perlman mentioned that they have run into several presumably K-capturing isotopes in the bismuth region, whose x-rays they have been unable to observe by absorption methods. Perlman and Robinson discussed the new x-ray spectrograph described in the Review of Scientific Instruments.

Huffman updated the group on their hafnium-zirconium separations. They find that hafnium can be adsorbed from fluoride solution by Amberlite IR-4 just as zirconium can; the resin can be used to purify hafnium, but they have been unable to recover all the zirconium from the resin.

Miller said that there is nothing to report on his latest bombardment of copper with 200 Mev deuterons, in which he is taking out fractions from calcium to zinc and beryllium and phosphorus. Cunningham revealed that Hopkins has had a new bombardment of arsenic with 200 Mev deuterons and will look for short-lived cobalt, nickel, and zinc.

Perlman asked the men to review their latest work and decide what data should be included on the latest revision of the isotope chart. In reply to a question from Cunningham, he said that, with regard to the heavy isotopes, their existence can be revealed if they can be made by cyclotron bombardments. Half-lives, radiations, and reactions by which they are produced can be revealed. Then Covey announced that he is collecting data on optical absorption curves of tripositive rare earths and actinide solutions; these will be tabulated and plotted. Covey said he is recovering platinum from alpha-contaminated counting plates, adding that stainless steel plates, which should arrive in November, should be used whenever possible.

Templeton reported that two of the incoming graduate students, Wayne Meinke and Don Orth, have elected to do their graduate research with us. They are now working in Gilman Hall pending their clearances and are bombarding gold with 40 Mev helium ions, which should produce interesting thallium isotopes.

Howland announced that the lead isotope he reported last week as Pb^{200} is probably Pb^{210} since it decays into bismuth rather than

thallium. The lead he used was uranium lead, which Perlman said should not be used in our bombardments. Howland plans to use some lead salts in which the Pb^{206} has been concentrated in the magnetic isotope separator. There was then a discussion about the plans for separating isotopes--here and at Oak Ridge.

* * * * *

Perlman gave the Chemistry 123 lecture at 10 a.m.]

Friday, October 17, 1947

In Oak Ridge. Today's meeting, attended by about 40 people, was on reactors. Speakers today included Lyle B. Borst (Brookhaven--he reported the foundation is already poured for their normal uranium-graphite pile; it will operate at 28,000 KW, give a neutron flux of 4×10^{12}, produce 1500 KW of electrical energy; they have had problems with fabrication contracts, labor, and Q clearances), Miles Leverett (Clinton--showed a model of the proposed pile, 3 kg enriched U^{235} fuel, 2×10^{14} thermal neutron flux, beryllium and graphite reflectors, will break ground in a few months), C. Rogers McCullough (Clinton--reported on a proposed helium-cooled, enriched U^{235}, BeO moderated pile, a precursor to a 225,000 KW pile to produce 10 kg U^{233} per month and is described in report MonN 383), Walter H. Zinn (Argonne--small, Na-K cooled test pile for a fast neutron converter), E. A. Luebkle (General Electric, Schenectady, reported on the GE intermediate neutron energy pile, with enriched U^{235}, high fuel burnup, some 10,000-30,000 KW power level), and Martin (Berkeley, described some plans for fuel element cooling by radiation).

Later I had a conversation with Jack East, who suggested that we should request a copy of their pulse analyzer chamber and its circuit through the AEC if we want one. Parsons would be glad to come to Berkeley to install it.

In the afternoon there was a theoretical talk by Eugene Wigner on the high flux pile, fast breeder pile, General Electric reactor, thermal breeder, with emphasis on fuel doubling time. Gale Young talked about the comparative economics of nuclear and coal-fueled electricity generating plants, including a summary of atomic energy applications--(1) industrial process heat, (2) generation of electricity, (3) naval propulsion, (4) other, such as airborne, submarine, and rocket propulsion. Henry Newson talked on control of high flux piles, Mark of Los Alamos discussed the problem of prompt criticality in a fast reactor, and finally Jane Hall of Los Alamos talked about the control of the Los Alamos reactor by use of the temperature coefficient.

After the meeting I saw Hyman who suggested to me that stainless steel cladding be used for the waste tanks at Hanford.

I also talked a bit with Bill Libby, who said that James Arnold, now at Harvard with George Kistiakowsky, is an extremely good postdoctoral candidate--strong in experimental and theoretical physical chemistry. Bill suggested that we should save the graphite in the Hanford reactors as a source of C^{14} for use in large-scale, low level work in animal

experiments; it could be used also as a source for synthesis of insulin, digitalis, etc.

I left Knoxville on Delta Flight 602, which unfortunately left late, so I missed my flight (United Flight 615) in Chicago; I had to stay overnight in Chicago.

Saturday, October 18, 1947

I managed to get a seat on United Flight 509 to San Francisco and arrived home during the late evening, instead of early morning as planned. It has been an extremely long trip. Helen told me that she and the children are well and that she left the children with Jeanette and had gone shopping today.

In today's football game Cal defeated Washington State in Memorial Stadium by a score of 21 to 6.

Sunday, October 19, 1947

My day was spent reading, resting, and becoming reacquainted with my family. Lynne has grown amazingly during my absence, and Pete seems rather pleased to have her around.

Monday, October 20, 1947

The first thing I did at the lab today was to look over my accumulated correspondence. While I was away, six copies of the Kohman, Seaborg, and Sullivan "Chart of the Nuclear Species" arrived from Hoylande Young. I agreed at the New York meeting to mark one copy for declassification and return it to her. In her covering October 3 letter, Hoylande said that she had asked the Declassification Office to arrange for additional Responsible Reviewers to handle the material in Volumes 14A and 14B.

I read an October 3 note from Eric Löf, whom I saw at the New York meeting. Löf said that he contacted a friend of his in the Swedish-American Line, who got in touch with the Los Angeles office. The Los Angeles office arranged to book my parents for the May 21 sailing from New York to Göteborg (Gothenberg), Sweden.

Rennie Taylor (Associated Press, San Francisco) sent me (while I was out of town) an article that he had written for the Greek newspaper Embros about our work. I did manage to recognize some names when I looked over the clippings today.

James M. Church (National Secretary of Phi Lambda Upsilon) wrote on October 6 that he was sorry that I was unable to stay for the Phi Lambda Upsilon luncheon in New York for my formal initiation into honorary membership. He said, however, that I may expect to hear from someone of the Alpha Kappa Chapter (UCLA) about arrangements for the ceremony to be held in Los Angeles this fall when I am able to visit there.

Joe McCarthy of Virginia City, Nevada, wrote on October 9, offering the location of a couple of abandoned mines with large, open stopes; he

understands the government is interested in such mines since the Russians have completed their large underground airport. He also suggested that we try separating U^{238} and U^{235} in the same manner that the Mint uses to purify gold.

I looked over an October 14 memo prepared by Horace Hopkins on the work he and Henry Neumann have done on the investigation of Se^{72} and As^{72} radiations.

On October 14 Saul Winstein sent me the publication list he promised when I saw him in New York. I shall use this when I prepare a letter for his nomination for the ACS Award in Pure Chemistry. For some of the articles on the list that have not yet appeared in print, he sent some very favorable comments of the referees. In addition, he described generally the nature of his work.

I also read a September 29 letter from Gert Friedlander, reporting that he received a job offer from Brookhaven, something that I learned while I was East.

Joe Katz and Clark Hindman have been very diligent in their efforts to put together a symposium on the chemistry of the transuranium elements for next spring's ACS meeting. In today's mail I received carbons of letters, requesting papers and abstracts, from men such as Bob Connick, R. C. Thompson, Cliff Garner, Art Wahl, R. E. Rundle (Ames), Leo Brewer, Ed Westrum, Al Florin, Amos Newton, Bill Libby, Charles Prescott, Ray Stoughton, and I. B. Johns (Monsanto). They also sent me a carbon of their letter written to Farrington Daniels, in which they asked for suggestions about the symposium.

Tuesday, October 21, 1947

Before going to campus, I answered a couple of letters. Maurice L. Huggins wrote on October 8 to invite me to attend this year's Harrison Howe Lecture and dinner on November 17, if I am in the area. Today I replied and explained that I shall be giving one of the Nieuwland Lectures at Notre Dame University on that date; I extended my congratulations to Drs. Carl F. and Gerty T. Cori, this year's lecturers.

I wrote to Professor John C. Bailar, Jr. (University of Illinois) to inform him that Joseph J. Katz, J. Clark Hindman, and I are exploring the possibility of having a symposium on "The Chemistry of the Transuranium Elements" under the auspices of the Division of Physical and Inorganic Chemistry at the meeting of the American Chemical Society in Chicago (Spring, 1948). I explained that we had not contacted him formally before since we had to arrange a number of items about declassification, participants, etc. I said that I hope it will be possible to arrange for such a program.

I gave the Chem 123 lecture today at 10 a.m. I think the class was surprised to see me.

In the late afternoon I went to the joint meeting of our group and the Gilman Hall group, and in the evening I went to the Research Conference, both in Room 102, Gilman Hall.

Wednesday, October 22, 1947

A teletype arrived this morning from Winston Manning: "SIMPSON'S FORMER SECRETARY, MISS DORAL BUCHHOLZ NOW LEAVING CHICAGO. WILL ARRIVE IN BAY AREA IN ABOUT TWO WEEKS AND WILL GET IN TOUCH WITH YOU THEN. SHE WILL BE AT 2919 SCOTT ST., SAN FRANCICSO." Iz and I talked about this--we are anxious to find a new secretary, particularly because a secretary needs to have a "Q" clearance and that takes time. I then telephoned Oliver Simpson, who said Miss Buchholz is good on shorthand and typing, although not as aggressive as she might be. He also described her education and experience, both of which seem suitable for our position. I then wrote a note to Miss Buchholz at the San Francisco address to ask her to get in touch with us when she comes to town.

I also took care of some of my other correspondence. I replied to an October 8 letter from David Halliday (University of Pennsylvania), who is preparing a paper on the genetic relationships in the transuranium elements when viewed as members of the four radioactive series. Halliday asked a number of questions about heavy isotopes, whose answers have not yet been declassified; so I sent him the chart depicting all the information that has been declassified for the heavy isotopes. In addition, I referred him to Cork's recent book on Radioactivity and Nuclear Physics and Bethe's book on Elementary Nuclear Theory. I explained that, although I have not been able to answer all of his questions, I would be interested in seeing his write-up when he has completed it.

On October 13, O. H. Bullitt, Jr. (E. I. du Pont, Wilmington) wrote to invite me to speak at a seminar at their Experimental Station in May or June. In my reply today I said that, if I can arrange to speak while I am on one of my many trips East, I will be willing to do it since I would enjoy seeing my friends at Wilmington.

I dropped a note to Winston Manning in order to send him a biographical outline that I promised to send him for use in preparing a talk on my behalf at the Nichols Medal presentation.

I read a memo that arrived in today's mail from Kenneth Priestley, transmitting a letter from Robert L. Daerr about Edgar Westrum's consultant agreement. Daerr wants to be assured that Westrum has adequate safe filing for the protection of reports and other restricted data, which may come to him in the course of his duties as consultant.

Later I looked in on some of the experiments.

Thursday, October 23, 1947

I attended this morning's group meeting along with Asprey, Ballou, Barton, Chubbuck (another Army man who is working on a master's degree with us), Cunningham, Eyring, Garden, Glenn, Hicks, Hopkins, Howland, Huffman, James, La Chapelle, Lindner, Magnusson, Miller, Moore, Neumann, Newton, Perlman, Robinson, Street, Templeton, Thompson, Werner, Wilkinson (whose clearance has finally come through), and Wolfe. I gave a rather detailed account of the scientific items I learned in Chicago and at the Information Meeting and other meetings at Clinton.

Then Wilkinson discussed activities in platinum, iridium and gold formed with the 60-inch cyclotron; he described the bombardments, the chemistry, the isotopes, and the radiations he has noted. This is the first meeting that Wilkinson has attended; he has done a prodigious amount of work. He also talked about some work he has done by bombarding tantalum with 20 Mev deuterons; he has found a 140 ± 2 day tungsten isotope. He also found an ~135-day rhenium period in some old tungsten exit strips. Wilkinson said that the products from the bombardment of tantalum with 40 Mev deuterons are being investigated by David Karraker and Harold Lohr, two of the incoming graduate students who have elected to do their research with us and who are working in Gilman Hall, pending their clearances. Wilkinson, obviously, is a talented and hard-working scientist.

Templeton reported that Po^{208} has a half-life of 3.0 ± 0.2 years, assuming that the only alpha emitters present in the samples were Po^{208} and Po^{210}; he said that he could find no evidence for anything at mass 209.

James talked about his experiments using fluosilicate salts as eluting agent for rare earths and actinides from Nalcite resin columns. He described his curves and the mechanism of the elution. Since the americium and curium peaks are well separated and are separated from the element 61 peak, this may be a better method of separation than citrate elution and is definitely the best method of separating americium from element 61.

* * * * *

The Chemistry 123 class had a midterm today.

Back on the hill I received a telegram from Ruth Rogers, stating, "GREATLY APPRECIATE OFFER OF POSITION AND GENEROSITY OF TERMS. DECISION MADE WITH SOME REGRET TO STAY IN CHICAGO LETTER FOLLOWS." This was not an unexpected reply, judging from Ruth's reaction to my offer when I talked with her in Chicago.

Friday, October 24, 1947

I worked on my correspondence for a while. On October 1, W. H Sullivan wrote from Chalk River, giving some corrections to our "Table of Isotopes" (BC-59) and asking about a reference to a 15-year element 43 activity that he says appeared in my 1940 compilation. In my reply today I asked for more information about the 15-year activity for I have been unable to find any reference to it.

In answer to earlier queries, I wrote to Frederick T. Hobbs (AEC) to explain that the Eldorado Mining and Refining Company's interest in "Transmutation of Radium to Actinium (Ac^{227})" by S. Peterson is to help compare the economics of preparing actinium by this method with extraction from natural sources. I said that, although I doubt this report will help in that task, we are submitting the original paper for declassification since it seems declassifiable under the present guide.

A high school student named William Gladis wrote on October 3 to ask a number of questions about the transuranium elements and the 4n + 1 series. Today I mailed him a chart, explaining that this contains all the information so far released about the transuranium elements, and

referred him to the article in The Physical Review entitled "The 4n + 1 Radioactive Series: The Decay Products of U^{233}" by Seaborg, Hagemann, Katzin, Studier, and Ghiorso. In answer to his query about the German reference to element 118, I said that no elements with atomic number higher than 96 have yet been produced and identified.

Jim Crowe sent me a congratulatory note, dated October 21, about my selection as the Nichols Medal awardee for 1949.

I replied to an October 9 letter from A. C. English, saying that I have sent him a set of my reprints and will put him on our reprint list. I also mailed him glossy prints of the micro-equipment, which he requested. English also asked if, since Cork had mentioned Pa^{232} and U^{232}, I thought there was any likelihood of further release of information on these isotopes. To this I replied that it is likely that there will be a liberalization of the declassification code soon. Finally, I said that I hope to attend the Chicago ACS meeting next spring and talk with him then.

I also answered the queries of a prospective graduate student, Jack M. Hollander, now a student at Ohio state. I explained that first he needs to be admitted to the graduate school here and therefore I have sent his transcript to Dean Wendell M. Latimer of the College of Chemistry. Then I told Hollander about the setup we have here for graduate students--clearance and half-time positions during the school year.

Saturday, October 25, 1947

I worked in my office for a while, and then Helen and I went to Memorial Stadium to watch California lose its first game this season to Southern California. The score was 39 to 14. Jeanette stayed home to take care of Pete and Lynne. [Both Pete and Lynne seem to be progressing beautifully, in my unbiased view.]

Sunday, October 26, 1947

Some of my morning was spent outlining and working on "The Eight New Synthetic Elements" for the Sigma Xi lecture in December. I then went over to Kezar Stadium to watch the Cleveland Browns defeat the 49ers (coached by Lawrence T. Shaw) by a score of 14 to 7. We saw the Browns' Otto Graham pass for 278 yards, including two touchdown passes to Mac Speedie and Dante Lavelli. Frankie Albert went only 7 for 15, passing for 85 yards.

Monday, October 27, 1947

I dictated a number of letters when I arrived at the office this morning. I wrote to Charlie Price at Notre Dame University to ask how many talks I should plan on giving in the Nieuwland Lecture Series, the approximate time for the individual lectures, and what type of audience will be in attendance. I told Price that I would like a blackboard and an ordinary (3-1/4" x 4-1/4") slide projector. Finally, I said that, since I had read that he was spending some time in England, I am sending a carbon of my letter to Milton Burton.

Using the material Saul Winstein sent me and the information I received

from Melvin Calvin and others, I wrote a long letter to Alden Emery, supporting the nomination of Saul for the 1948 Award in Pure Chemistry. Among other things I said, "I would also like to mention that in the course of my discussions of Winstein's work with his colleagues I have had occasion to learn the opinion of his work which is held by practically every one of the leading physical organic chemists in this country. I have been struck by the unanimity of his contemporaries in the same field in their high regard for his work, and this is as fine a recommendation as can be tendered any scientist." I sent eight copies of my letter to William Young at UCLA for transmittal to Alden Emery.

I answered an October 8 letter from Donald McPherson (John Wiley) who, among other items, asked for my comments on the outline of a manuscript by Herman Yagoda of the National Institute for Health on "Nuclear Emulsions and Their Applications in Radioactivity." Since I do not know of Yagoda's work first hand, I talked with Eugene Gardner and Amos Newton and then passed on their rather negative comments to McPherson, suggesting that he ask for further opinions.

Allan Kastrup of the American-Swedish News Exchange asked, in a letter dated October 21, for a biographical sketch with all the main facts about my descent, career, etc., for possible use in a magazine article and also for reference use. Today I sent him a short biography written by Helen, an outline of biographical data on my professional career, and a copy of the letter I wrote to Miss Carlson of the American Swedish Monthly, in which my Swedish ancestry is described.

In response to an October 23 request from Mrs. Florence Birkhead of Mills College, I mailed her a photograph, a biographical sketch, and an abstract of my upcoming (November 11) talk, "Peacetime Applications of Atomic Energy."

I also wandered through the labs to look in on the research and to talk with the fellows.

Tuesday, October 28, 1947

I looked over my notes in my campus office and then gave the Chem 123 lecture at 10 a.m. Back on the hill I answered an October 23 letter from Bob Gould, who asked whether he should put a note in Chemical and Engineering News about Macklin and Knight's work, reported in the September 1 issue of The Physical Review, on the x-rays associated with U^{234}. I told Gould that I believe they are wrong in their interpretation and that I would rather explain the x-rays as due to the rather orthodox and well-known internal conversion process of a nuclear gamma ray. Therefore, I said, I have my doubts whether he would want to run a feature article.

I made a tour of the labs to talk with some of the students. Every time I go through the building, I am increasing aware of the crowded conditions.

At 4:15 p.m. I went to the meeting of the Seaborg-Connick groups, and in the evening I attended the Research Conference.

Wednesday, October 29, 1947

I wrote a note to Ed Westrum to inform him that we have suggested that he be made a responsible reviewer for Volume 14 of the Plutonium Project Record and to say that we hope he will accept such an appointment. I mentioned that the work in his department here is going rather slow.

Several hours today was taken up drafting my Sigma Xi talk. I then checked on the research of the men.

Thursday, October 30, 1947

Present at this morning's group meeting were Ballou, Barton, Chubbuck, Conway, Covey, Cunningham, Eyring, Garden, Ghiorso, Glenn, Goeckermann, Hicks, Hopkins, Howland, Huffman, James, King, La Chapelle, Lindner, Neumann, Perlman, Robinson, Seaborg, Templeton, and Wolfe.

Hopkins discussed his most recent bombardment of arsenic with 200 Mev deuterons. He found a new 36-hour chromium with Miller's help. In zinc he saw Zn^{69}--a 57-minute β^--emitter and the 14-hour I.T. parent of the 57-minute β^--emitter. In nickel he found the 2.6-hour Ni^{65} and the 53-hour Ni^{66}.

Conway reported that Beaufait's original hafnium sample was only 92% hafnium (impurities are barium, strontium, copper, and 2% zirconium); he has analyzed platinum discs and some special purity platinum--Hilger platinum is the best and contains 0.8 ppm gold and rhodium. Conway, in response to my query, said that he is going to start classifying the lines of the actinides.

Hicks described some of his work connected with the search for heavy rare earth fission products in slow neutron fission of americium. In his calibration column experiment with 1.5 mg of each rare earth, lutetium through europium--Nalcite with citrate elution--he obtained a small amount of pure lutetium, but the elution curve was poor--the europium, gadolinium, and samarium seemed to bunch together.

Robinson reported some rather unsuccessful attempts to use the Cauchois x-ray spectrograph to detect K and L x-rays of lead and in a 6-day bismuth sample formed by deuterons on lead. The films were badly fogged. Barton said that he was confident that in the 4-day exposure he had observed L_α and L_β lines of lead, but Robinson said the lines were very weak.

Lindner discussed some of his results of the long bombardment of antimony with 200 Mev deuterons, most of which were reported previously. Te^{118} has been determined to be a 6.0-day K electron capture isotope. He has found an ~4-day tellurium decaying by K? capture to an antimony of 38 to 40-hour half-life (K capture); the antimony may be Sb^{117}.

Ghiorso mentioned that, within a factor of two, Cm^{242} is responsible for the high spontaneous fission rate found in the various curium samples; his counting of the pure Cm^{242} sample from Clinton Laboratories enabled him to establish this.

Ballou reported on methods of preparation of tantalum carrier. He also talked about some isotopes he has been considering that might be alpha unstable, such as Zr^{88}, Ti^{44}, and Nd^{140}. Bombardments of scandium and yttrium with deuterons to look for Ti^{44} and Zr^{88} alpha particles have led to negative results.

At this point Perlman announced that a new list of stable isotopes from Oak Ridge is now available and that Joe Hamilton has 1 mc of Ac^{227} in case anyone needs actinium tracer.

Templeton reported that the crystal structure of SmOCl prepared by Asprey is tetragonal with lattice constants: $a = 3.973 \pm 0.005$ and $c = 6.692 \pm 0.010$.

Finally, James talked about the carrying of americium and 61 on LaF_3 with variation in the amount of LaF_3 precipitated; 90% of 61 is precipitated when only 30% of the americium is precipitated.

* * * * *

At 10 a.m. I gave the Chemistry 123 lecture and checked in with Miss Kittredge before going up to the hill.

My sister Jeanette has been having trouble with sore throats and her doctor suggested a tonsillectomy. Today was the day! Jeanette expected this to be an uneventful, but painful, operation; however, when I checked at Alta Bates Hospital in the afternoon, I learned she is having some difficulties. Later, while her friend Eino Karpinen was visiting her during the evening, she suffered from some postoperative bleeding.

Friday, October 31, 1947

Before going to the office this morning, I checked on Jeanette, and learned that the doctor feels everything is under control. I drove her home to recuperate.

A long letter arrived from Ruth Rogers, explaining in more detail why she refused my offer to work for us in Berkeley. Ruth said that she has embarked on a small, personal training program, which she hates to give up now. In addition, she said that after reading Time a couple of weeks ago, "that the U. of C. is almost too impressive, and star-studded for plain, ordinary mortals. At any rate, I don't feel ready for it yet." Ruth again recommended Doral Buchholz--if she decides to continue as a secretary.

As usual I talked with some of the fellows about their work and then did some report reading.

Saturday, November 1, 1947

A draft of my Sigma Xi talk entitled "The Eight New Synthetic Elements" is being typed, and this morning I looked over part of it. In addition, I selected slides to be used for my Nieuwland Lectures.

Later I talked with some of the graduate students about their research.

During the afternoon I listened to the radio broadcast from Los Angeles of the UCLA-California game. Cal beat UCLA by a score of 6 to 0.

Sunday, November 2, 1947

Again I worked on my Sigma Xi talk--this will not be given until the end of December, but naturally it has to be completed soon so that it can be declassified. I also intend to send copies of it to a number of people in order to get their comments and their help with the references.

I spent some time with Peter--he isn't getting quite as much attention as his sister these days although he doesn't seem bothered by this.

Monday, November 3, 1947

At the office this morning I gave Mary some more instructions about the manuscript for my Sigma Xi talk.

In today's mail I received a letter from Charlie Price, who said he has made a reservation for me to stay on the Notre Dame campus November 16 to 20 although he can change this to a downtown hotel room, if I wish. He has scheduled my talks for Monday, Tuesday, and Wednesday evenings, and he will meet me if I let him know when I plan to arrive.

I also read a letter from W. H Sullivan, who said that he found the 15-year technetium isotope on the photostated copy of a 1942 chart that I distributed on the project in the latter part of 1942. Sullivan mentioned that he plans to visit the Bay Area from November 11 through 16 and will try to visit the lab. He also said that he is trying to poll the members of the Subcommittee on Nuclear Constants on Ken Bainbridge's suggestion that Professor A. O. Nier be invited to join the Subcommittee.

A note arrived from Edgar Westrum, thanking me for the slides I lent him and saying that the semester is well underway. Westrum also mentioned that he is giving a graduate course in Statistical Mechanics at Wayne University.

I made my usual rounds of the labs to talk with the students and then read reports.

Tuesday, November 4, 1947

I looked over the notes and then gave the Chemistry 123 lecture this morning. Back on the hill I found that I had considerable correspondence to handle. I wrote to Mlle. Perey in Paris:

I am writing to inquire as to the symbol which you have finally chosen to apply to element 87, francium. It was given in your thesis as Fa, but I gather from Paneth's article in Nature that you later decided upon Fr. It seems to me that the final word on this should rest with you and I would like very much to know your decision in order that I might use the proper symbol.

I would like to congratulate you on your fine work on this element.

I answered an October 28 letter from J. W. Buchta (Reviews of Modern Physics), who sent me a sample page of the type of material proposed by Dr. Katharine Way ("Recent Nuclear Data") for an article in Reviews of Modern Physics and asked for my opinion. I wrote that I believe it is worth publishing since it is a different type of publication than the "Table of Isotopes." I added that Iz Perlman and I hope to have our up-to-date "Table of Isotopes" declassified and ready for publication by next spring.

In response to an October 28 letter from Joseph E. Mayer (Editor, The Journal of Chemical Physics), who asked if I would accept an appointment to the list of Associate Editors of the journal if it were confirmed by the Executive Committee of the American Institute of Physics, I wrote that I would like to do this, but I am worried that I won't have much time to devote to it.

In reply to an invitation, dated October 27, from Professor Allan C. G. Mitchell (Department of Physics, Indiana University), who asked if I would be able to speak in Bloomington during my trip to Notre Dame on November 17. I explained that I cannot do it since the Nieuwland Lectures extend for the whole week and I then must go to Washington for a meeting of the General Advisory Committee, after which I am flying directly home because of my teaching duties.

Pacific Telephone and Telegraph Company sent me a form, which I filled out today, justifying my need for a telephone--telephones are still in short supply. I pointed out that I am a member of President Truman's nine-man General Advisory Committee to the Atomic Energy Commission and need to be able to be reached at any time from Washington, D. C. In addition, I need to be able to be reached by members of my research group at the laboratory.

In the late afternoon I went to the meeting of the joint Connick-Seaborg groups, and after dinner I attended the regular Research Conference meeting in Gilman Hall.

Wednesday, November 5, 1947

In today's mail an October 31 letter arrived from Donald McPherson (John Wiley), thanking me for my comments, which Amos Newton and Eugene Gardner helped me prepare, on Yagoda's proposed book on Nuclear Emulsions. McPherson enclosed a check for $25 for my efforts and said that next time he requests advice, he will arrange the fee in advance. He also said that, at Dr. L. F. Curtiss' suggestion, they are going to submit a general proposal about publishing the book of tables for nuclear

science to the Committee on Nuclear Science for consideration at the meeting on November 10.

I read a November 3 letter from Edgar Westrum, who reported that Charles E. Keller from Oak Ridge will interview him in Ann Arbor on November 10 about the matter of becoming a responsible reviewer for Volumes 14A and 14B of the PPR. Westrum added that it appears feasible for him to make a visit to Berkeley about the first of the year.

A note arrived from W. H Sullivan, dated November 3, saying that he has talked with Nate Ballou about a position at the Naval Radiation Laboratory. Sullivan emphasized that he will make no formal offer until permission has been granted by Dr. Lawrence and me. He again mentioned that he will be in the area from November 11 to November 16, and he would like to discuss this problem then.

I also worked on my Nieuwland Lectures, dividing my material into three logical sections to accommodate the three lectures I am to give.

Thursday, November 6, 1947

Present at this morning's group meeting were Asprey, Ballou, Barton, Covey, Chubbuck, Cunningham, Fries, Garden, Ghiorso, Glenn, Goeckermann Hicks, Hopkins, Howland, Huffman, James, La Chapelle, Lindner, Miller, Moore, Neumann, Newton, Robinson, Seaborg, Street, Templeton, Thompson, Werner, Wilkinson, and Wolfe.

Ghiorso described determinations of fissionability that he and Cunningham plan to do early in December with the Argonne heavy water pile: confirm fissionability of Am^{242}, look for fissionability of Cm^{243}, look for Pu^{239} as the daughter of Cm^{243}, look for fissionability of Am^{240}, look for fissionability of Cm^{240} and/or Cm^{241}, check fissionability of Pu^{236}, and examine the transcurium fractions of the last Hanford bombardment of Am^{241} (51NB). Ghiorso said that they probably will have as much pile time as is necessary, and that Cunningham will do the purifications as needed.

Asprey reported that he tried to make Am_2O_3 by heating AmO_2 to about 1425°C for nine minutes. There appeared to be no change, but Templeton will examine the sample. The sesquioxide may have formed.

Templeton said that he has made x-ray diffraction measurements on a sample of $SmSO_4$ on magnesium but could find nothing but magnesium. A sample of $SmCl_2$ on magnesium gave magnesium lines and eight other lines, as yet unindexed.

Hopkins discussed the yields of isotopes from deuteron bombardments of arsenic. All arsenic isotopes have about the same yield. He assigned a value of 1.0 to arsenic isotopes for comparative purposes; on this basis selenium isotopes of the same mass numbers have a yield of 0.02; yields of isotopes from arsenic to copper are about 0.1 to 1.0; nickel, cobalt, and iron yields are 0.005. These figures fit with the idea that 100 Mev neutrons are causing the reactions. The break in yield comes at nickel or cobalt, which are formed by knocking out 12 nucleons. At this point Newton said that the yield distribution can also be explained by

inelastic scattering of deuterons and that Thornton says that the yield of neutrons from the deuterons is only about 2%. Templeton mentioned that the yield distribution could be explained by saying that the primary products are formed around copper and nickel and that weaker secondary particles are in large abundance and give high yield in the neighborhood of arsenic. I suggested that Hopkins calculate the cross section for the neutron reaction--if it is of the order of 100 barns, the neutron reaction mechanism is impossible. Hopkins said that around copper, the positron-emitting isotopes predominate while in the iron region, negative beta particle-emitting isotopes are comparable in yield with positron isotopes.

Lindner talked about the yields from 200 Mev deuterons on antimony. He found a drop in yield at the same relative position as Hopkins got with arsenic but said there is not the corresponding small yield of tellurium isotopes ($\Delta Z = 1$ isotopes). He compared the isobars--Cd^{109}-Pd^{109}: the yield of palladium is about a factor of 100 less than that of cadmium; In^{112}-Ag^{112}-Pd^{112}, the yields of indium and silver are about equal, but palladium is way down. For the few cases he has observed, the negative beta particle-emitting isotopes seem to have the lower yield.

Miller than talked about the yields from 200 Mev deuterons on copper--there seems to be a relatively smooth drop in yield as we go down from copper but no sharp drop as with arsenic and antimony. Similarly to arsenic, he seems to get comparable yields of β^+ and β^--emitting isotopes of elements near the target nucleus and higher yield of β^- isotopes of elements far from the target area.

Huffman reported that they have successfully adsorbed zirconium and hafnium on IR-4 resin from 0.01 M zirconium, 0.01 M hafnium, and 0.3 M HF solution. So far they have had no measurable fractionation of zirconium and hafnium when eluting. They will now try column elution.

Street said he has made a series of 10 cm column runs with citrate in order to look for a rapid method of separating americium from curium; he varied pH and flow rates and found the width of the peak is very sensitive to flow rate. The slower flow rates seem the most promising.

* * * * *

After the meeting I gave the Chem 123 lecture. Then back up on the hill, after stopping in the Department office to talk with some of my colleagues, I looked over today's mail.

Mrs. Ruth B. Conroy (Secretary, Division of Chemistry and Chemical Technology, National Research Council) sent me a carbon of a letter addressed to Dr. L. F. Curtiss, in which it was confirmed that the Executive Board of the National Research Council has approved the appointment of the Committee on Standards and Units of Radioactivity for the academic year 1947-48, with the following personnel: Dr. L. F. Curtiss (Chairman), Dr. Robley D. Evans, Dr. Warren C. Johnson, and Dr. Glenn T. Seaborg. She stated that this is a joint committee with the Division of Mathematical and Physical Sciences.

Sometime ago I turned down an invitation to speak at the Southern California Section of the ACS because of my extensive travel schedule. I suggested Iz Perlman as an alternate; he was invited and accepted the invitation to speak tomorrow evening. Unfortunately, Iz is quite ill, and he asked me to go down and read his talk. Iz has been so cooperative with and helpful to me that I naturally agreed. So, in the evening I took the overnight "Lark" from Oakland to Los Angeles.

Friday, November 7, 1947

When I arrived in Los Angeles, I went to South Gate and spent the morning with my parents. They were very pleased to see me since it has been several months since I visited them. Later I was picked up and driven to the USC campus to give an informal seminar on the transuranium elements; this was arranged by Wayne Wilmarth, a former graduate student of Melvin Calvin who has joined the Chemistry Department faculty there.

I attended the dinner of the Southern California Section of the American Chemical Society; this preceded my talk that I called "Nuclear Reactions with Ultra High Energy Particles" and was given at the Rodger Young Auditorium (936 West Washington Blvd., Los Angeles); this was a substitute for Iz Perlman's lecture. I began my talk with an historical description of nuclear reactions, then described the recent advent of the 184-inch cyclotron at Berkeley and its characteristics, the plans for an accelerator in the billion electron volt range at Berkeley, and finally the results we have been obtaining in our experiments at the 184-inch cyclotron. The reporters who were present seemed fascinated by my remarks about the possible creation of matter from the energy of the billion electron volt particles that will be produced in the new proposed accelerator.

I saw many of my UCLA friends during the evening; I also saw my friend L. Reed Brantley of Occidental College, who originally invited me to speak to the Section. Afterwards I returned to my parents' home to spend the night.

Saturday, November 8, 1947

I caught a morning plane to return home (via San Francisco). Helen and I then were able to take in the University of California-University of Washington football game in Memorial Stadium--Jeanette took care of the children. California was the winner by a score of 13 to 7. "Pappy" Waldorf has certainly transformed our football team.

Sunday, November 9, 1947

Today I worked for a while on my Sigma Xi talk and then went over my notes for the talk I am scheduled to give on Tuesday at Mills College.

Herman Robinson came by in the afternoon and took some pictures of the children, one of which we plan to include with our Christmas cards this year.

Lynne and Peter Seaborg, 2808 Ellsworth Street, Berkeley, California
November 9, 1947

Monday, November 10, 1947

After some calls and administrative matters, I looked over my correspondence and found another letter from Charlie Price--our last letters crossed in the mails. Today he answered my questions about the audience for the Nieuwland Lectures: the audience of 200 to 300 will consist of all types--staff and graduate students through undergraduates, non-scientific staff, and visitors. He suggested that I aim the talks at something between the technical and semipopular level. He again said that we can drive to LaPaz (16 miles) late Thursday afternoon to catch the "Capitol Limited" to Washington if I wish, but they would be delighted to have me stay over to Saturday to take in the Notre Dame-Tulane football game.

In a November 7 letter, Milton Burton reiterated that he and his wife Sarah want to entertain me when I am in Notre Dame; he added that the department is having a meeting to discuss, among other things, social arrangements for my visit.

A November 3 letter arrived from O. H. Bullitt of the E. I. du Pont Experimental Station in Wilmington. Bullitt is pleased that I may be able to visit and give a talk to them and asked that I let them know by January or February. He then said that any time in May or June will be suitable although they prefer a day other than Friday.

I read a letter from Alden H. Emery, who said that he had written Dr. Lamb and asked if he would object to withholding publication for one year of papers presented before a symposium on the chemistry of the transuranium elements--a matter I discussed with Emery when I had lunch with him last month. Unfortunately, Emery said, he has learned that Dr. Lamb is ill and the issue cannot be decided until he is well enough to resume his routine.

I then dictated a number of short notes. I sent Dudley E. Chambers (General Electric, Schenectady) the signed copies of the extension of my consultation contract that he mailed me on October 31. I mentioned that my time for my October trip to Schenectady is three days--October 6, 7, and 8--including traveling time.

In a note to Jack Hollander (Ohio State) I said that I have learned that Dean Latimer accepted him as a graduate student and that George Everson (Personnel Director of the Radiation Laboratory) will send him a personnel security questionnaire. I explained that the clearance may take two or three months but should be completed by early spring in case he wants to come then.

I wrote to Jack East (Clinton Laboratories) to inform him that I have learned from Al Ghiorso and Herman Robinson that some of the men here have been working on a circuit similar to his new pulse analyzer so we shall not request one of his. However, we are anxious for further information on the type of ionization chamber and the type of gas purification he uses.

To Charlie Price I said that I plan to arrive in South Bend on Monday, November 17, at 12:56 p.m. on NYC Train No. 6, but if I miss this connection I shall come on the earliest following train, which might possibly be the Chicago, South Shore, and South Bend Railway. Unfortunately, I wrote, I have to be in Washington on Friday and will not have a chance to see the great Notre Dame team play.

I submitted my Sigma Xi talk, "The Eight New Synthetic Elements" (UCRL-9), for declassification and, in the covering memo, I said that it is declassifiable under the present Declassification Guide and doesn't require the more liberal declassification rules of the impending new Guide (which would delay its declassification).

Tuesday, November 11, 1947

Iz Perlman gave the Chem 123 lecture this morning while Helen and I went to the Mills College campus, first to the office of President Lynn Townsend White and then to Lisser Hall, where I spoke on "Peacetime Applications of Atomic Energy" at an 11 a.m. assembly. President White introduced me. I began by saying,

> There are two important aspects of the peacetime applications of atomic energy--first, its application to industrial power uses and second, the use of the by-product radioactive material for important research in medicine, biology, and other scientific fields.

I then described the potential use of plutonium, uranium-235, and uranium-233 as fuel in atomic energy machines--first stationary structures to produce electricity, possibly later for the propulsion of boats or even large airplanes but never for the propulsion of automobiles. The rest of the talk was a description of the future prospects for the use of radioactive isotopes in medical therapy and diagnosis, biochemical and medical research, as well as many other areas of scientific and industrial research. I said that I believe that the greatest benefits to humanity in the long run will come from the use of

these radioactive materials.

After the talk Helen and I had lunch with President White, faculty members (including Chemistry Professor Richard Wistar and his tall, stately, and attractive wife), and student leaders. After lunch Helen and I returned to Berkeley and I went to the laboratory.

Wednesday, November 12, 1947

After going to First Aid for routine blood and urine tests, I dictated a long letter, addressed to Dean Latimer, in support of the promotion of Robert E. Connick to the tenured rank of Associate Professor. I described Connick's abilities and achievements and concluded with:

> I believe that Dr. Connick should be rated as one of the two or three outstanding inorganic chemists in this country and in his own age group I would rate him as very probably the number one man in this field. It is fortunate that a man of sufficient calibre is available to carry on and extend the field of chemical kinetics at the University of California since not a small amount of the prestige enjoyed by our Department of Chemistry stems from the work of the late Professor Bray in this field. I can recommend Dr. Connick without reservation and I believe that he is a great asset to the chemistry staff of the University of California.

A draft copy of my Sigma Xi talk, "The Eight New Synthetic Elements," was mailed to George E. Boyd in Oak Ridge, asking that he look it over. I explained that I want to do justice to his work on technetium and element 61, but only with his full approval and with proper referencing. I also sent a copy to George W. Parker (Oak Ridge), again asking for proper references to his work. To Parker I mentioned that we have gone ahead with arrangements for his visit to Berkeley and hope the arrangements will be satisfactory.

I sent Joe Katz some Berkeley titles for the April ACS symposium on the chemistry of the transuranium elements: "Preparation and Properties of the Higher Chlorides of Uranium" by C. H. Prescott, Jr.; "The Oxidation Potential of the Uranium Five-Six Couple and the Kinetics of the Disproportionation of Uranium Five in Perchloric Acid Solution" by D. E. Kern and E. F. Orlemann; "The Isolation and Properties of Curium" by I. Perlman and L. B. Werner; "The Isolation and Properties of Americium" by B. B. Cunningham; and "The Tracer Chemistry of Americium and Curium" by S. G. Thompson, R. A. James and L. O. Morgan. I mentioned that I'll be in Chicago between 10:45 and 11:30 a.m. on November 17 (between trains) and maybe we can get together to look over the list, or, perhaps, he might want to phone me here in Berkeley.

This afternoon Helen went to Mrs. Latimer's home for tea with the Chemistry Department wives.

Thursday, November 13, 1947

This morning's group meeting was attended by Asprey, Ballou, Barton, Conway, Covey, Cunningham, Eyring, Garden, Ghiorso, Glenn, Hicks,

Hopkins, Howland, Huffman, James, King, La Chapelle, Lindner, Magnusson, Miller, Neumann, Newton, Perlman, Robinson, Seaborg, Stewart, Street, Templeton, Thompson, Werner, and Wolfe. I announced that W. M. Manning will be here next Tuesday to talk with students interested in positions at Argonne National Laboratory after graduation, and Perlman added that W. H Sullivan will be here this afternoon.

La Chapelle reported that new experiments performed to produce a chloride of Np(V) resulted in a product with a Cl/Np ratio of 1.4 to 1.5. The product of a different procedure has yet to be analyzed. Nor, he said, has he gotten x-ray data on the bright yellow and crystalline "NpO_2Cl" prepared by the evaporation technique.

Eyring said that he has prepared $NpCl_4$ from NpO_2 by treatment with $AlCl_3$. Templeton said the structure is tetragonal like UCl_4 with very slight difference in cell size. Templeton also said that he has not examined the residue from Eyring's sublimate, but he has looked at some of Eyring's Eu_2O_3 (the pattern has not been indexed). The Eu_2O_3 changes into the cubic Y_2O_3 structure on ignition. I asked if this type of structure change could account for the americium oxide of cubic structure. Cunningham replied that it couldn't for the cubic americium oxide has the fluorite structure while the Eu_2O_3 changes from hexagonal to cubic (not the fluorite structure). Cunningham feels that the black AmO_2 has some americium in it of lower oxidation state. In reply to a question by Asprey, Templeton said a mixture of AmO_2 and Am_2O_3 would give an x-ray pattern with both sets of lines superimposed.

I suggested that the preparation of americium metal should be done next, but Eyring explained that they have not received the single crystals of beryllia for the needed crucibles.

Hicks talked about a Nalcite column separation of macroamounts of rare earths plus yttrium, europium, and 61 tracers. All of the rare earths were completely separated except europium and gadolinium. Ytterbium, thulium, erbium, holmium, and dysprosium were separated with no detectable impurity. Lutetium had 0.2% impurity; the yttrium tracer came out over the same sample range as europium, gadolinium, and part of samarium. There was no analysis for terbium. Yttrium was found in the spectrographic analyses. Hicks also said some radioactivity eluted late that has not been identified. Conway explained that about 0.2% ytterbium was found in the lutetium, but there may have been slightly more present. Hicks then said they plan to use this type of separation of rare earths in a bombardment of tantalum with 200 Mev deuterons next Monday, assigning peaks at the various sample numbers and not adding much carrier.

Asprey announced that he plans to try to make Am_2O_3 by dissociation of AmO_2 via heating.

Hopkins then reported that the cross section for the formation of As^{72} from As^{75} by 200 Mev deuteron bombardment is about 0.02 barn, which seems low. He said that if one considers the effective beam to be only the stripped neutrons, the true cross section would be about 10 barns. Cunningham said that the theoretical physicists still have no explanation for 50-fold lower yield of Se^{72} compared with As^{72}. I suggested a

chemical loss, but Lindner said only a small difference in the yields of antimony and tellurium in antimony bombardments was found. Miller said he found a yield of Cu^{62} that was 70 fold higher than Zn^{62} in the copper bombardment, in agreement with the As^{72}-Se^{72} results.

James said that they received part of Joe Hamilton's long bombardment of U^{235} with 20 Mev deuterons and purified a considerable amount of Np^{235} (400 day, K capture). Pulse analysis of a carefully purified neptunium fraction showed alpha particles of 5.0 Mev, probably due to alpha branching of the Np^{235}; the yield corresponds to a half-life for alpha emission of 1000 years. The possibility that the alpha particles are due to Np^{236} was ruled out by milking protactinium (to look for Pa^{232}) from the neptunium: only 100 c/m of protactinium that did not decay were found. I said we need to know the alpha half-lives of more odd Z elements in order to make predictions for element 97--all we have up to now is Am^{241} (500 y) and Am^{239} (1.5 y). I also said that someone should work on Pa^{229}. James suggested that perhaps their K x-ray counting efficiency assumptions may be way off and that L capture may be important. Ghiorso mentioned that Simpson predicted several hundred thousand years for Np^{235}. He then suggested that someone use higher energy deuterons on Th^{232} to get a higher relative yield of Pa^{229} compared with Pa^{232}; I pointed out that higher energies give a still more complex mixture of products, but Howland pointed out that Ghiorso meant only about 50 Mev, which would give a good d,5n yield compared with d,2n.

* * * * *

After giving the Chem 123 lecture, I returned to the hill and dictated a very positive letter addressed to Professor Latimer for Edwin F. Orlemann to support his promotion to the tenured position of Associate Professor. I concluded by saying,

> He is probably one of the two or three outstanding analytical chemists in this country and, in my own opinion, it would be correct to rate him as the number one man in his age group in this field. I can recommend him very highly and believe that he is a great asset to the staff of the University of California.
>
> In conclusion, I should like to mention that the Chairman of the Department of Chemistry of the University of Chicago has told me that they are interested in Dr. Orlemann to fill their great need for someone to take charge of their analytical chemistry program; they regard Orlemann as their first choice and probably intend to approach him. The shortage of competent chemists is probably felt more keenly in this field than in any other, leading to a situation where a large number of leading universities are forced to bid for the few good men in the field.

I received and read an October 8 letter from Erich Leonhardt, a former East German now living in Bonn, who asked that I arrange for him to get a CARE package. Leonhardt explained that he had spent time in England as a young man.

A November 5 congratulatory note arrived from Colonel Marston Taylor Bogert (Beilstein-Gmelin Committee) because of the announcement that I am

being awarded the Nichols Medal for 1948. Congratulations also arrived from W. A. Hamor (Mellon Institute of Industrial Research, University of Pittsburgh) and Sid Fox (an old friend from UCLA who is now at Iowa State College). Fox commented that, in the past few years, he has pointed out that Professor Ramsey at UCLA was so tough that only a Seaborg could hope to get an A in Ramsey's course.

A memo arrived from John H. Manley (Secretary of the GAC) about a number of items concerning the November 21 GAC meeting: 1. Downgrading of certain subject matter has not been considered by the AEC; therefore the Committee may want to postpone discussion. 2. There will be a meeting with the Advisory Committee for Exploration and Mining at 2 p.m. on November 21. 3. The Committee will be supplied with background information on the matter of Sandia although the paper from the Military Liaison Committee (MLC) about specific topics has not been received. 4. It is expected that the two drafting teams for the Power Statement will have their drafts prepared for the meeting. 5. There will undoubtedly be an examination of the Los Alamos plans for the Pacific Proving Ground Operation.

Friday, November 14, 1947

Since I shall be leaving town tomorrow evening, I tried to take care of all my pending correspondence the first thing this morning. I mailed a copy of the paper entitled "Search for Elements 94 and 93 in Nature. Presence of 94^{239} in Pitchblende" to Morris L. Perlman (General Electric, Schenectady) and a copy of "Search for Elements 94 and 93 in Nature. Presence of 94^{239} in Carnotite" to Clifford S. Garner (UCLA), in order to obtain their comments and suggestions for changes. Since these are pre-war reports, I suggested to both men that the changes not be extensive--we will have typed them in a manner suitable for publication in (JACS) Journal of the American Chemical Society.

In a similar vein I sent a copy of "The Chemical Properties of Elements 94 and 93" and "Nuclear Properties of 93^{237}" to Arthur C. Wahl (Washington University, St. Louis) for his comments. These reports, I said, have been edited somewhat by the editors of the National Nuclear Energy Series in order to make them correspond to journal standards. I noted that the first should probably be submitted to Journal of the American Chemical Society and the second to The Physical Review. I also mentioned that the first paper was altered, leaving out the detailed discussion of procedures, in order to make it more declassifiable and more suitable in length. Finally, I included a copy of the paper, "Search for Elements 94 and 93 in Nature. Presence of 94^{239} in Carnotite," of which Norman Bonner is a co-author and asked Wahl to give it to him for his comments.

On November 4 Harry Althouse mailed me a collection of papers called "Ausgewahlte Kapitel aus der Allgemeinen Biochemie" and asked for my comments. Today I wrote Althouse and said that I passed the material on to Professor Melvin Calvin, who is better versed in biochemistry than I. I told him that Calvin said that he thinks this is very sound and interesting work in elementary biochemistry and should serve a useful purpose.

Congratulations for the Nichols Medal award arrived from Frederic D. Rossini (National Bureau of Standards).

A letter arrived from David Halliday, thanking me for the copy of the isotope chart and enclosing a copy of his manuscript, written for teachers of physics, for my comments. I looked over the write-up and then sent him a few comments. I said, for example, that we refer to the radioactive isotopes outside the main line of radioactive decay as "collateral" members rather than as "tributaries" but this is only a matter of preference. I also questioned the accuracy of his information on some of his astatine isotopes.

In a letter I received today Edward L. Gordy (Editor, Chemical Bulletin, a publication of the Chicago Section of the ACS) informed me that, as a result of the reader poll, I have been designated one of the "ten ablest chemists or chemical engineers" working in the field of nucleonics.

At 3 p.m. I, along with Leo Brewer, A. J. Carlson, Jim Cason, and Harvey E. White, took part in the prelim examination of Raymond Sheline, Kenneth Pitzer's graduate student. He presented as his propositions to be defended, "A Proposal for Detecting the Neutrino," "The Structure of Streptomycin," and "A Scanning Geiger-Müller Counter." We asked him to speak on the latter; Sheline did very well and readily passed the examination.

Saturday, November 15, 1947

This morning I talked with various men about their plans for research for the next couple of weeks--while I am out of town--and after lunch I went to Memorial Stadium to watch about half of the UC-University of Montana football game. UC was winning easily [the final score was 60-14] when I left in order to spend a little time with Peter, Lynne, and Helen before catching the 5:45 p.m. train--"City of San Francisco"--bound for Chicago.

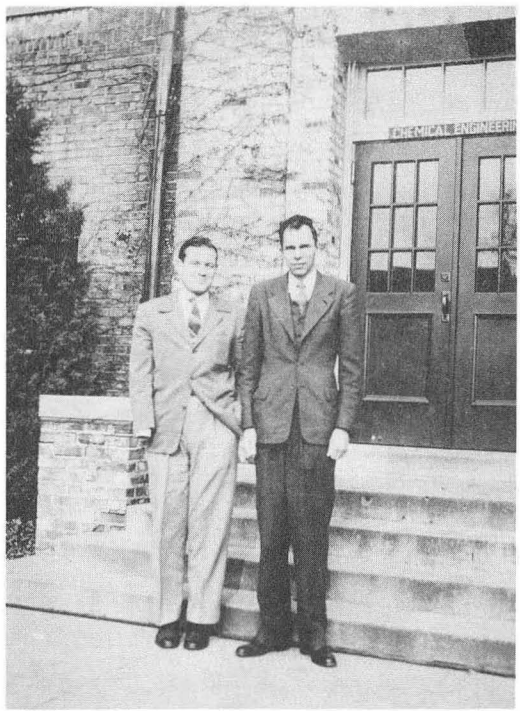
Sunday, November 16, 1947

En route to Chicago. Much of my day was spent going over my notes for the three Nieuwland Lectures, scheduled for Monday, Tuesday and Wednesday.

Monday, November 17, 1947

I arrived in Chicago about 10:45 a.m. and telephoned Joe Katz to go over some of the details of the symposium on the chemistry of the transuranium elements that we are planning for next spring's ACS meeting in Chicago. At 11:30 a.m. I took the NYC Train No. 6 to South Bend, where I was met by Charles C. Price and driven to the campus of the University of Notre Dame.

I am staying on campus during this visit. A group from the Department of Chemistry entertained me at dinner.



Charles Price and Glenn Seaborg, Notre Dame, November 1947

Tonight's 8 p.m. talk in the series was entitled "The Transuranium Elements and Nuclear Energy" and covered first the historical background of the discovery of the chemical elements, the discovery of natural radioactivity, nuclear transmutations, the neutron, artificial radioactivity, nuclear fission, and the discovery of neptunium and plutonium. I then talked about the large-scale production of plutonium on the wartime Plutonium Project through a chain reaction in a pile and the development of the process for the chemical separation of plutonium from uranium and fission products, including a description of the ultramicrochemical work with plutonium.

Tuesday, November 18, 1947

In Notre Dame. I talked with a number of friends today, including Charlie Price, Paul Doty, and Milton Burton of the Chemistry Department.

Tonight's 8 p.m. talk covered the manifold prospective uses of nuclear energy. I began with a description of the magnitude of nuclear energy ($E = mc^2$), the wartime production of plutonium at Hanford in order to produce the Nagasaki bomb, power piles, and the possibility of breeder reactors (utilizing Pu^{239} from uranium and U^{233} from thorium). I described the prospective use of power piles to produce process heat, generate electricity in stationary plants, for naval propulsion of airplanes, submarines, and rockets. This was followed by a descrription of the prospective wide use of radioactive isotopes in medical therapy, diagnosis, and research; and in other fields of research including industrial research. The biggest benefit to mankind, I said, first and in the long run will probably come from the use of radioactive isotopes.

Wednesday, November 19, 1947

In Notre Dame. When I learned that today's lecture would be at 4 p.m. and that I would have no scheduled duties for tomorrow, I decided to advance my departure for Washington and asked one of the secretaries to arrange for reservations on TWA Flight 210, leaving about noon tomorrow, instead of on the overnight train trip on the "Capitol Limited" from nearby La Paz. After wiring for a reservation at the Hotel Statler for tomorrow night, I visited with a number of people, including physicist Eugene Guth.

In today's Nieuwland Lecture I covered the general topic of the discovery, isolation, and chemical properties of the four transuranium elements.

After the lecture I had dinner and a very pleasant evening with Sarah and Milton Burton and a number of their invited guests in the Burton home.

Thursday, November 20, 1947

In Notre Dame. I telephoned the office of Robert Lavender, AEC Patent Adviser, to arrange for an appointment with him later today. My TWA Flight 210 left South Bend at 12:50 p.m.

When I arrived in Washington, I took the limousine to the Hotel Statler, checked in, and then went to see Lavender. My purpose in this visit was to explain to Lavender that we are not happy with the way papers, specifically one of Leo Brewer's, are being held up in his office for patent reasons. This holds up publication and affects much of the work that was done on the project. Lavender was cooperative and promised to expedite any paper I requested.

[In Berkeley, our group met as usual, and was presided over by Iz Perlman. In attendance were Ballou, Barton, Conway, Covey, Cunningham, Eyring, Glenn, Goeckermann, Hicks, Howland, Huffman, James, King, La Chapelle, Lindner, Neumann, Perlman, Robinson, Street, Templeton, Voyer, Werner, Wilkinson, and Wolfe. First Wilkinson described the chemistry he did and some preliminary results on the bombardment of tantalum for one hour with 200 Mev deuterons. Additional chemistry to separate the individual rare earths, which account for only about 10% of the activity, is being done. He mentioned that a sample of bromine showed very little activity, indicating only a small amount of fission. Templeton commented that, if fission were observed, it will be the first time that tantalum has been fissioned with 200 Mev deuterons; Wilkinson said he thought it definite that some fission occurred.

Perlman announced that a new power system for the deflector on the 184-inch cyclotron has been installed, so Ghiorso's experiments on counting fissions and secondary particles in the deuteron beam and experiments on the secondary alpha production of astatine by bombardment of bismuth with deuterons are now possible. Templeton described the proposed experiments on bismuth to produce At^{211}.

Cunningham talked about Asprey's work on the oxides of americium--he made the black oxide by heating americium in air at 800°C. It has the fluorite structure and is assumed to be AmO_2. Asprey heated the black AmO_2 at 1650°C in a tantalum crucible in vacuo to try to produce Am_2O_3, but it volatilized, probably because the tantalum reduced the AmO_2 to AmO known to be volatile. When this was repeated with a platinum

crucible, the product was red--x-rays showed that it is not AmO_2 so it may be Am_2O_3; it is quite stable. Am_2O_3 is expected to be reddish but not so dark.

Howland described the separation of bismuth from an old sample of lead originally given 82 μah of 20 Mev deuterons in August of 1946. Bismuth was separated from the entire target and put through 11 purification cycles. He found a long-lived bismuth that is either Bi^{203}, Bi^{205}, Bi^{207}, or Bi^{208}. Howland said, assuming all d,2n and d,3n reactions on lead have the same cross section and that the counting efficiency of Bi^{206} is the same as that for this long-lived bismuth, the half-life comes out: if Bi^{203} = 7 years, Bi^{205} = 100 years, Bi^{207} = 300 years, Bi^{208} = 200 years. The radiations are essentially identical with those from Bi^{206}, but it does not decay. Howland said he bombarded Pb^{206} oxide, containing 60% Pb^{206} and less than 0.05% Pb^{204} with 40 Mev deuterons in the 184-inch cyclotron for 40 minutes. The bismuth fraction decayed with a 12-hour half-life (Bi^{204}) with no indication of any activity ascribable to Bi^{202}.

Howland also talked about the techniques for mounting powder targets for the 184-inch cyclotron. In addition, he plans to study the magnetic susceptibility of the actinide elements in a micro apparatus designed by Dr. Calvin; this is a variation of the Gouy method.

In answer to a question from Perlman, Robinson said that a few glass pieces are missing from the oscilloscope for measuring pulse heights. Huffman reported a column run for the separation of hafnium and zirconium but the fractions have not been analyzed. Covey announced that the stainless steel discs have arrived and that various people have reported that they work well for mounting Geiger samples.

* * * * *]

Friday, November 21, 1947

In Washington. In my usual manner I took a taxi from the Hotel Statler to the AEC Building in order to attend an Executive Session of the GAC meeting. All of the members were present: J. Robert Oppenheimer, James B. Conant, Lee A. DuBridge, Enrico Fermi, I. I. Rabi, Hartley Rowe, Glenn T. Seaborg, Cyril S. Smith, and Hood Worthington. We then met with James B. Fisk, Ralph P. Johnson, and George L. Weil. Johnson talked about support for basic research, and Weil described the reactor program and the special program to produce polonium in a reactor. Fisk reported on the Reactor Development Group, which consists of Fisk (chairman), Weil (executive secretary), Harold Etherington, Gale Young, Walter H. Zinn, Winston M. Manning, Alvin M. Weinberg, and Harvey Brooks. Fisk said the group, which met on November 17 with Eugene Wigner, Admirals Earle W. Mills and Thorvald A. Solberg present, did not favor building a practical power reactor but favored the present program. In the future the group may consider a thermal power breeder, a high temperature gas-cooled pile, a mobile unit, and a plutonium production unit of new design. The power pile group at Clinton would consider a Navy mobile unit, including other types besides gas-cooled.

We want our representative on the Reactor Development Group and Luis

Alvarez was suggested for a year or two. The next meeting will be December 5 in Chicago and Charles W. Wende and W. Kelly Woods from Hanford will be invited to attend. The group, which will meet every two or three weeks, will also ask Ernest Lawrence and possibly Farrington Daniels to give their views. There will be a meeting on December 8 of the industrial power pile group. The group will be asked to work on limited objectives--e.g., a mobile unit for submarine use.

Fisk read a letter from Ernest Lawrence offering to work on the design of a practical, rush job, power pile at Berkeley; Fisk asked the GAC for advice. There was some feeling that such a program would be premature and might use men there who should be making measurements and designing apparatus for use with the synchrotron and the frequency-modulated cyclotron.

At 2 p.m. there was a joint meeting with the Advisory Committee for Exploration and Mining. John K. Gustafson will be the Director of the AEC's Division of Exploration and Mining. Members of this committee are Wilbur Judson, Gustafson, Donald H. McLaughlin, Walter J. Williams, E. L. DeGolyer, Wilbur E. Kelley, and C. K. Leith. Kelley reviewed the raw material situation.

Later we members of the GAC talked about nuclear weapons tests.

Bob Gould and Jim Crowe of the American Chemical Society staff had dinner with me.

Saturday, November 22, 1947

In Washington. I again went to the AEC Building, where at 9 a.m., the GAC discussed the Sandia organization and the future of the NEPA (Nuclear Energy for the Propulsion of Aircraft) project with the AEC Commissioners. Tonight a committee, consisting of Fermi, Smith, Conant, and DuBridge will draft a statement on the future of atomic power using the Fermi-Smith and Oppenheimer-Rabi drafts.

The GAC also decided to investigate the AEC's export control regulations informally.

On the subject of the Berkeley bevatron, Rabi said he thinks that the cost estimate is way off judging on the basis of Alvarez' underestimate for his linear accelerator project. Brookhaven estimates a much higher cost. Rabi said he wants to pass on the technical feasibility of the plan and would like to take it to Columbia and Brookhaven for evaluation. Fermi (with Oppenheimer agreeing) is concerned that the present machines will not be used to the maximum. He would stake his scientific future on this rather than on a new machine. In addition, he would like to see the Radiation Laboratory plans, including those on research with their other accelerators, for the next five years. Fermi then moved, on Fisk's request, that there were inadequate data and the Research Division should 1. Get more information on the Berkeley plan on the use of personnel during the construction period. 2. Get the complete plans from Berkeley and Brookhaven reviewed by a competent group--possibly exchange Berkeley and the Brookhaven plans for mutual criticisms.

Conant noted that he is more interested in the general question of government (AEC) support of research in pure science, apart from research on nuclear bombs and fuel, and where to draw the line. He added to the motion a number 3. To try to develop such general criteria. Then, using such data and criteria, the GAC will come to some conclusion by time of next meeting. Point 3 was added to the motion to be discussed at the present meeting. The motion passed: 8 for, GTS not voting.

Fisk then raised the question of whether there should be a power group working on naval propulsion. We favored this and pointed out that this should be primarily for engineering-type research--also without limiting the program to this--e.g., the group should also consider natural uranium reactors.

The members of the GAC met and had lunch together today with the AEC Commissioners.

At the 2 p.m. meeting the GAC discussed declassification--e.g., Fermi's views on nuclear weapons and army officers' lack of information and understanding of this important matter. We talked to General McCormack about this, and we also mentioned the need to educate the public on nuclear weapons, especially the claims of some proponents about the feasibility of super weapons.

Then at 3 p.m. the GAC again met with the Commissioners and discussed declassification (so we can educate the army, public, etc., about nuclear weapons), the situation with respect to expansion and more efficient operation at Sandia and Hanford, and the report that the GAC plans to make to President Harry Truman (covering the first year's operation of the AEC).

At 5:30 p.m. the GAC was honored by a cocktail party (stag) given by the Military Liaison Committee at the Army and Navy Club.

[At home, California defeated Stanford in the Big Game, which was played in Palo Alto today. Stanford was leading 18-14, and Cal had the ball on its own 20 with three minutes left to play when injured halfback Paul Keckley was allowed to go into the game. Jackie Jensen took a lateral from quarterback Dick Erickson and intended to run. Instead, he fired a horrible pass to Keckley 15 yards downfield who bolted 65 yards for a touchdown. Cullom's kick made it 21-18 in favor of California.]

Sunday, November 23, 1947

In Washington. I again attended the GAC session at 9:30 a.m. in the AEC Building. We worked and adopted the draft on the future of atomic power that Fermi, Smith, Conant, and Du Bridge produced last night, discussed the urgency of expansion and improvement of the nuclear weapons production facilities at Sandia. Our five-page statement on nuclear power described the technical problems and complex economic factors to be overcome in building a nuclear power system; we conclude with the summary that we do "not see how it would be possible under the most favorable circumstances to have any considerable portion of the present power supply of the world replaced by nuclear fuel before the expiration of twenty years." We discussed our first annual report to President Truman

(which Oppenheimer will draft), the need for declassification (we will stress with the Commissioners the nuclear power and superweapon misunderstanding by the public), the need for a policy on the support of pure research (e.g., the bevatron).

At 2 p.m. we held our windup joint session with the AEC Commissioners and staff, and with the members of the Military Liaison Committee. Oppenheimer summarized our recommendations on Sandia, Fisk's three questions (NEPA, Naval propulsion, bevatron), nuclear weapons tests (there will be a GAC meeting on December 30 in Chicago to discuss this), the Military Liaison Committee's view on keeping the gt level of plutonium and tritium classified as top secret, our views on the status of the nuclear weapons tests of our rivals, and the need for a public statement on big weapons.

The afternoon session was over early enough for me to go back to my hotel, take a taxi to the airport, and catch Capital Airlines Flight 903 to Chicago (it left Washington at about 6 p.m. and arrived at about 8:30 p.m.) I had a long wait in the Chicago airport for my connecting flight to San Francisco. I tried to call Joe Katz, but he apparently was not at home.

Monday, November 24, 1947

I left Chicago on TWA Flight 153, which made stops in Kansas City and Los Angeles, and arrived in San Francisco at about 10 a.m. [Don Ameche was aboard the flight as far as Los Angeles.] From the airport I took a limousine downtown, where I was met and driven to the lab.

At the lab I looked over my stack of mail: Eric A. Löf (American Cyanamid Company) wrote on November 12 to inform me that I have been unanimously elected to receive the 1948 John Ericsson Gold Medal of the American Society of Swedish Engineers. Today I wrote Löf to say that I am happy to be the recipient and that I shall be in New York on February 11 for the occasion of the dinner and in order to receive the medal.

Simon Freed sent an abstract for the Chicago ACS Symposium in April; in a covering letter, dated November 12, he thanked us for the americium fluoride that Cunningham sent him.

I read a letter dated November 12 from my friend, L. Reed Brantley, thanking me for my talk before the Southern California ACS Section. [This was the talk I gave on November 8 for Iz Perlman.]

Arthur B. Lamb sent me on November 17 a manuscript entitled "A Periodic Arrangement of the Atomic Nuclei. Revision to Date and Inclusion of Radioactive Isotopes" by Dr. H. L. Johnston. This, Lamb explained, is supposedly an extension of Johnston's predictions of 1931, and Lamb asked for my opinion as to the merits of Johnston's contribution and my suggestions. I asked Dave Templeton to look over the manuscript and advise me about my response.

I read a November 17 note from Bob Gould, thanking me for my comments on the x-rays from uranium and saying that he will see me in Washington (as he did last weekend).

W. Y. Chang (Professor of Physics, Princeton) wrote on November 17, to tell me about Chien-Chang Woo (Institute of Physics) and T. C. Chu (Institute of Chemistry) of Academia Sinica, Nanking, who wish to apply for admission to work under my guidance on radioactivity. Chang said both men are capable research workers and pleasant and responsible men. They have a grant to work in this country for a year. Chang said that he understands that it may be difficult for them to work along these lines but, since they are just beginners, they need training on fundamental, academic work. This is a matter that I shall discuss with Professor Latimer since, at the present time, we cannot invite Chu to work on the hill.

In today's mail I received a letter from Charles Coryell, who has finally received a copy of our report BC-59, "Table of Isotopes." Coryell sent us a few corrections and said that he will check it over in more detail when he has time. He also sent the opening section and bibliography of his talk at the University of Wisconsin, saying that he had to make a number of decisions about referencing project material and mentioning that he is asking the University of Wisconsin Press permission to mimeograph the whole paper for circulation.

Also in today's mail I received a copy of the preliminary program that Joe Katz and Clark Hindman worked out for our April symposium on the chemistry of the transuranium elements. I am scheduled to give the "Introductory Remarks and Historical Survey." There are presently listed 31 papers, all of which must have abstracts written in the near future and the papers declassified. Ten of the papers listed are scheduled to be written by men at the University of California.

Tuesday, November 25, 1947

I spent some time on campus talking with Professor Latimer about my trip and other things. The Chemistry 123 class was given the following midterm at 10 this morning; I then went up the hill and made the usual rounds to see how the work at progressed during my absence.

1.

(15) Am^{241} decays only by alpha-emission. Its specific alpha-activity is 6.55×10^6 disintegrations per minute per microgram. What is the half-life of Am^{241} in years?

(15) Am^{242} is a beta-emitter of 18-hours' half-life which decays into Cm^{242}, an alpha-emitter. A pure sample of Am^{242} of 2×10^6 disintegrations per minute has in it, after 18 hours decay, 5000 alpha-disintegrations per minute due to Cm^{242}; after another 18 hours, there are 7500 alpha-disintegrations per minute. What is the half-life of Cm^{242}?

2.

(5) (a) What mechanism for gamma-ray stopping accounts for most of the absorption of 1 Mev gamma-rays in aluminum absorbers?

(5) (b) Why does lead have a lower half-thickness for 200 Kev gamma-rays than does aluminum? Half-thicknesses are expressed in milligrams per cm^2.

(15) (c) The following isotopes of uranium, element 92, and modes of decay are known:

U^{233}	U^{234}	U^{235}	U^{237}	U^{238}	U^{239}
α	α	α	β^-	α	β^-

Predict the mode of disintegration for U^{232}, U^{236}, U^{240}. Give the reason for the prediction.

(5) (d) Give a qualitative explanation why no alpha-particles are seen among the heavy elements (above lead, atomic number 82) of energy around 3 Mev.

3. (25)

A 1 cm^2 area thin gold foil 0.01 mm. thick (4.15×10^{19} atoms Au^{197} per cm^2) is exposed to a beam of slow neutrons for 2 minutes. The amount of Au^{198} activity induced by the reaction: $Au^{197}(n,\gamma)Au^{198}$ is 1.76×10^6 disintegrations/min. The half-life of Au^{198} is 2.70 days and the cross section for the reaction is known to be 100×10^{-24} cm^2. What is the slow neutron flux in neutrons per cm^2 per sec.?

4. (15)

In the fission of 1 gm. of U^{235}, Ce^{144} (275 days' half-life) is formed with fission yield of 5.3%. What activity in curies does the Ce^{144} possess?

Wednesday, November 26, 1947

Jeanette and her friend Eino Karpinen are leaving Berkeley today to move to Southern California--they plan to get married in about a week and Eino, a floor layer, plans to find work in the area.

At the lab today I wrote a brief letter of recommendation for E. J. Leshan, who is applying for admission to the graduate school at Harvard. I explained that I do not have much information on him although he made a grade of "B" in our course in nuclear chemistry, a course in which the class was composed of about two-thirds graduate students. I said that Leshan impressed me as having a rather nice personality.

Another note went to my old friend, Professor Wayne K. Wilmarth at USC. I said that we were surprised to see that USC is going to play Michigan in the Rose Bowl and I would appreciate it if he could, by some means, get me two tickets for the event. I then said that I enjoyed seeing him at the time of my recent talk there.

A rather lengthy letter went to Joe Katz, commenting on the "Preliminary Program" for the ACS seminar on the chemistry of the transuranium elements. I told Joe that I shall soon send him both a secret version and a declassifiable version of Paper 22.1, "The New Element Americium (Atomic Number 95)," and Paper 22.2, "The New Element Curium (Atomic Number 96)" for publication in PPR, Volume 14B. Finally, I mentioned that I spoke with Lavender when I was in Washington about the holding up of one of Brewer's papers for patent reasons and I believe that I can get him to release any papers that are being held up for that reason if he (Joe) will give me information about them.

In today's mail was a memo from President Sproul, requesting that I serve as a member of a promotion committee, under the chairmanship of Professor W. F. Langelier. When I checked, I learned that the other members of the committee are A. R. Olson, E. Segrè, and J. G. Hamilton. The men under consideration for promotion to Associate Professor (tenure rank) are Robert E. Connick, Edwin F. Orlemann, and Richard E. Powell.

Thursday, November 27, 1947 (Thanksgiving Day)

This morning Stan and I went out to Mira Vista for nine holes of golf this morning and lots of conversation. This is the first time I have played in about three months: SGT-46, GTS-54.

Helen, Peter, and I then had our Thanksgiving dinner together--Lynne did not partake of the meal. Later I worked on my writing projects.

Friday, November 28, 1947

Today is an academic holiday, giving the graduate students a full day to work on their research. After looking at the mail, I made my usual rounds to talk with the men about their work.

In today's mail a letter arrived from Morris Perlman stating that he had no changes to propose in our paper, "Search for Elements 94 and 93 in Nature. Presence of 94^{239} in Pitchblende," although in retrospect he

wishes that he had given the final sample a longer count in the fission chamber to preclude the small possibility that uranium was responsible for the observed alpha activity. However, he said, the conclusions were cautious and probably correct, and he believes the paper should be published in its present form.

A most helpful reply arrived from George Boyd, to whom I recently sent a draft copy of my Sigma Xi lecture, "The Eight New Synthetic Elements." Boyd sent me some new data and several more complete references.

I also received a nice congratulatory note from Alden Emery about my selection as the 1948 recipient of the Nichols medal.

I wrote a long letter to Alden Emery in reply to his recent letters, saying that it seems to me that the Nucleonics Glossary project (about which he inquired in his letter of November 18) is probably a worthwhile undertaking. In his November 25 letter Alden included a letter from Arthur Lamb, who answered my questions about the delay of the publication in ACS journals of papers presented at our proposed symposium at the ACS meeting next spring [this is desirable so that they can appear first in our National Nuclear Energy Series volumes]. I told Emery that I believe that there will be no major disagreements and that I am willing to go ahead with plans for the symposium on the basis of Dr. Lamb's letter, which suggested as an alternative, the diversion of some of these classic papers to the Journal of the American Chemical Society for publication. I pointed out that the symposium contributions will not be identical with those offered for publication in the NNES.

Another long reply went to Charles Coryell, covering a miscellany of subjects: corrections to the "Table of Isotopes," referencing to MDDC (Manhattan District) items, the Wisconsin Symposium, etc. I also included a draft copy of "The Eight New Synthetic Elements," my Sigma Xi talk--I asked for his comments.

Mary Bender prepared and submitted my claim for reimbursement for my recent trip.

Saturday, November 29, 1947

Much of my day was spent at the Radiation Laboratory talking with the various me and catching up on my report reading.

Sunday, November 30, 1947

Helen, Lynne, Pete, and I spent the day together. In the afternoon we went for a ride in our Hudson.

I also worked for a while on my paper for the spring symposium on the chemistry of the transuranium elements. I am scheduled to give "Introductory Remarks and Historical Survey."

[Oppenheimer's report (following) to Lilienthal of the Seventh Meeting of the GAC (November 21, 22, and 23) is dated November 28, 1947.]

GENERAL ADVISORY COMMITTEE
to the
ATOMIC ENERGY COMMISSION
Washington 25, D. C.

November 28, 1947

Dear Mr. Lilienthal:

The General Advisory Committee held its Seventh Meeting in Washington on November 21st, 22nd and 23rd, 1947. This brief report will supplement our oral discussions with the Commission, the Military Liaison Committee and the Staff of the Commission.

1. Draft Statements on Atomic Power

In accordance with our plans of the Sixth Meeting, the GAC considered two draft statements on atomic power, one secret and one top secret. The Committee found that despite slight differences in emphasis and in tone these documents reflected a common evaluation of the situation and together contained an account of the principal facts on which, as of today, the prospects for atomic power in its various forms may be evaluated. The Committee transmits these two draft statements to the AEC in the hope that they may prove useful.

The Committee further prepared a third draft, classified confidential, which included the parts of the secret and top secret drafts which (a) seemed of the greatest relevance and (b) involved the minimum of disclosures not declassifiable under present declassification policy. This confidential draft it transmits as an example of a statement which might be released publicly and which could be helpful in putting the views of technical, industrial and military men on a relatively sound basis of fact. The Advisory Committee recommends to the Commission that it prepare public statements on atomic power, making such use of our draft statements as it desires. Should it prove useful or convenient to the Commission to release our confidential draft as a statement of the Advisory Committee, the Committee has agreed to concur in this action.

2. Low Grade Ores

The GAC had the pleasure of meeting with the Director of Raw Materials and the Advisory Committee on Exploration and Mining, and is glad to note the attention which the Commission is giving to this most important field of activity. After common discussion, the GAC felt it desirable to emphasize again the very great importance which it attaches to the development of source materials available in the United States in which the uranium content lies in the range of one one-hundredth of a per cent. It asks the Commission to commun[redacted]portance to the

Advisory Committee on Exploration and Mining, and suggests that the various draft statements on atomic power be made available to that Committee in order to give background to this view.

3. Raw Materials Hold-up

As a further result of our meeting with the Advisory Committee on Exploration and Mining, we had occasion to consider the account presented by the Commission's Staff on the raw materials situation during the next three years, during the period, that is, before one or more of the many developments now being actively pursued may be expected to ameliorate the situation.

After noting that our requirements were listed at figures apparently in excess of actual plant intake requirements, we appointed a sub-committee with Mr. Worthington as chairman, and Dr. Smith and Mr. Rowe as members. The purpose of this committee will be to explore with the Commission's Staff methods whereby hold-up and delay in refining, fabrication, stock piling and pipe-lining procedures might be reduced.

4. Pacific Tests

After studying working papers on the nature of the weapons to be tested, and the order in which they might be tested, the GAC agreed to consider these questions in detail at a special meeting to be held in Chicago, December 29th and 30th. At that time formal proposals from the Los Alamos Laboratory will be available and certain at present obscure technical questions may be on a firmer basis. We shall hope to invite to this meeting representatives of the Director of the Los Alamos Laboratory as well as members of the Commission's Staff most closely involved.

We wish to note at this time that the technical situation, particularly with regard to the amount of uranium 235 that can be included in the implosion weapon, is now rather different from that which was before us at the Fifth Meeting of the GAC, and that for this reason our earlier recommendations should be construed as tentative.

5. Bevatron Program

The Director of the Research Division referred a proposal to us from Dr. Lawrence in Berkeley for the construction of a proton synchrotron in the multi-billion volt energy region. He asked us whether this proposal was sound, whether it was timely, and whether such an instrument should be built in Berkeley, in Brookhaven, in both, or in neither.

The GAC discussed this and related questions at very great length but is not prepared to answer any of Dr. Fisk's questions definitively at this time. The Committee suggests the need for at least some further information. We believe it essential that some detail be obtained by the Commission on the technical plans for

the bevatron. No plan whatever was available to us. The Committee would suggest that both Brookhaven and Berkeley give an outline of their present plans, that these be studied and compared by a competent technical group and conceivably that they be interchanged between the two laboratories. This step is required in order to determine the feasibility and reasonableness of the proposals as technical proposals.

The Committee also believes that it would be necessary to explore the probable impact of bevatron construction on the proposed scale, and at the proposed rate, on the activities of the personnel at the radiation laboratory at Berkeley, since it has noted that that laboratory will shortly be in possession of other major electro-nuclear equipment, the use of which for fundamental studies in science should not be jeopardized. For its part the GAC will attempt to formulate criteria for the extent of Government support of pure basic science in Government supported laboratories. Differences of opinion between the members of the Committee have made progress on this problem very slow.

6. Sandia

At the Sixth Meeting the members of the Military Liaison Committee asked the views of the GAC with regard to the work at Sandia, and this was made an order of business for the Seventh Meeting.

As a basis for our consideration we had a memorandum prepared by Admiral Parsons and transmitted by the Military Liaison Committee to us, and two thoughtful working papers prepared by the Secretary of the GAC, the longer of which we have asked to have made available to the Commission's Staff.

We recognized that some of the difficulties at Sandia derive directly from the fact that it is at this site that the interface, contemplated in the Atomic Energy Act between civilian and military operations, lies. We also recognized that on the civilian side Sandia has been seriously under-staffed, quantitatively and qualitatively, and that many of its difficulties are inheritances from the early days.

We have two suggestions which we believe may help to alleviate this situation. We do not believe that there is any error in the technical plan of the work at Sandia. (a) We believe that steps analagous to those already taken at Los Alamos to strengthen and encourage the civilian staff need to be taken at Sandia. On the one hand these involve attention to housing, salaries, etc. On the other they mean clear statements and corresponding actions to establish the importance of the work for the common security. (b) We note that two essentially different and not functionally related activities now take place at Sandia. One of these is the

engineering and testing (and these are indeed inseparable) of those components of atomic weapons which are needed to convert the warhead into a useable bomb. Examples are the design of the bomb casing, the arming and fusing systems, and the necessary test equipment for service use. The other activity is the training of service personnel in the assembling and use of atomic weapons. Both sets of functions are indeed important, but they involve quite different relations between civilians and the services. It seems likely to us that a clear functional division of these activities, and perhaps a geographical division, would greatly improve the quality of the engineering work.

7. Reactor

The Director of the Division of Research told us of his tentative plan to add to the Reactor Development Group a member whose main concern would be an evaluation of prospects of nuclear energy for the propulsion of aircraft. He suggested a possible candidate.

After discussion, the GAC concurred with the view that this problem should be incorporated in the work of the Reactor Development Group, perhaps by the addition of one or more members specifically interested in it. The Committee wished to emphasize, however, the importance of involving aircraft engineers as well as experts on nuclear reactors in any studies to be made, and felt that the aircraft industry must participate on the working level if these studies were to have meaning. The Committee did not agree that the member suggested for the Reactor Development Group was a promising prospect, and wished to withhold more detailed recommendations until the precise form of the decision of the Research and Development Board has been determined.

8. Naval Power Reactor

The Director of the Division of Research reported that the Reactor Development Group had looked favorably on encouraging the power pile sections of the Clinton Laboratories to undertake a study, a design and a program of construction of a mobile power unit, perhaps suitable for naval propulsion, and in the design of which considerations of fuel economy were ignored.

The GAC welcomed the initiative of the Reactor Development Group and felt it particularly important that decisions and priorities reflect the interest of available personnel. The GAC has come to feel that in the present Reactor program there is too little encouragement for work on the engineering aspects of power production and utilization. It is pleased to note the trend in the Research Development Group toward the consideration of a reactor in which engineering reliability may outweigh in the beginning material economy, and in which experimental facilities are incorporated for their use in engineering development rather than for research in physics.

Power for naval vessels seems a desirable interim objective and a suitable one for the study of the power pile group at Clinton under the guidance of the Reactor Development Group. The GAC, however, is convinced that other problems of reactor development will remain or become urgent, and that the above mentioned program will still leave the overall effort somewhat out of balance. In particular it believes that systems using natural uranium require further effort.

9. The Urgency of Hanford Construction

In the reports of both the Second and Third Meetings the GAC has already called attention to the need for increased capacity in reactors of the Hanford type, both as replacements for the present reactors whose lifetime is uncertain, and for an increase in our plutonium production. The Committee now wishes to call attention to the urgency of the construction of further reactors. It notes that to delay the construction of two new Hanford reactors will mean a decrease in our effective stockpile of weapons, say some five years from now, of about 10% for each year of delay.

10. Discussion with the Military Liaison Committee

A. As part of our discussion with the Commission and the Military Liaison Committee, we explored with the latter our past recommendations with regard to four points:

(a) Modified urchin design.
(b) g/T level at Hanford.
(c) Declassification of the nuclear physics of tritium and distribution of small amounts to research institutions.
(d) Down-grading of work on thermo-nuclear explosives from top secret to secret.

In each case we attempted to present a fairly complete account of the reasons for the GAC's recommendations and to explore, and where possible to meet, countervailing views of the members of the Military Liaison Committee. In particular we were able to assure the MLC that our endorsement of proposed changes in urchin design was not a casual endorsement, and that it involved unqualified approval.

B. During these same discussions we had occasion to raise a general point which has recurred frequently during the meetings of the Advisory Committee. We are not convinced that the present policy of the United States with regard to the secrecy of information on atomic energy is the best policy to assure the common defense and security. In particular we believe that the declassification of results in basic physics and chemistry has been most healthy, and we suspect that it has been of relatively little use to rival efforts. But we note that in the equally important areas of technological, military and industrial effort basic facts which must underlie intelligent future effort are still for the most part secret. And we note further

that misconceptions as to the nature of atomic weapons and our present status with regard to them are widespread, and these could frustrate or subvert the steps necessary for our common defense.

As an example of the kind of action which we believe needs to be taken toward a gradual amelioration, we cite the proposed public statement on atomic power. On the far more difficult question of the military aspects of atomic energy we shall attempt in the future to prepare a paper on the advantages which might come from clearing up public misunderstanding, and which would then have to be carefully weighed against any countervailing disadvantages. This, on the suggestion of the Chairman of the Military Liaison Committee, we hope to undertake at our next regular meeting which will be in early February.

J R Oppenheimer

Chairman
General Advisory Committee

Hon. David E. Lilienthal
Chairman, Atomic Energy Commission
Washington, D. C.

Monday, December 1, 1947

Iz and I recently interviewed and hired a new secretary, Doral Buchholz, who has just moved to the Bay Area from Chicago, where she worked for Morris Kharasch for a number of years and for Oliver Simpson for a few months. In addition to a good background and references, Doral already has a clearance, so she was able to begin working almost immediately. I took advantage of her arrival today and dictated to both Mary and Doral in order to catch up on my correspondence. It was an ideal situation to introduce Doral to the pace of our work.

Doral Buchholz

I replied to a November 13 letter from George Watt, who was asked to serve on the Committee on Inorganic Nomenclature of the ACS. Watt's assignment is concerned with the designation of specific isotopes in inorganic compounds. Watt enclosed a list of ideas and asked for my comments. I made a number of suggestions---that I am not at all sure that he can't dispense with the subscript which designates whether the labeling isotope is active or inactive, that I am not sure whether it is a good idea to use the mass number as a superscript when the formulae are written out, etc. I also said that I suppose the systems for inorganic and organic nomenclature should be as consistent as possible. Finally, I told Watt about our plans for a symposium on the chemistry of the transuranium elements at the April ACS meeting in Chicago and asked that he tell Tom Morgan that we have scheduled a paper, "The Tracer Chemistry of Americium and Curium," by S. G. Thompson, R. A. James, and L. O. Morgan, which Stan or Ralph will actually give.

Recently, I read an article by English/Canadian authors [B. G. Harvey, H. G. Heal, A. G. Maddock, and W. L. Rowley, J. Chem. Soc., 1010 (1947)] on the chemistry of plutonium. I was quite angry when I noted that the article made no reference to American work, and I complained to our English scientist, Geoffrey Wilkinson, that I didn't think the authors had acted properly. While I was away, a November 9 letter arrived from Maddock, explaining that he had heard about my feelings from Wilkinson and saying that he was disgusted by the manner in which the article was handled. He explained that it had been submitted for declassification to the Canadian authorities in the summer of 1946 with an introductory statement about the source of the material and acknowledging the information and advice of Bertrand Goldschmidt and me. However, in order to have the document declassified, all reference to unpublished work of the U. S. project was deleted. Today I replied to Maddock and said that I believe I now understand the background sufficiently to feel entirely satisfied that it was handled about as well as possible. In reply to his query I said that the Plutonium Project

Record of the National Nuclear Energy Series may, in part at least, be published next year and should be available for general purchase. I told Maddock, who is beginning to work on the chemistry of protactinium, that I think there may be a lot of good research not undertaken on the basis that there is a suspicion that it has already been done, but I believe he should go ahead on the assumption that the field has not had adequate coverage. Finally, I said that Wilkinson is now fully admitted to the laboratory and is using the 184-inch synchrocyclotron.

I mailed Truman Kohman a copy of my coming Sigma Xi lecture, "The Eight New Synthetic Elements," and asked for his comments, saying that I want it to be as accurate as possible since it is scheduled to be published in The American Scientist. An additional copy of the lecture went to Professor F. A. Paneth, again asking for comments and suggestions. I specifically asked Professor Paneth about a reference in his paper to the absence of masurium (element 61) in hundreds of kilograms of rhenium, a reference to Auer von Welsbach's negative results in a search for element 61, and a reference to a similar search by S. Takvorian. I told Paneth that we have no doubt that Geoffrey Wilkinson will make an excellent contribution to our program.

A note went to Dr. H. B. Hass (ACS Committee on Foreign Compendia) in response to his query of November 20; I said that it seems to me that Harold Oatfield's project (a Cumulative Index to Chemisches Zentralblatt for the period 1881-1895) is worth supporting although I do not feel that I can comment on the method of financing it.

At the request of our local patent office I mailed copies of our two main papers on americium and curium to F. W. Test of the Chicago Patent Office. I explained that copies have been sent to Chicago for issuance as secret reports and that the copies I am sending him should be turned over to Dr. J. J. Katz of Argonne. I also mentioned that modified versions are being submitted for declassification preparatory to publication in the Plutonium Project Record.

In reply to a query from L. F. Curtiss about suggestions as to a time and place for a meeting of the Committee on Standards and Units of Radioactivity, I said that I will be in Chicago at the end of December, but this will be a very busy time and not a good time for a meeting. The next meeting of the General Advisory Committee of the AEC will be on February 6, 7 and 8 in Washington, D. C., so that Monday, February 9 will be convenient since I shall be in New York on February 10 and 11 (and again on March 5).

I wrote to Mr. T. C. Chu (Institute of Chemistry, Shanghai, China) and told him that Professor Latimer and I, in response to a request from Professor W. Y. Chang of Princeton, shall be pleased to have him come and work with us at the University of California.

On November 24 Mae H. MacKay (Assistant to the Editor of Encyclopaedia Britannica) asked several questions about the consistency of the figures in my article entitled "RADIOACTIVITY, ARTIFICIAL" with a chart in the article "NUCLEUS." After taking care of her questions, I made a few additional corrections to bring my material more up to date.

I mailed Professor Heimo Hardung (Vienna) a reprint, giving all the information available on the fission of bismuth and neighboring elements. I told him that I shall send him a reprint of my September ACS speech, which appeared in C & E News, as soon as I get some. Hardung wrote on November 10, requesting such information. He mentioned that his university did not get the dollars to buy the wartime, The Physical Review.

Ray Stoughton answered, on November 11, my questions about L. D. Norris, who was suggested as a possible graduate student, saying that from what he has learned Norris is probably a better than average but not outstanding student. Stoughton's description of Norris' lab ability sounds as though he again is about average. On the basis of the letter I wrote to Stoughton today to say that we shall not try to interest Norris in our program; however, I said that we are still interested in Arthur Fry's future plans. Finally, I told Stoughton that all of us are interested in his final decision on the job offer he received from W. H Sullivan to go to the Naval Radiological Laboratory at Hunter's Point.

Finally, I wrote to the Sales Manager of the Hudson Motor Car Company in Detroit to commend the fine service I received from the Berkeley dealer, H. W. McKevitt, Inc. Mr. McKevitt, I said, sold me a 1947 Hudson at exactly list price and is maintaining a level of courtesy and service at prewar standards. This is in contrast to the Oakland dealer who offered me an obviously brand new, but labelled "slightly used," Hudson car at about $300 above list price.

Today's mail brought a letter from Art Wahl, offering two minor changes to "The Chemical Properties of Elements 94 and 93." Wahl said that the department would like 100 bound reprints when the paper is published. He mentioned that Bonner had no changes to suggest in his paper.

A note arrived from J. W. Buchta (Rev. Mod. Phys.), who said that they will be pleased to receive the revised "Table of Isotopes" next year.

An informative letter also arrived from E. J. Durham about the Nichols Medal ceremony. Durham said that Manning and Halford have accepted the assignments I suggested, and he asked that I send him copies of an abstract of my 45-minute acceptance speech by February 1 and the complete manuscript by the middle of February. Durham also asked for a tentative list of people whom I want invited to the dinner.

I also received a letter from Joe Katz, who said that he and Hindman had visited Bailar at the University of Illinois to discuss our proposed symposium. Bailar, Joe said, was favorably impressed, will leave the details to us, but would like to have the abstracts by early in January.

Tuesday, December 2, 1947

I spent some time on administrative matters and read reports when I first got to the office.

Mary Bender prepared and submitted a bill for my travel expenses to Mr. Laurence C. Jones (Secretary of the Southern California Section of

the ACS) for the talk I gave there on November 7. She also submitted a $150 bill to General Electric for the time I spent consulting in Schenectady in early October.

Watson and Helen Davis asked on November 30 for my comments on a chart of elements that they plan to publish in Chemistry magazine in December. Today I wired my response, "CHARTS ARE CORRECT WITHIN MY KNOWLEDGE, EXCEPT MASS OF PROTACTINIUM SHOULD BE 231."

I gave the Chem 123 lecture at 10:10 this morning. Back on the hill I dictated an abstract for my introductory remarks for the symposium on the chemistry of the transuranium elements to be held next April at the Chicago ACS meeting.

In today's mail I received and read the following reply to my recent query from Marguerite Perey:

Cher Monsieur,

Le symbole du Francium a été beaucoup discuté, mais il est exact que le Professeur Paneth était d'accord avec moi pour donner le symbole Fr. dans son article de Nature.

D'ailleurs le nom et le symbole ont été acceptés à la Commission Internationale de Juillet à Londres.

J'ai regretté de ne pas vous rencontrer à Londres au Congrès International de Chimie et vous prie de croire à mes sentiments les meilleurs.

M. PEREY.

M. Perey

Translation:

The symbol for francium has been discussed a great deal, but it is true that Professor Paneth is in agreement with me in giving the symbol Fr in his article in Nature.

In addition, the name and symbol have been accepted by the International Congress in London in July.

I am sorry not to have seen you in London at the International Congress of Chemistry, and I send you my very best regards.

Later in the afternoon I attended the joint meeting of the joint Connick-Seaborg groups in Gilman Hall, and after dinner I went to the regular Research Conference, also in Room 102, Gilman Hall.

Wednesday, December 3, 1947

This morning I wrote to Ralph Halford (Columbia University) to tell him that I have learned from E. J. Durham that he has accepted the task of discussing my personal background in connection with the award of the Nichols Medal next March. I included a copy of the biographical sketch Helen prepared and told Ralph that I shall try to furnish any other information that he may want.

Livia Appel (University of Wisconsin Press) wired that my portrait is missing and asked for another copy. [This is in connection with publication of the talks at the University of Wisconsin symposium this past September.]

I mailed to Joe Katz two copies each of seven out of the eleven abstracts due from our men for the April ACS symposium, explaining that three of those not included are from Connick and Brewer and which they consider as already declassified. Included among those I mailed was my abstract for my "Introductory Remarks and Historical Survey":

The four known transuranium elements are neptunium (number 93), plutonium (number 94), americium (number 95), and curium (number 96). Although they were all discovered within the last eight years, they have already been the subject of very extensive and complete investigations, so that we now have a great deal of information about their chemical properties. All four of these elements, first found and investigated in tracer amounts by the methods of radiochemistry, have now been isolated in weighable amounts and hence investigations of their chemical properties have also been carried out on the macroscopic scale.

The results of these various investigations indicate that not only the transuranium elements, but also several of the heaviest natural elements, as well as a number of the undiscovered transcurium elements, are all related to each other in that they form a transition series. The evidence points to a filling of the inner 5f shell of electrons in such a way that a half completed shell is reached at tripositive curium (number 96). Thus, the program includes papers which discuss the chemical properties of actinium (number 89), thorium (number 90), protactinium (number 91), and

uranium (number 92), which have also been the subject of intensive chemical investigations during the last few years.

Maurice L. Huggins (Chairman of the Harrison Howe Lecture Committee) wrote on November 21 to say that he was sorry that I could not be with them for this year's Harrison Howe Lecture. He then congratulated me on the Nichols Medal Award and the Nieuwland Lectureship.

A December 1 letter arrived from W. H Sullivan in Chalk River, containing a few notes for the "Table of Isotopes" and "Chart." H mentioned that he will be in California from December 10 through the 14th and will try to call me or see me if I am in town.

Harper W. Frantz (Pasadena City College) wrote on November 21 to ask if a symbol has been officially adopted for the element francium. Frantz needs this information for a freshman chemistry laboratory manual they are developing. He also asked permission to use the periodic table I used in my lecture there last spring in the manual. In my reply I told Frantz that Mlle. M. Perey has assured me that she is proposing the symbol Fr and since there is no reason to doubt that she is the discoverer, I believe her proposal will be adopted by the international committee. I also said that I would not object to his using my form of the periodic table.

Later I made the rounds of the labs to see how the research is progressing.

Thursday, December 4, 1947

Present at this morning's group meeting were Asprey, Ballou, Barton, Chubbuck, Conway, Covey, Eyring, Garden, Ghiorso, Glenn, Hicks, Hopkins, Howland, Huffman, La Chapelle, Meinke (our new graduate student whose clearance has just been approved), Miller, Neumann, Newton, Perlman, Robinson, Seaborg, Street, Templeton, Thompson, Werner, and Wolfe. I announced that Miss Kittredge told me that Giddings and Brosmer from General Electric are here to interview possible new employees.

Glenn reported that they have made the first experiment to look directly for alpha particles and fission fragments produced by 200 Mev deuterons on heavy element targets, using a small ionization chamber mounted in a box filled with helium at one atmosphere pressure; no fissions were observed over the high neutron background. Next they plan to repeat the experiment with the beam coming out through a hole in the concrete shield.

Eyring described his unsuccessful attempts to make americium and europium metal, but he did prepare neodymium metal, which was strongly magnetic. I expressed surprise at the magnitude of the magnetism. In response to a query by me, Howland said that even the salts of rare earths exhibit this effect. Eyring reported that he checked the method and got a nice pellet of plutonium metal, and he has tried some variations in the method to try to produce americium and europium metal. I remarked that Spedding has had trouble trying to produce europium metal.

Hopkins, using the yield of As^{72} as 1, reported yields of some

products from the spallation [I have discussed the word "spalling," the word suggested by W. H Sullivan some time ago, with a member of the University's linguistic staff and concluded the word "spallation" to be more euphonious--it seems to be generally accepted to describe these reactions] of As^{75} with 200 Mev deuterons: Se^{73} = 0.08-1, Ni^{57} = 0.0002, Ni^{66} = 0.003, Co^{55} = 0.0006, Co^{61} = 0.009. He isolated a 36-hour chromium but did not determine the sign of its radiation. The yield of Cl^{38} = 0.00001. He noted two other unknown activities in chlorine fraction (not chlorine): an 18.5-hour positron emitter and an about 110-minute activity of unknown sign of the radiation.

Perlman asked if the yields in cases such as the following could be examined to determine the relative sticking probabilities of neutrons and protons: $Te^{119(?)}$ (4 day) and $Sb^{119(?)}$ (40 hour) from deuteron bombardment of Sb^{123}. He said that spallation reactions are now thought possibly to occur by the reaction of the individual nucleons, and the relative yields of these two isotopes should give a measure of the relative neutron and proton sticking probabilities. However, he said, the catch is that Sb^{119} can be made from protons as well as neutrons and knowing whether it is produced by knocking out one proton plus five neutrons or knocking out six neutrons is likely to be difficult.

Ghiorso announced that he and Cunningham are leaving December 9 for Chicago to measure fissionability of Np^{235} and Np^{236} and the rest of the isotopes on their list.

La Chapelle reported that he reacted the oxychloride of neptunium(V) with CCl_4 at elevated temperatures and that Templeton is examining the products by x-ray diffraction. Templeton said that the samples are interesting, but he hasn't figured them out yet. Templeton went on to report that Zachariasen's cell constants for AmO_2 were not on the smooth curve for uranium, neptunium, and plutonium dioxides until he corrected for the lanthanum impurity; however, the Berkeley sample of AmO_2 with at least ten times less lanthanum still had the same experimental value. I suggested that the lattice dimension is off the curve and the sample may have a mixed oxidation state of americium.

Hicks said he has bombarded tantalum with 200 Mev deuterons and is examining tungsten, tantalum, hafnium, and the rare earth fractions. The latter is being put through a column to yield thulium, ytterbium, and lutecium. Hicks described his chemical procedure.

Covey, who is taking inventory, requested information from the fellows on the location of our ionium and the 2 mg radium sources.

Perlman returned to the subject of bringing the deuteron beam through the shielding and said it would be interesting to know the lethal dose for such deuterons; he referred to a comment by R. R. Wilson in a "Letter to the Editor" of The Physical Review, which stated the lethal dose is arrived at in a matter of a second or so.

Conway reported that the silicone grease used on the rotary condenser of the 184-inch cyclotron is about 30% organic, which may be lithium stearate. In addition, he reported he is beginning to look at the sharp optical absorption bands of solid rare earth crystals, preparatory to

working on actinide crystals.

Ballou, who has been doing sodium amalgam extractions of samarium from buffered acetic acid solutions, tried separating americium from curium by this method; he observed an enrichment of americium in the precipitate. It does not give an americium-curium separation, but it does separate them to a useful extent from element 61. Ballou plans to try amalgams of metals of smaller reducing potentials in an effort to extract americium away from curium, and I noted that Stan Thompson has tried this. Stan added he did a few experiments with mixtures of americium and curium and sodium amalgam, in which the ratio of the americium to curium activity was increased by a factor of nearly two in the samarium precipitate that was formed. He had thought of doing the reduction from a hydrochloric acid solution and precipitating barium chloride, but he never did the experiment.

* * * * *

At 10:10 a.m. I gave the Chem 123 lecture and then returned to the hill after checking in with Miss Kittredge and various colleagues.

Cliff Garner sent me a few minor typographical changes to the paper about 94^{239} in carnotite, which I received today. Cliff also said that he told Hindman and Katz that he will report on this paper at the spring ACS symposium, provided that it is cleared in time. He also mentioned that he has been unsuccessful in getting clearance on any of his Los Alamos results and therefore does not plan to try to discuss any dry chemistry. I immediately replied that I agree he should speak on the natural occurrence of plutonium rather than try to get his Los Alamos results cleared. I suggested that he send his title to Hindman and Katz immediately and the abstract, similar to the summary in our paper, within a few days.

I also prepared a recommendation for our undergraduate student, David D. Cudaback, to be used for applying for an undergraduate scholarship. I said I believe Cudaback gives good evidence that he will become a good research scientist.

Friday, December 5, 1947

Recently Professor Arthur Lamb, as Editor of the Journal of the American Chemical Society, asked me for comments on "A Periodic Arrangement of the Atomic Nuclei" by H. L. Johnston. I gave the paper to Dave Templeton to read and give me his opinion. We talked it over, and today I wrote Professor Lamb, saying that I do not believe the paper should be published. I said that his 1931 paper was a definite contribution, but Professor Johnston does not seem to be cognizant of recent progress and literature. I then went on to point out numerous specific errors in the paper.

I also wrote to George Boyd and thanked him for his suggestions for my Sigma Xi lecture; I asked for a photostat of the Takvorian article. I also asked if the later article by Perey [J. Chim. Phys. 43, 262 (1946)] used the symbol Fr in it, for it is better to use the journal reference rather than a private communication. Finally, I said that I assume that

my reference 17 should be Ketelle and Boyd, private communication, for the isolation of both technetium and element 61.

Another note went to David Lilienthal, thanking him for the copy of his speech before the American Society of Mechanical Engineers; I said that I believe his discussion of secrecy and security was well done and should be a great help in clarifying this issue for the American people.

I read two interesting documents that arrived from Chicago yesterday: "Isolation of Actinium (Ac^{227})" by French Hagemann and "The Preparation of Actinium Compounds" by Sherman Fried and French Hagemann. These are write-ups of work I learned about on my recent visit to Chicago. I routed the reports to Iz, Burris, and Dave Templeton.

I looked over a memo from W. H. Zachariasen to A. J. Dempster entitled, "Crystal Structure Studies of Actinium Compounds (ANL-FWHZ-142)." These studies were done on compounds prepared by Sherman Fried and French Hagemann. I routed the report to Perlman, Cunningham, and Templeton.

My copy of the Extension of Retainer Agreement arrived from E. E. Chambers (Executive Engineer of General Electric Company, Schenectady) today.

I spent considerable time talking with the men about their research.

Saturday, December 6, 1947

Again, I spent the day at the lab--reading reports and journals and talking with the men about their research.

My sister Jeanette was married to Eino Karpinen by a Congregational minister, Charles S. Mandell, in Los Angeles today. Her long-time friend, Inez Wyman Sicola, and a friend of Inez', Tom E. Prusik, were witnesses.

Sunday, December 7, 1947

I drafted a two-paragraph non-technical abstract of my Sigma Xi lecture and then worked for a while on my revisions to Volume 14A of the Plutonium Project Record.

Later I spent some time with Pete; I have plans for making him a star football player.

Monday, December 8, 1947

At the office this morning I gave the abstract I prepared yesterday ("The Eight New Synthetic Elements") to Mary to have typed. This abstract, which includes some description of the background and significance of the paper, is to be used for press releases. Later I proofed the typing and had it mailed to the American Association for the Advancement of Science (AAAS) office in Washington, D. C. (1515 Massachusetts Avenue, N.W.).

Although there had previously been only three new elements discovered since the turn of the century, there have in addition been eight new elements discovered during the last ten years. These elements are all radioactive and all except one of them do not exist in appreciable amount on the face of the earth and can be made only by artificial means and may be designated as "synthetic" elements. Four of these elements are the famous "transuranium" elements which are heavier than the heaviest element on earth, uranium, which has the atomic number 92. The names of these four, with the corresponding atomic numbers, are neptunium (93), plutonium (94), americium (95), and curium (95). The other four elements have smaller atomic weights and fill in the four previously existing "holes" in the periodic classification of the elements. The names of these four with their corresponding atomic numbers are technetium (43), promethium (61), astatine (85), and francium (87). Francium is the only one which exists in easily detectable amount on earth. (However, plutonium exists naturally in extremely small amounts.)

For the first time all of the elements up to uranium are known and thus, together with the four transuranium elements, a total of 96 elements are now known. The only possibility which now remains for the discovery of new elements is to discover elements which are higher on the scale than the number 96. Of these eight recently discovered elements plutonium (number 94) is of transcendent importance because this is the element which, together with the famous isotope, U^{235}, forms the basic substance for the atomic energy development. Thus, this substance is important not only because it is an explosive ingredient for atomic bombs, but also because it constitutes a "fuel" substance for the future industrial applications of atomic power.

A note arrived from John C. Bailar, Jr. (Division of Physical and Inorganic Chemistry, ACS), who said that Katz and Hindman had visited him and discussed the symposium on the transuranium elements scheduled for the Chicago ACS meeting. Bailar said the arrangements have been well handled and that he thinks the symposium should be an outstanding one.

Emilio Segrè returned the copy of my Sigma Xi talk that I sent him about a week ago with a few pencilled-in remarks. He asked for a copy of the talk.

Tuesday, December 9, 1947

I spent the morning on campus and gave the Chem 123 lecture at 10:10 a.m. Later back on the hill I wrote a letter to Joe Katz, giving him the status of our various PPR papers and asking him for a copy of Paper No. 22.7 ("Preparation and Radioactive Properties of Am^{242}" by Manning and Asprey) for my use.

In the afternoon I returned to campus again to attend the meeting of the Connick-Seaborg groups. Then, I attended the Chemistry Department's Research Conference after dinner.

Wednesday, December 10, 1947

Much of my day was spent talking with the graduate students about their research and working on various writing projects. Preparation of the papers for the Plutonium Project Record is an enormous job.

Stan Thompson took his prelims at 2:30 this afternoon in Room 102, Gilman Hall. His committee consisted of William F. Giauque, Melvin Calvin, Joel H. Hildebrand, L. H. Duschak, Joseph G. Hamilton, and Bruno H. Zimm; and the propositions he discussed were "The Age of the Earth" (physical chemistry), "Energy Relations in Carbohydrate Metabolism" (organic chemistry, and "The Electronic Structures of Uranium and Tungsten" (inorganic chemistry). He reported later that he passed the examination, which pleased me very much since Stan and I have been friends since our freshman year (1925) at David Starr Jordan High School in Watts (Los Angeles).

Helen went to the monthly tea for the wives of the Chemistry Department professors. Today the tea was held at Frances (Mrs. T. Dale) Stewart's home (506 Arlington Avenue) in Berkeley.

Thursday, December 11, 1947

In attendance at this morning's group meeting were Ballou, Barton, Chubbuck, Conway, Covey, Garden, Glenn, Goeckermann, Howland, Huffman, James, King, La Chapelle, Lindner, Magnusson, Meinke, Miller, Neumann, Newton, Perlman, Robinson, Seaborg, Stewart, Street, Templeton, Thompson, Wilkinson, and Wolfe. Conway began the meeting by telling about his plans to study the absorption spectra of crystals of lanthanide and actinide salts in order to compare the similarities of atomic structure for the two series. He said the salts of the lanthanides are known to give sharp absorption bands of discrete structure just as their solutions do, and solids are usually studied in the form of single crystals at low temperatures although it is now known that several moderate-sized crystals show more absorption lines than a single large crystal. Conway went on to say that he has difficulties preparing large crystals. I pointed out that this work is especially important because we do not have directly comparable spectra. In response to a question from Newton about what kind of transitions give sharp lines in solids and solutions, I said that it is the rearrangement of 4f electrons to give different atomic terms, different L values, same total quantum number (i.e., 4).

Wilkinson reported more results from their tantalum + 400 Mev helium ion bombardment: the rare earth fractions above erbium are decaying with a curved line with a slope of several days, each segment different. The lighter rare earths have decayed out, and the heavier fractions are being put through the Nalcite column again for further purification. There then followed a discussion about the last batch of resin from Clinton Laboratories that is giving poorer results. Wilkinson said that he and Hicks had no difficulty getting adequate separation in their work. He then went on and said that he has worked up a tantalum foil that was used to cover targets bombarded with 40 Mev helium ions. He found the previously known 140-day W^{181} and a new 64-hour rhenium, probably Re^{182}, which has about 0.2 Mev conversion electrons and a 1.6 Mev gamma ray, in addition to K and L x-rays. Templeton commented that David

Karraker and Harold Lohr, two of our as yet uncleared new graduate students who are working in Gilman Hall, have also seen this rhenium in a similar bombardment.

Howland described his capillary assemblies that should be suitable for his proposed magnetic susceptibility studies of the actinide elements. He plans to run $NiCl_2$ to calibrate the magnet first. I said he should decide what actinides he will want soon so someone can supply them and that uranium and neptunium will be the least hazardous. I also told Howland to have Nels Garden approve the procedure for use with plutonium and americium. Howland said he will need 50 to 500 micrograms of element in less than 0.5 ml of solution.

Neumann reported that he has bombarded titanium with 40 Mev helium ions in the 60-inch cyclotron for three hours and found the 43-minute Cr^{49}. Chromium was separated several days later and gave a decay curve that was flat for several days and then decayed with a 16-day slope. I suggested that this looks like the V^{48} daughter, but that there must be two decay chains going on to give the peculiar shape at the beginning of the decay curve.

Meinke talked about his bombardment of gold with 40 Mev helium ions. He separated gold, thallium, and mercury and found, in the thallium fraction, periods of 2 hours, 7.5 hours, and 27 hours. Five days after the bombardment he milked mercury from the thallium and found a trace of what probably was Hg^{197}. Perlman commented that he heard that Don Orth, another new but as yet uncleared graduate student working in Gilman Hall, found a new mercury isotope also, but Templeton said that it is not a long-lived mercury. Meinke reported that the 2-hour thallium has soft electrons, but he did not separate the mercury early enough to see the 43-minute mercury; he cannot estimate the yields.

Wolfe said that he is going to study the yields of osmium, iridium, and platinum as spallation products of uranium plus 200 Mev deuterons; he described the chemical separations he plans to use.

Chubbuck reported a new 36-hour cerium activity, probably Ce^{137}, found in a bombardment of lanthanum with 40 Mev deuterons on the 184-inch cyclotron; the aluminum absorption curve showed conversion electrons of about 200 kev while the lead absorption curve showed a gamma ray of about 0.2 Mev.

James talked about his bombardment of Pu^{239} with 40 Mev helium ions. The solution containing the curium and americium transmutation products as put through a Nalcite column with citrate elution. The curium from that elution was then run through Nalcite with fluosilicate elution to separate it from rare earth elements. He has thus established the 55-day Geiger activity as curium, probably Cm^{241} (x-rays, electrons, no gamma ray). The americium plus element 61 fraction from the citrate elution was also put through a Nalcite column with fluosilicate elution to separate the americium from element 61. He did not find the 50-hour activity found in similar bombardments. I added this means that Am^{240} has a long half-life and is an orbital electron capture isotope. James said that Cm^{239} also must have a very short half-life.

* * * * *

After the meeting I gave the Chem 123 lecture and then went to my office on the hill.

I dictated a note to Kenneth Priestley to give him the names of the Berkeley people who will want to attend the American Chemical Society meeting in Chicago on April 19 to 24, 1948, in order to present papers at our symposium on the "Chemistry of the Transuranium Elements": L. Brewer, M. Calvin, R. E. Connick, B. B. Cunningham, L. B. Magnusson, E. F. Orlemann, I. Perlman, C. H. Prescott, G. T. Seaborg, and S. G. Thompson. In addition, I gave Priestley a tentative list of those who may attend the Brookhaven Information Meeting on April 26, 27, and 28: M. Calvin, R. E. Connick, B. B. Cunningham, G. T. Seaborg, and S. G. Thompson.

A prompt reply from George Boyd arrived in today's mail, who said he is sending me the photostat copy of the Takvorian article, with a more correct listing for the names in my reference 17. He said that Mlle. Perey used the name "francium" with symbol "Fa" in her articles. However, I shall use the symbol "Fr" since this is what Mlle. Perey suggested in her recent letter to me.

A December 8 letter arrived from Edgar Westrum, congratulating me on the Nichols Medal Award, saying that Hoylande Young expects confirmation on his appointment as Responsible Reviewer, and noting that he can spend eleven days in Berkeley during the holidays--he is feeling especially ambitious! I promptly dictated a telegraphic reply: "VERY MUCH HOPED YOU COULD COME AND DELIGHTED TO HEAR YOU PLAN DECEMBER 26 ARRIVAL."

Two copies of the July-October 1947 issue of the Record of Chemical Progress arrived from Neil Gordon. This contains "Chemistry of Transuranium Elements," the write-up of the talk that I gave at Wayne University in March. Gordon apologized for the delay in publication and said the issue will have a circulation of 5,000 copies.

Another note arrived from W. Y. Chang at Princeton, asking for our decision about the admission of T. C. Chu and Chien-Chang Woo to the University of California.

Friday, December 12, 1947

I made my usual rounds of the labs to check on the research. In today's mail I received a response from Truman Kohman to my Sigma Xi manuscript. Kohman said it seems factually accurate and that his comments, of which there were several, were of only a minor nature and aimed at perfectionism.

I try to read--reports and journals--daily; it is a rather awesome task to keep up with the literature.

Saturday, December 13, 1947

The first thing I did this morning at the office was to handle some of my correspondence:

On November 6 Neil Gordon wrote to ask names of other men trained

here to develop work in inorganic chemistry and atomic energy at Wayne University. Gordon said they were impressed with Paul O'Connor, whom I recommended last year, and were sorry that he chose the University of Minnesota. In my response today I explained that I delayed answering since it is not yet clear which of our men will complete their doctorate next summer. I said that the group will not be large; however, I can mention one outstanding man--Manfred Lindner--and another capable person who completed his doctorate about five years ago with me and who wants to go into academic work--Dr. Gerhart Friedlander at General Electric Company in Schenectady.

I answered a December 5 letter from L. L. Tully (Educators Association, Inc.), who asked for a 500- to 1000-word article for their encyclopedia about the history and the developers of the atomic bomb (geared to high school students). In my reply I first gave the names, symbols, and valences of elements 95 and 96, as a follow-up of my original letter. I then explained that I do not feel I have time to prepare the article they need, but I have talked with Dr. D. H. Templeton, one of my able colleagues, who is willing to prepare such an article. Finally, I said I agree with him that such articles should be accurate and that authorities in the field should take the time to prepare them.

A master's student, Carl F. Miller, asked me to write in his behalf to Professor Frank H. Spedding at Iowa State College, where Miller wants to continue his graduate studies. Today I wrote Spedding, explaining that Miller has been encouraged to apply to Iowa State by Professor Latimer, that his record here is quite good, and that he seems to have a nice personality.

As a follow-up to my wire, I wrote a note to Ed Westrum to tell him that we are happy that he will be able to spend some time with us over the holiday, but I will be away on trips to Los Angeles and Chicago. This should not affect his program, I said, and he should contact Eyring or Cunningham about any preliminary work that should be done before his visit.

Sunday, December 14, 1947

Today I worked on my Sigma Xi lecture a bit, incorporating the changes and new information my friends have sent me.

The kids, Peter in particular, provided me with some entertainment.

Monday, December 15, 1947

I recently received two manuscripts entitled "Exchange Reaction between Thallium(I) and Thallium(III) Ions in Perchloric and Nitric Acid Solutions" by Prestwood and Wahl and "The Exchange Reaction between Valence States of Thallium in Perchloric Acid Solutions" by Harbottle and Dodson from Arthur Lamb, who asked for my opinion about their merits and suitability for publication as "Communications to the Editor" in Journal of the American Chemical Society. Today I wrote Lamb, said that I have read the manuscripts carefully and consider them suitable for publication without change.

Today's mail brought a thank-you note from Harper W. Frantz (Pasadena City Schools) for my sending him the symbol (Fr) for francium. Frantz also mentioned that he will correct the symbol for francium in another edition of his freshman chemistry laboratory manual, which he and a co-author are preparing, but he will not use my version of the periodic table after all because their manual will accompany Pauling's General Chemistry; therefore they will use Pauling's form of the periodic table.

A lengthy letter arrived on December 8 from W. H Sullivan, updating the work of the Committee on Nuclear Constants and suggesting that each individual compiler circulate his data/table to the other members of the committee. Sullivan also asked for preferences for the date of the next meeting in Washington. I thought that I would talk to Sullivan about the points he made in his letter when he visited here, but I have learned that he cancelled his trip. So today I wrote and told him that the only date he listed for the next meeting that is acceptable to me is February 9 and even that my conflict with Dr. L. F. Curtiss' meeting. I also asked that, since he has a photostatic copy of our "Table," he arrange to have it circulated to the other members if he thinks it is necessary.

I received a nice letter, dated December 9, from Charlie Price, thanking me for my Nieuwland Lectures and saying they were excellent, profitable, and enjoyable to all. Price added he may see me at the AAAS meeting in Chicago and that they will always be glad to see me at Notre Dame.

Also, a December 8 letter arrived from Ralph E. Himstead (General Secretary of the American Association of University Professors), noting that my nomination for membership has been received and presented to the Committee on Admission of Members. Upon approval my membership will become effective as of January 1, 1948.

Tuesday, December 16, 1947

I took care of some of my correspondence and administrative matters before going to campus to give the Chem 123 lecture.

On December 3 Donald McPherson (John Wiley) wrote me about the book Joe Katz and Clark Hindman are planning to write entitled Comparative Inorganic Chemistry of Period VII Elements. McPherson sent me their outline and their statement of objectives and asked for my personal opinion of their qualifications and for suggestions about the book. I replied today that I believe they are good prospects for the authorship of such a book. I made a few rather trivial comments about their outline and then said that I shall be in Chicago at the end of the month when I am scheduled to give the AAAS Sigma Xi lecture and would like to talk with him further about this matter.

A note went to Dr. Hyp J. Dauben (Bill Dauben's brother) at the University of Washington in Seattle, to tell him that Manfred Lindner, one of our better graduate students, will finish his doctorate next summer and would be a good bet for a staff position in the Department of Chemistry there.

Another note went to Wayne Wilmarth (University of Southern

California), who recently called me to say that he was unable to obtain Rose Bowl tickets for me legitimately and that scalpers' tickets are going for as much as $15 a piece. I told Wayne that I doubt I want to go as high as $15, but I hope he will keep trying. I also gave him my parents' phone number, explaining that we shall be spending the holidays in the Los Angeles area beginning next Monday.

I answered the lengthy December 10 letter from Bob Campbell about the periodic chart he wants to publish. I pointed out that as scientists we do not consider the chart representative of periodic tables as a whole and perhaps he should include a picture of a representative chart such as the 1947 Welch version. I made a few other corrections and suggestions and then said I shall be in Chicago at the end of the month but not in New York until around the 10th of February.

In the late afternoon I went down to campus for the joint meeting of the Connick-Seaborg groups and after dinner went to the Research Conference.

Wednesday, December 17, 1947

As usual I dictated some letters when I first came into the office. I wrote to Sigfred Peterson at the University of Louisville, enclosing a copy of my Sigma Xi lecture and asking him to look over the references. In particular, I asked him whether I should refer also to Professor Perlman in regard to the matter of the production of actinium from radium, explaining that I believe I first heard of this idea from him.

I then wrote to Joe Katz to give him a couple of corrections to paper 22.3 ("The Neptunium (4n + 1) Radioactive Family") of Volume 14B of the PPR. I also made a slight change in Chapter 1 (Introduction) of Volume 14A.

In response to a December 12 letter from Henry Allen Moe of the Guggenheim Foundation, I wrote a letter of recommendation for Robert E. Connick. After covering points such as I made in a recent letter for promotion that I wrote for him, I said, "Dr. Connick has a fine personality and presence. He is a real scientific scholar, very careful in all of his work. He is a good teacher and has been particularly successful in the training of graduate students, turning out a number of well-trained Ph.D. men in the field of inorganic chemistry. Since Dr. Connick has done all of his work here at the University of California, I think that he would profit greatly if given the opportunity to pursue his investigations at some other centers of research in inorganic chemistry."

Later in the afternoon I heard Emilio Segrè and Bob Leininger talk about their recent work with astatine at the regular weekly research seminar in Le Conte Hall. Segrè described the production and properties of 0.4-second At^{212}, via the reaction $Bi^{209}(\alpha,n)At^{212}$, and 8.3-hour At^{210}, via the reaction $Bi^{209}(\alpha,3n)At^{210}$. Leininger described his tracer work on the chemical properties of astatine. He found that astatine is not as metallic-like as originally surmised but is more halogen-like. He postulates several, possibly three, higher than zero oxidation states.

Thursday, December 18, 1947

Present at this morning's meeting were Asprey, Ballou, Barton, Conway, Eyring, Garden, Glenn, Hicks, Hopkins, Hyde (who is visiting from Argonne), James, La Chapelle, Lindner, Meinke, Miller, Moore, Neumann, Newton, Perlman, Robinson, Seaborg, Street, Templeton, Thompson, Voyer, Werner, and Wolfe. La Chapelle reported on some attempts to prepare $NpCl_5$ from pentavalent neptunium compounds. The attempts have been rather unsuccessful although he has made what is probably $NpOCl_3 \cdot xH_2O$ and $NpO_2Cl \cdot xH_2O$ and, in one early attempt, produced a red sublimate whose x-ray diffraction pattern looked like $NpCl_5$, which has not been duplicated. La Chapelle said that he is making $CbCl_5$, TaO_2Cl, and $TaOCl_3$ for comparison with neptunium compounds.

Templeton talked about his experiment on the reaction Bi^{209} + deuterons to produce At^{211} in order to determine whether the reaction is due to secondary alpha particles or to negative mesotron emission. If its production is due to secondary alpha particles, the cross section should be a function of the thickness of the bismuth target. Astatine was separated with tellurium carrier, and polonium was plated out on silver. Clean astatine fractions free of polonium have been obtained and, if enough deflected deuteron beam can be obtained at the 184-inch cyclotron, they will pursue the experiment.

Ballou talked about his proposed experiments on the possibility of element 61 occurring in nature. He thinks 61^{145} may be long-lived. He talked about his experiments that prove the natural alpha particles ascribed to samarium are actually due to samarium and not element 61. He also reported on the procedure that he and Wolfe are developing to isolate the platinum metals from uranium bombarded with 400 Mev helium ions. At this point I mentioned the reported β^- activity in natural neodymium, and I reported that Takvorian has also proved that the alpha activity ascribed to samarium is actually due to samarium and not element 61. Takvorian identified some "alpha particles" in the 61 fraction as very soft activities. I described the early work of Libby, who reported very soft beta activity in neodymium with no apparent hard beta particles from an element 61 daughter.

Hyde discussed a proposed experiment to make new isotopes of protactinium and actinium by bombardment of ionium with deuterons. He also said that Studier plans to look for U^{240} by irradiation of U^{238} in the Hanford pile.

James, Perlman, and I talked about the possible production of Th^{231} from a deuteron bombardment of Pu^{239}. Perlman said that, if a metastable excited state exists, the alpha half-life might be shortened to the point where it would compete with gamma-emission--citing as an example the decay of ThC'.

Newton told of his plans for the further study of the fission of thorium with 40 Mev helium ions, mentioning the isotopes he plans to examine, and saying that he plans to do the chemistry as rapidly as possible.

Eyring reported on his results in reducing rare earth fluorides with

lithium--with NdF_3 he obtained a pellet of metal, which is grey, malleable, and softer than barium metal. He obtained no reduction to metal with EuF_3 and no pellet with samarium. I mentioned, at this point, that Westrum will be here from December 26 to January 5 to help with metal production. I also said that if americium cannot be reduced to metal, it will be evidence for a +2 state.

Finally, Hicks reported on his bombardment of lutecium with 40 Mev helium ions--he found a 7.5-hour tantalum activity, decaying by K capture--probably Ta^{176}--with a 2-3 Mev gamma ray, in addition to two conversion electrons. This experiment was performed to aid in the identification of the hafnium and tantalum isotopes formed in the spallation reactions of tantalum. I then announced that the next meeting will be on Tuesday, December 30.

* * * * *

After the meeting I gave the Chem 123 lecture. I then made a brief stop at my office on the hill before going home for lunch with Helen and a honeymooning couple, Ed M. "Monk" and Margaret Mulligan. Margaret is the former Margaret Smith, attractive sister of Edrey Smith Albaugh, my former secretary at the Met Lab. Helen and I often saw Margaret at her parents' home in Chicago and once attended a Chicago Bears-Green Bay Packers football game with her during our wartime stay in Chicago.

Later, after a routine chest x-ray by Dr. E. Schulze Heald (2560 Bancroft), I went back to my office and wrote to George Boyd to return his photostat of the Takvorian article. I said that I found the article interesting although I have not studied it enough to determine whether Takvorian would have been able to detect Libby's reported low-energy neodymium beta particles. I commented that the presence of actinium and its daughters reduces his sensitivity in his search for possible radioactivity in element 61. Finally, I mentioned the work of Nate Ballou, who is interested in possible radioactive isotopes of element 61 (and technetium) that might exist in nature. Some of his ideas, I said, appear in the notes of our group meeting of October 9, of which Ray Stoughton has a copy.

W. A. Hamor (Editor of the ACS Chemical Monographs) wrote on December 9 to ask me to prepare a treatise on transuranium elements for the series. In my reply today I explained that it would be unrealistic of me to take on the job of writing a book on the transuranium elements because of my heavy schedule. I then pointed out that my colleagues are critical of the high cost of the Monographs and that the sale price per page is considerably higher than works published by independent concerns such as John Wiley.

A congratulatory note arrived today from A. N. Stevens (Editor of Research Today) about my being awarded the Nichols Medal for 1948.

Late in the afternoon Larry Magnusson came in and told me that he passed his prelims today. His committee was composed of George E. Gibson, Robert E. Connick, T. Dale Stewart, Earl R. Parker, Derrick H. Lehmer, and Bruno H. Zimm.

Friday, December 19, 1947

At the office this morning I took care of more pending correspondence:

On December 15 Professor Arthur Lamb asked for my comments on a manuscript called "Electronic Exchange between Ferric and Ferrous Ions in Perchloric Acid Using a Diffusion Separation Method" by Van Alten and Rice. I read the paper and today wrote to Professor Lamb, saying that I find the results reporting a rate of exchange slow enough to be measurable, astonishing and difficult to believe. However, I can not find anything wrong from the description of their experimental technique and there seems to be no reason why the paper should not be published. In fact, the paper may stimulate investigation in this field.

I wrote to Professor W. Y. Chang (Princeton University) to tell him that letters have been sent to both T. C. Chu and Chien-Chang Woo to tell them that we shall be glad to have them come work with us. I noted that copies of these letters were sent to him and that he probably has received them by now.

A letter went to Harry Althouse to say that we have looked over the book entitled Physikalische Chemie in Medizin and Biologie by W. Bladergroen, which Althouse sent us. We believe, I said, that the book is better than any other book in English covering this area and that it is worth translating into English.

Today's mail brought a December 18 memorandum from M. A. Stewart (Acting Dean of the Graduate Division), stating that G. T. Seaborg, W. M. Latimer, and E. M. McMillan have been appointed as the thesis committee for Stanley Gerald Thompson.

A note arrived from Joseph E. Mayer, notifying me that I have been appointed an Associate Editor for the Journal of Chemical Physics for the term beginning January 1, 1948 and ending on December 31, 1950.

I then made the rounds of the labs to make sure that the graduate students had formulated plans for their research during the vacation period--the Christmas holiday for the University begins on Monday.

I left the lab earlier than usual to make certain that Helen had everything ready for our trip to Los Angeles, where we plan to spend the holidays. Herman Robinson drove us to the railroad station. The children, Pete at least, were intrigued by the new experience of train travel in a bedroom and behaved quite well, and Helen made an attractive picture in her new red suit. The Oakland Lark left at 8:43 p.m.

Saturday, December 20, 1947

Helen, Pete, Lynne, and I arrived at the Los Angeles railroad station about 9 a.m. Jeanette and Eino met us and drove us to South Gate. My parents were pleased to meet Lynne, their three and one-half month old granddaughter. Pete seemed happy to be here although, of course, he didn't remember his first visit here at Christmas last year.

Sunday, December 21, 1947

In South Gate. Clayton Sheldon and I went to Rio Hondo for a game of golf: CES-115, GTS-114. [I keep an ample supply of old golf clubs here in South Gate to fill my needs.]

My mother prepared her usual splendid Sunday afternoon dinner for Jeanette, Eino, and the rest of us.

Monday, December 22, 1947

In South Gate. Helen and I left the children with their grandparents and went Christmas shopping for family gifts, especially to provide Santa Claus with presents for Pete and Lynne.

Tuesday, December 23, 1947

In South Gate. Helen and I again went Christmas shopping.

Wednesday, December 24, 1947

In South Gate. I played golf this morning with Wayne Bartholomew at Rio Hondo (WB-113, GTS-100). [Wayne, who works for the Department of Parks in South Gate, is the husband of an old friend of Helen's, Violet Johnson--Violet was a secretary at Santa Ana Junior College, while Helen was a student there (1935-37).]

Saul and Sylvia Winstein stopped over to visit with us, and later my cousin Elmer Johnson and his wife Lillian came by to see us.

Mother prepared the usual Swedish Christmas Eve supper of saffron bread, lutfisk, sill, rice pudding, etc.; then we had our regular exchange of family gifts.

Thursday, December 25, 1947 (Christmas Day)

In South Gate. Mother prepared a traditional dinner of turkey and its trimmings. Jeanette and Eino, along with Bob and Ruth Engstrom, joined us for dinner. Lynne was so frightened by Ruth's shrill voice and laughter that we had to take turns taking her out to the backyard to get beyond the range of Ruth's voice.

Friday, December 26, 1947

In South Gate. Wayne Bartholomew, Clayton Sheldon, and I went out for a round of golf at Rio Hondo this morning: WB-108, CS-113, GTS-99. Clayton, Rita, and their two sons, Ronnie and Gayle, spent some time with us after the game.

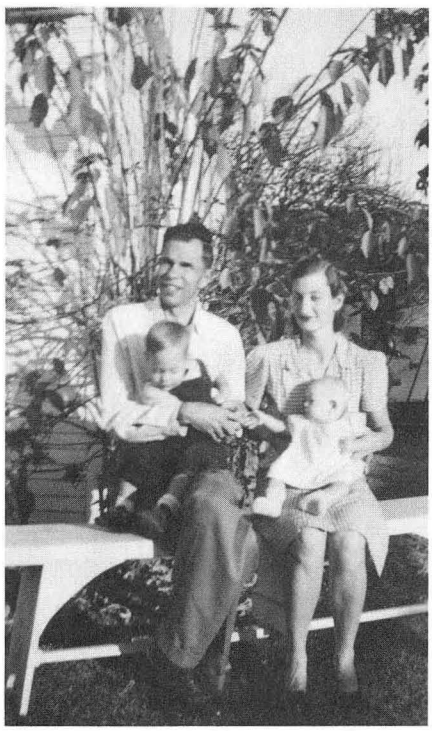
I put in a call to my office in Berkeley to check on my mail, and Mary told me that I received a letter from our new graduate student, Jack Hollander, who would like to meet me while I am in Chicago. I told her to send him my schedule.

Later Nathalie and Harlan Baumbach and young Robert stopped by to

visit. Helen and I had dinner with Jeanette and Eino at their trailer home.

Saturday, December 27, 1947

In South Gate.



Glenn with Pete,
Helen with Lynne in the
backyard of 9237 San
Antonio Avenue, South Gate,
December 1947

Selma Seaborg holding Lynne,
Ted Seaborg holding Peter,
South Gate, December 1947.

Sunday, December 28, 1947

I left Los Angeles at 9 a.m. via TWA Flight 94 and arrived in Chicago at about 5 p.m. There I took a taxi from the airport to the Hotel Windermere, where I checked in and then had dinner.

[In South Gate, several people stopped by to visit with Helen and see the children: Georgia and Allyne Snyder (sisters who were childhood friends of Helen's in Santa Ana), Mary Klever and her daughter Barbara (Mary's husband, Floyd is a cousin of Helen's), Peggy and George Kaspar (Peggy is a daughter of Mary Klever and a sister of Barbara), and Clarence and Libby (Seaborg) Jaedecki (Libby is my father's first cousin).]

Monday, December 29, 1947

In Chicago. I took a cab to the New Chem Building on the University of Chicago campus for a special meeting of the GAC at 9:55 a.m., attended by all of the GAC members except Rowe; that is, Robert Oppenheimer, James Conant, Lee DuBridge, Enrico Fermi, Cyril Smith, Hood Worthington, Isidor Rabi, and I, in addition to Secretary John Manley. Oppenheimer suggested five items for the agenda: (1) a trip through Argonne National Laboratory, which we tentatively decided to forego because of time considerations; (2) the problem of Clinton Laboratories; (3) the Los Alamos weapons test program; (4) a report from the Subcommittee on Material Flow; (5) the report of the GAC through the Chairman to President Truman. Item (3), we decided will be considered in Executive Session from 8:30 to 10 a.m. tomorrow, to be followed by a discussion on the same topic with the members of the Military Liaison Committee and the Atomic Energy Commission. Worthington said that his Subcommittee on Material Flow (agenda 4) has not found anything urgent to report so the matter can be held over.

At 10:05 a.m. the GAC was joined by Commissioner Robert Bacher and Director of Research James Fisk. Bacher told us that, since a number of candidates have refused to take the position of Director of Clinton Laboratories, the Commission has decided to remove reactor development from there. DuBridge asked what would be left at Clinton, and Bacher replied, "both stable and radioactive isotope production, the Oak Ridge Institute, a strong chemical development based on facilities that exist there and nowhere else, and biological research." Fermi expressed some concern about this move, indicating that some of the people engaged in reactor work at Clinton Laboratories might not want to move to the Argonne Laboratory. We agreed that, among the key people, the great unknown as to whether they would be willing to transfer, would be Alvin Weinberg and Gale Young. The consensus was that there was a lack of unity of direction of effort at Clinton and that, in spite of urgings by the AEC, the laboratory has actually avoided taking responsibility for a vigorous program of reactor development. We concluded that, even though the proposal to centralize at Argonne may lead to loss of men, it appears that we may be left with an inadequate reactor program if the transfer to Argonne is not made.

At 10:55 a.m. we were joined by Walter Zinn, Director of Argonne National Laboratory, and William B. Harrell, Business Manager of the University of Chicago that operates the Argonne Laboratory under

contract. Zinn had some doubts about the feasibility of transferring the reactor work from Clinton to Argonne, including worries about transferring the work from a place with strong capabilities to one with relatively weaker capabilities. Oppenheimer replied that it is not useful to compare what can be done at Argonne with what one hoped for at Clinton. Zinn also pointed out that Eugene Wigner is strongly opposed to the move. Since many of the key people at Clinton have a great deal of confidence in him, it could be difficult to secure their support. Zinn said he is also worried about the effect the influx of a sizeable number of people from Clinton will have on the spirit at Argonne, but DuBridge stated the personnel at Argonne would be stimulated by the news that Argonne was to have the primary responsibility for the development of reactors; this will help personnel from Clinton. Rabi stressed the point that the permanent long-range aspects of the proposal should be an added attraction to personnel. Zinn also stated that, in his opinion, the DuPage site would be adequate for development work but not for pilot plan construction.

Harrell, in order to clarify his impression of the discussion, gave the following summary: (1) the AEC and GAC concur that reactor development should be largely centralized; (2) Clinton Laboratories is not a realistic location for the centralization; (3) Argonne Laboratory is the most desirable place. Zinn said he agrees with (1), but he still has grave doubts about (2) and (3). Fermi said he shares the same doubts although he is disappointed that Clinton Laboratories has not developed as had been hoped. Since the AEC has already made the decision about the move, the GAC took no formal action.

We then approved Oppenheimer's draft of the report on atomic energy activities for 1947 to President Truman with the addition of a couple of editorial changes and adjourned at 1:15 p.m.

Later I met with Katz and Hindman to discuss the status of the symposium papers--we set approximately January 5 as a deadline for the abstracts. They also talked about the book they are planning to write, Comparative Inorganic Chemistry of Period VII Elements, and we discussed their outline--they said they plan to dedicate it to me. Joe told me that he will write Chapter XIX of Volume 14A soon.

Clyde A. Hutchison (University of Chicago's Institute for Nuclear Studies) spoke with me at length about his magnetic susceptibility work. To my satisfaction he has found that crystalline compounds of Nd(III) at low temperatures behave very much like U(IV) compounds, indicating analogous f^2 structure, in agreement with my actinide concept.

Darrell Osborne and I discussed, among other things, personnel matters. I told him that George Manov, an old UC friend, is interested in changing fields and might be a good candidate for Argonne.

I also saw our new graduate student Jack Hollander and told him about our setup. He has an offer from the University of Chicago for $1,500 (minus $500 tuition). Hollander said he wants his chemist-fiancée to work and I said we can arrange something. He will write to me if he is not cleared by February 15.

Tony Turkevich (Institute for Nuclear Studies) told me that he has

devised a method for measuring the power level of the Soviet plutonium production-reactor complex by measuring the level of 10-year Kr^{85} in the atmosphere. The Soviet production of Kr^{85} released to the atmosphere will add to that produced by the U. S. He has estimated the U. S. production as follows--three times the level produced at our Hanford Works will give 40 disintegrations per minute of Kr^{85} per liter of atmospheric krypton per year. To monitor production we can periodically measure the Kr^{85} content of the krypton produced by the Linde Company.

Cyril Smith gave a cocktail party for members of the GAC from 5 until 7 p.m. at his home (5735 Kenwood).

Tuesday, December 30, 1947

In Chicago. I again took a taxi to the University of Chicago campus and met at 8:50 a.m. with the same members of the GAC as yesterday, in addition to Dr. Norris E. Bradbury (Director of the Los Alamos Laboratory), who was present to present the plan of shots for the test of nuclear weapons, the Sandstone series, scheduled for the Eniwetok Atoll in the Pacific next spring. At the beginning of the discussion, I raised the additional question about continuous monotoring of the accumulation of atmospheric radioactivity, particularly radiokrypton gas, resulting from pile operation, as a method of measuring the level of plutonium production; we agreed to make this an item for discussion at our next meeting. After Bradbury's presentation, DuBridge moved that we support the Los Alamos plan for nuclear weapons testing; the motion was seconded by Conant and carried unanimously. This motion signifies our approval of the types of weapons and order of their testing but leaves open some details of the design and the precise criterions for determining whether a weapon is good or bad. This executive session adjourned at 10:05 a.m.

We were joined at 10:15 a.m. by General Lewis H. Brereton (Chairman of the Military Liaison Committee), Rear Admiral William S. Parsons, Rear Admiral Thorvald Solberg, Commissioner Bacher, General James McCormack (Director of Military Application), Captain James S. Russell (McCormack's deputy). Oppenheimer summarized the previous discussion and said that the GAC approved the Los Alamos plan for nuclear weapons tests. There was then a discussion of the laboratory techniques used for the investigation of the assembly behavior of the nuclear weapons and of the measurements to be made in the tests. We talked about the possibility of subsequent operations at Eniwetok Atoll and whether there should be two or three tests in the Sandstone series, on the basis that the GAC had presented evidence of the need for three shots. Finally, it was agreed that the test plan would be considered for final action by the Research and Development Board, the Military Liaison Committee, and the AEC after it has been carefully outlined for consideration by these three bodies. We adjourned at 11:30 a.m.

At noon I recorded a 13-minute synopsis of this evening's talk at the NBC radio station here in Chicago.

I had dinner at the College Inn in the Hotel Sherman with Carl Anderson, Harlow Shapley, George A. Baitsell, Forrest R. Moulton, and George B. Pegram. (I also saw John A. Madigan of St. Thomas College, who sent his greetings to Ed Orlemann.)

Then at 8:15 p.m. I delivered my Sigma Xi lecture, "The Eight New Synthetic Elements," in the Ball Room of the Hotel Sherman. I began by saying:

> The history of the discovery of the chemical elements is a fascinating story which begins with prehistoric times and extends through the ages to the present time. These building blocks of nature, which now total ninety-six in number, have been patiently sought out, one by one, in careful researches carried out by investigators in many lands. In the great majority of the cases recorded history has passed on to us the name of the discoverers and the circumstances under which the discoveries were made.

I concluded with:

> Any future elements which might be discovered must have atomic numbers greater than 96, that is, must lie in the "transcurium" region. It seems quite likely that it will be possible to produce and identify new elements in this region, but the major difficulty here is one of starting materials. It seems likely that the most stable, that is, the longest-lived isotopes of the elements with atomic numbers 97 and 98 will be those with mass numbers perhaps as high as 247 and 248 and higher. Thus, it can be seen that the problem here is one of starting materials since the heaviest isotope now known is the Cm^{242}. The rather detailed understanding of the transition series which exists in this region makes it possible to make some good guesses about the chemical properties of such new elements, a fact which will be of considerable help in such a program.
>
> The talk was very well received.

Hoylande Young came up to me after the meeting, and I promised to call Harold Fidler about Westrum, Orlemann, and Tom Jones being responsible reviewers. She will also call Stoughton about being a responsible reviewer for the "Table of Isotopes" and the Chart of Isotopes.

[In Berkeley, Perlman conducted the group meeting, which was attended by Ballou, Barton, Conway, Covey, Cunningham, Goeckermann, Hopkins, Howland, James, La Chapelle, Lindner, Magnusson, Miller, Neumann, Newton, Perlman, Street, Templeton, Thompson, Werner, Westrum, and Wilkinson.

Cunningham reported on some of the results he and Ghiorso obtained from their fission measurements using the heavy water pile in Chicago. Their experiments were hindered because they were unable to obtain a very low background of fission counts. Clinton curium (Cm^{242}) has a fission cross section of about 60 barns as previously found. Curium from 51NB (14-months bombardment at Hanford of Am^{241}) has about the same cross section as Clinton curium. The alpha to fission count ratio for americium samples is for pure Am^{241}, 10; americium from 51NA, 6; americium from 51NB, 4. The ratio for 51NA is higher than found before (pure Am^{241} has a slow neutron fission cross section of about 2.8 barns). The column runs on the neutron-bombarded curium from 51NB for finding elements above curium did separate some fission activity from alpha activity, but the fission background was too high to get good

measurements and the column runs were not the best. The americium and curium that James separated from a bombardment of Pu^{239} with 40 Mev helium ions were put through a column and the fission activity was eluted where plutonium should have come out. The results on 51NB curium do not seem to fit with Werner's determination of its specific activity. Perlman interjected to point out that James' more recent value of the half-life of Cm^{242} (162 days by direct decay) brings Werner's value closer toward indicating pure Cm^{242}, but Werner said the specific activity is still 15% too low.

Goeckermann reported that he has been isolating fission products from a long bombardment of bismuth with 200 Mev deuterons. He found 4-day positron-emitting I^{124} and 19-day negative beta-emitting Rb^{86} (in this case he also saw long-live positrons, possibly indicating branching of Rb^{86}). In the tellurium fraction he found a hard positron that may be the 3-minute positron-emitting antimony daughter of 6-day Te^{118} (K capture). Te^{121} (17-day, K capture) was also probably present.

Westrum described their determinations of the heat of disintegration of Cm^{242} calorimetrically. For 340 micrograms (weighed by Werner) the grand average value is 4.65×10^{-2} cal/min, and this should be good to $\pm$ 0.5%. This leads to a value of 151 days for the half-life using a value of 6.18 Mev for the distintegration energy. There followed a discussion of why Westrum found a higher specific activity calorimetrically than Werner did by alpha counting. Westrum also said that he and Eyring are now preparing to make pure americium metal.

Wilkinson told about his bombardment of holmium with 40 Mev helium ions, in which he found two thulium activities: (1) an 8.4-hour activity that probably is Tm^{166} or Tm^{168} with 100 kev e^-, K and L x-rays, 1.8 Mev gamma ray, positrons with mean energy of 0.6 Mev and maximum energy of 2.1 Mev, negative beta particles of apparent range about 4.6 Mev--since the latter is unreasonable, he believes this is two negative beta particles. (2) He found a long-lived (about 9 days) activity tentatively called Tm^{167} with conversion electrons of about 100 kev. There was a discussion about the assignment of the 8.4-hour activity in which Ballou suggested that the β^+ and β^- activities were two different isotopes, but Wilkinson said that it is not necessary to make that assumption and that the isotope could be 94% K, about 2% β^+ and about 4% β^-.

Templeton then mentioned an interesting energy level calculation that can be made for Bi^{207}, assuming that the At^{211} radiations have been correctly interpreted. At^{211} decays to Bi^{207} by emitting a 5.94-Mev alpha particle and also decays to Po^{211} by orbital electron capture. Let x be the energy change for the latter decay. Po^{211} decays to Pb^{207} by the emission of a 7.43 Mev alpha particle. Then, by closing the cycle, Bi^{207} is unstable with respect to Pb^{207} by (1.49 + x) Mev. This is a sizeable energy difference but they have never observed any activity for Bi^{207} and have experimental evidence that its half-life is very long.

Lindner reported on the results of a bombardment of antimony with 40 Mev deuterons, the basis for the mass assignments of the tellurium activities mentioned by Goeckermann. The two tellurium activities in question are 4.5-day and 6.0-day activities, both K capture, produced from Sb^{121}.

With 200 Mev deuterons the 6.0-day was always found in much higher yield relative to the 4.5-day activity, but in the 40 Mev deuteron bombardment the 4.5-day activity was produced in higher yield than the 6.0-day activity. Therefore the 4.5-day tellurium and its antimony daughter (K, 40 hour) have been assigned to mass 119, and the 6.0-day tellurium and its antimony daughter (β^+, 3.3-minute) to mass 118. Lindner also said that 200 Mev bombardments of antimony produced Sb^{124}, from Sb^{123}, and it will be interesting to get the yield for this. He also plans to get the relative yields for $Ba^{138}(p,2\alpha)I^{131}$ and $Ba^{138}(n,2\alpha)Te^{131}$ in a 200 Mev deuteron bombardment where the deuteron is known to act like separate proton and neutron. Perlman said that it will be necessary to run two experiments since two factors are operative: 1. Do proton and neutron stick with the same yield and 2. Does emission of multiples of alphas predominate over the emission of the equivalent number of protons. Thus it will also be necessary to determine the yields of I^{131} and Te^{131} with La^{139} as the target. Perlman said that the people working on high energy bombardments should study the paper by Serber and the one by McMillan on the multiple particle ejection reactions.

Ballou talked about his 25 μah bombardment of yttrium with 20 Mev deuterons: he found a large amount of Zr^{89} (β^+, 80 hours), and the decay curve has now tailed into a long period. Because of theoretical considerations he looked for alpha activity in Zr^{88}, but that which was found was probably due to an isotope of thorium formed from impurities in the yttrium. An absorption curve of the long-lived activity indicated a beta energy of about 0.6 Mev. Ballou plans a longer bombardment; he said that his long-lived activity is not necessarily Zr^{88} but may be produced from impurities in the yttrium target.]

Wednesday, December 31, 1947

In Chicago. Donald McPherson (John Wiley) and I had breakfast together and discussed the proposed Katz-Hindman book on Comparative Inorganic Chemistry of Period VII Elements that John Wiley Co. is considering publishing. I again gave them a good recommendation.

Earl Hyde came by to see me at the hotel and told me that he found about 10^6 c/m of Po^{210} in his ionium plus 20 Mev proton bombardment. He saw x-rays, which are due to Pa^{229} or Pa^{232}. In the actinium fraction he found only Ac^{225} and no evidence for a Ac^{229} or Ac^{230} (two days after the bombardment). The energy of the Pa^{229} alpha particles = 5.68 Mev (4.24 cm range in air) instead of previous 5.4 Mev (by pulse analysis).

Later I took a taxi from my hotel to the Chicago Airport and caught a special TWA Constellation at 7:30 p.m. to Los Angeles, where I arrived about midnight. There I was met by Ed Westrum who had flown down to Los Angeles from Berkeley in order to attend the Rose Bowl game. Westrum provided me with a ticket to the game.

NAME INDEX
(Underlined entry indicates appearance in photograph)